

P. 455.

A
JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,
AND THE ARTS:

ILLUSTRATED WITH ENGRAVINGS.

BY WILLIAM NICHOLSON.

V O L. III.

L O N D O N:

PRINTED FOR THE AUTHOR, BY W. STRATFORD, CROWN-COURT, TEMPLE-BAR.
AND SOLD BY G. G. AND J. ROBINSON, PATERNOSTER-ROW.

M.DCCC.

100-231901



NATIONAL ARCHIVES
AND RECORDS ADMINISTRATION

THE NATIONAL ARCHIVES

100-231901

100-231901

100-231901

100-231901

100-231901

TABLE OF CONTENTS

TO THIS THIRD VOLUME.

A P R I L 1799.

ENGRAVINGS of the following Objects: 1. A Pocket Ribband Machine for charging a small coated Phial with Electricity; and, 2. Apparatus for obtaining an invariable Measure from Experiments with a Pendulum.

I. A statical Enquiry into the Source of Nutrition in succulent Vegetables. By Mr. J. Gough. - - - - - page 1

Succulent plants have been supposed to derive much of their nutriment by absorption from the air; but it may be questioned whether they do not merely subsist for a long time without any supply, when surrounded by dry air only. Statical experiments to determine this point. Uniform result, that the succulent vegetables of Europe are nourished by the common vehicle of water, either conveyed to them by the earth, or deposited on their leaves, in the form of rain.

II. Description of a Pocket Ribband Machine for charging a small coated Phial with Electricity. By J. Fell, Ulverstone. - - - - - p. 4

An endless web of varnished ribband is drawn by the hand between two boards faced with mole-skin, and passed in the electrified state in contact with a wire of a coated jar, which, by that means, becomes charged.

III. Of the Corundum-stone from Asia. By the Right Hon. Charles Greville, F. R. S. - - - - - p. 5

Scientific observations on the crystalline forms of corundum. Table of the specific gravities of the corundum, sapphire, topaz, ruby, and diamond, from different authorities.

IV. Experimental Researches concerning the Principle of the lateral Communication of Motion in Fluids, applied to the Explanation of various hydraulic Phenomena. By Cit. J. B. Venturi, Professor of Experimental Philosophy at Modena, Member of the Italian Society, of the Institute of Bologna, the Agrarian Society of Turin, &c. - - - - - p. 13

Hollow whirl formed in a reservoir from which water flows through an horizontal aperture. Theory deduced from the doctrine of central forces. The vertical section of the cavity is convex towards the axis. Other phenomena explained. Lateral communications of motion takes place in the air as well as in water. It is the cause of sound in organ pipes and of the increased power of sound through divergent conical pipes. Remarkable difference between the sonorous vibrations in a pipe, and those which are propagated in the open air. The contraction of the stream issuing through an orifice in a plate, is not the Newtonian cataract. The velocity of the contracted stream, is nearly the same as that of a body falling freely from the surface of the water in the reservoir. Singular form of the contracted stream after issuing through an aperture not of a round figure.

V. On the mechanical Fabrication of Lint, and the Manufacture of Hats. By N. L. - - - - - p. 22

VI. Report and Observations on the Art of Hat-making. W. N. p. 23

Description of the manufacturing processes. Enquiry whether this Art may profitably be executed by machinery. Circumstances necessary to be attended to.

VII. A popular Account of Experiments which have been made or attempted, for the Purpose of obtaining an invariable Measure of Length, from the Difference between the Lengths of the same Pendulum, when adjusted to measure different known Portions of Time. (W. N.) - p. 29

The pendulum is itself an universal measure, determined from the time of vibration. Practical difficulties which probably caused this research to be laid aside. Premium directed to this object by the Society for the Encouragement of Arts. Mr. Hatton's project for measuring the difference of the lengths of the same pendulum adjusted to measure different portions of time. Subsequent improvements and experiments with this apparatus. By Mr. Whitehurst and Dr. George Fordyce.

VIII. On the vibration of the Wings of a Fly. By S. R. - p. 35

Optical phenomenon which proves that the wings of the common fly makes six hundred vibrations in one second.

IX. An Account of several Veins of Sulphate of Strontian, found in the Neighbourhood of Bristol, with an Analysis of the different Varieties. By William Clayfield - - - - - p. 36

Natural history and chemical analysis of sulphates of strontian. Experiments on the deleterious properties of barytes.

X. Letter from Dr. Beddoes respecting the Discovery of Sulphate of Strontian, announced at page 535, Vol. II. of this Journal - - - p. 42

Remarks on the communication of Dr. Gibbes, apparently claiming the discovery that the mineral found near Sodbury, in Gloucestershire, is sulphate of strontian. Other information.

Scientific News, and Account of Books - - - p. 43

Rectification of ether. Difference of tones produced by the gases through the same pipe: Stereotype printing. Account of the new institution for mechanical improvements; its constitution and list of members.

MAY.

M A Y 1799.

Engravings of the following Objects: 1. An Engine, by Rochon, for the speedy and accurate engraving of Metallic Plates: and, 2. The Furnace for converting Bar-iron into Steel by Cementation.

I. On the Philosophical Uses of a common Pocket Watch. By the Rev. William Pearfon. - - - - - p. 49

Utility of the common watch for measuring small portions of time. Method of determining those portions. Numbers proposed for a new watch to measure hours, minutes, seconds, and quarters, without any extra wheel-work. Caution to medical men in the purchase of a stop-watch, on account of an imposition practised in the construction of those instruments.

II. A Letter from Mr. H. Davy, introductory to the Experiments contained in the subsequent Article, and on other Subjects relative to the Progress of Science. - - - - - p. 55

III. Experiments and Observations on the Silex composing the Epidermis or external Bark, and contained in other Parts of certain Vegetables. By Humphry Davy. - - - - - p. 56

Light produced by the collision of two pieces of bonnet cane against each other, or against siliceous stone or steel. This phenomenon is not electric; but arises from silex in the epidermis of the vegetable. A portion of the epidermis examined by calcination, fusion with alkali, solution in water, and precipitation by muriatic acid: nearly two-fifths of silex were obtained. The wood and inner bark of the cane afforded scarcely any silex. Examination of other canes; the English reeds and grasses, the husks of wheat, &c. all which afforded silex. The silex appears to be organically disposed in vegetables. General remarks.

IV. Analytical Table of the Results of the Course of Experiments on the lateral Communication of Motion in Fluids. By Citizen J. B. Venturi, Professor of Experimental Philosophy at Modena, &c. Drawn up by the Author himself. - - - - - p. 59

V. A Memoir on the Typographic Art. By Alexis Rochon, of the French National Institute, and Director of the Marine Observatory at the Port of Brest. - - - - - p. 61

Encomium on the art of printing, with a slight prospect of its origin. Printing by cast plates in relief first practised by William Ged of Edinburgh, in 1744. Observations on this art and that made use of by the Chinese. Invention of Dr. Franklin for multiplying the copies of hand-writing by a mechanical process of engraving. A neater process by the author, by means of counter-proofs.

Description of a machine for stamping letters in a metallic plate, with great speed and precision. It consists of an horizontal wheel which carries all the punches; a screw to make the impression, by acting on the head of the punch; and a carriage to which the metallic plate is attached, and which is moveable between each stroke, according to the spaces required between letters, words, and lines. Report of the French Academy, and account of the expedients by which the necessary precision and dispatch are insured. Stereotype method of printing. Method of taking impressions of coins in bell-metal by means of heat. The ancients used heat in their process of coining.

- VI. On the Processes for manufacturing Hats, the Use of Machines, and other Objects. By N. L. - - - - - p. 73
 Practicability of machines for hat-making. Particular uses of dregs of beer in that art. Application of heat. Construction of the bow. Impositions and dishonesty of the workmen. General remarks.
- VII. Account of a Substance found in a Clay-pit, and of the Effects of the Mere of Difs upon various Substances immersed in it. By Mr. Benjamin Wiseman, of Difs in Norfolk. With an Analysis of the Water of the said Mere, by Charles Hatchett, Esq. F.R.S. in a Letter to the Right Honourable Sir Jos. Banks, Bart. K.B. P.R.S. &c. - - - - - p. 77
 Inflammable mineral. Various substances exposed to the water of Difs Mere. Analysis of the water, and examination of those substances. Formation of Pyrites.
- VIII. Abstract of Experiments and Observations on the internal Use of Phosphorus. By Alphonfus Leroi, Professor at the School of Medicine in Paris. - - - - - p. 85
 Phosphorus is found to stimulate or increase the powers of life and muscular energy. Various facts. Oxide of iron with phosphorus. Intimation of certain remarkable experiments, said to have been made with phosphorus.
- IX. Letter of Enquiry respecting the proper Form of a Boiler for Steam-engines. - - - - - p. 86
 Description of the usual form of a boiler for steam. Objections to the depth of its contents. Remarks on the effects of such depth, and the general production of steam.
- X. Description of the Furnace for converting Bar-iron into Steel. By Mr. Joseph Collier. - - - - - p. 88
- XI. On the Origin of the Areometer. By Citizen Eusebe Salverte. - - - - - p. 89
- XII. Chemical Considerations on the Use of the Oxydes of Iron in the dyeing of Cotton. By J. A. Chaptal. - - - - - p. 92
- Scientific News, and Account of Books. - - - - - p. 93
- Respirability of the gaseous Oxide of Azote.—Erratum from the Rev. Mr. Pearson of Lincoln.—New Produce of European Sugar.—Notice of a Translation of Dr. Gren's elementary Treatise on Chemistry.—Death of Dr. Frederick Albert Charles Gren.—Communication from Dr. Beddoes.

J U N E 1799.

- Engravings of the following Objects: 1. Apparatus for ascertaining a standard of Weight and Measure. 2. Sketches of Barometric and Thermometric First-movers.
- I. Account of some Endeavours to ascertain a Standard of Weight and Measure. By Sir George Shuckburgh Evelyn, Bart. F.R.S. and A.S. - - - - - p. 97
 Short

Short history of the invention and use of the pendulum, with a moveable centre of suspension. Ineffectual endeavour to use Whitehurst's machine. Account of various instruments for determining the weight and measure of determinate quantities of water. Metallic cube and cylinder. Scale of equal parts, with apparatus and method of using it. Hydrostatic balance and weights. Operations for determining the solidities of the cube and cylinder.

II. Letter from Dr. Beddoes, respecting Citizen Fourcroy's Account of the Discoveries of Mayow p. 108

Considerable inaccuracy in Citizen Fourcroy's general estimate of the mental powers of Mayow.

III. Observations and Experiments on various saponaceous Compounds, particularly the Fish Soap of Sir John Dalrymple. By Mr. Robert Jameson, F.L.S. &c. p. 108

Short history of soap. Methods of making the different kinds, white, mottled, yellow, soft. Contents of the waste leys. Soap of whale-oil. Sir John Dalrymple's soap of fish. Observations on the fatty matter into which water converts muscular fibre. Detail of experiments on the soap of fish, made at the request of the board of trustees at Edinburgh. Soaps of wool, of glue, of peat. Neither the muscular fibre, nor wool, nor glue, will unite with tallow and alkali, in the common process for making hard soap. Peat soap gives a brown colour to cloths. Antiseptic power of alkalis.

IV. Experiments and Observations to prove that Snow does not contain Oxygen either in Solution, or in Combination, and that its fertilizing Quality does not depend on this Cause. By Dr. Joachin Carradori de Prato p. 119

Hassenfratz having inferred that snow contains oxygen, the author made experiments with fresh water fish. They died immediately in snow water, which had not been exposed to the atmosphere; though the same water was by a short exposure rendered fit for the respiration of fishes. Snow water does not emit oxygen by solar light, and does not therefore contain that substance in combination.

V. On certain Properties of Strontian and Barytes. By Citizen Vauquelin p. 122

Strontian, as well as barytes, is disposed to combine with silex and with alumine, which they render soluble in water. Barytes combines by double affinity with oil, and forms an insoluble soap: it also combines directly with animal substances in the humid way. From these properties it is inferred that strontian and barytes ought to be classed with the alkalis, and may be used for similar purposes, &c.

VI. Concerning those perpetual Motions which are producible in Machines by the Rise and Fall of the Barometer, or by the Thermometrical Variations in the Dimensions of Bodies. (W. N.) p. 126

Short enumeration of such natural powers as afford perpetual motions. Description of the barometrical clock in Cox's museum. Other methods of applying the barometer to similar purposes. Thermometers, fluid and solid. Construction of self-moving wheels by expansion-bars.

VII. Chemical Considerations on the Use of the Oxides of Iron in the dyeing of Cotton. By J. A. Chaptal. (Concluded from p. 93) p. 129

Nankeen colours. Violet from madder and iron. Peculiar management. Observations on astringent vegetables, particularly galls.

VIII. Ac-

VIII. Account of the Experiments of Citizen Clouet on the different States of Iron, and its Conversion into Cast-steel - - - p. 131

Concise history of the science of steel-making. Combinations of iron and carbone—of iron and glafs—of iron, carbone and glafs—and of iron and its oxide. Report of experiments, particularly those in which the carbone was afforded by carbonate of lime. Theoretical and general observations on this method.

Scientific News. Account of Books, &c. - - - p. 138

Mr. Dodd's plan for a bridge, sufficiently elevated to admit ships above the present London bridge—Mr. Dance's survey, by order of the city. Exposition of facts tending to shew how far the ascent of this lofty bridge might prove inconvenient. On the Gloucestershire strontian. Silix in Dutch rush. British Mineralogical Society. Kelly's spherics.

J U L Y 1799.

Engravings of the following Objects: 1. Uncommon Effects of the Refraction of the Air at Sea. 2. A Brew-house Boiler and Furnace, by Count Rumford.

I. Observations upon an unusual horizontal Refraction of the Air, with Remarks on the Variations to which the lower Parts of the Atmosphere are sometimes subject. By the Rev. S. Vince, A.M. F.R.S. and Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge. p. 141

Strange appearance of ships seen in the horizon from Ramsgate. Two images, one inverted and the other erect, besides the object itself. Calais cliffs. Theory.

II. An Account of some Endeavours to ascertain a Standard of Weight and Measure. By Sir George Shuckburgh Evelyn, F.R.S. and A. S. (Continued from page 107 of the present Volume) - - - p. 147

Determination of the specific gravity of a metallic cube and cylinder. Weight of a cubic inch of water. Examination of a metallic sphere.

III. On certain useful Properties of the oxygenated muriatic Acid. By the Rev. Alexander John Forsyth - - - p. 158

Colours brightened by oxymuriate of carbonated potash. Ink rendered black. Effect on other dyeing materials. Observations. Effect of the same salt on soap and expressed oils.

IV. On the Management of Fire, particularly with regard to the Construction of Boilers. By Benjamin, Count of Rumford - - - p. 161

General remarks. Production of heat by combustion, explained and applied to the construction of furnaces: Combined and radiant heat. Conductors and non-conductors. Cloathing, &c. Flame is a hot wind. Form of boilers and furnaces. Measure of effect. Fuel—quantity saved. Coke, pit-coal, charcoal, oak. Description of a brew-house boiler. Regulating door.

V. Ex-

- V. Extraction of Sugar from Carrots, &c. Experiments on Barytes and Strontites. Extract of a Letter from Mr. William Henry of Manchester, to the Editor, dated June 20th, 1799 - - - p. 168

Processes for obtaining sugar in considerable quantity from carrots, and other vegetables. Experiments on strontian and barytes.

- VI. Concerning those perpetual Motions which are producible in Machines, by the Rise and Fall of the Barometer, or the thermometrical Variations of the Dimensions of Bodies. W. N. (Concluded from p. 128) p. 172

First-movers deriving their power from the expansion of compound bars, of metallic chains, of catgut, &c.—Estimates of the quantity of force accumulated by the barometrical and thermometrical contrivances before described.

- VII. Essays on the Art of Dyeing, by Means of the Solutions of Tin, and the coloured Oxydes of that Metal. By J. M. Hauffman - - - p. 175

Solutions of tin in various acids, mixed with decoctions of vegetable colouring matters afford combinations of the oxyde with those colours; which may serve as pigments, or be fixed on piece-goods. Variations of the processes and effects. Conditions and manipulations described and enumerated.

- VIII. Observations on the Manners, Habits, and Natural History of the Elephant. By John Corfe, Esq. - - - p. 181

Incorrect relations concerning the elephant; his sentiments of modesty, slavery, &c. Size and growth. Extreme size. What may be his real degree of sagacity.

- Philosophical News and Accounts of Books, &c. - - - p. 185

Chladni's experiments on the resonant figures on glass. Philosophical transactions. Berthoud on time-pieces.

A U G U S T 1799.

- Engravings of the following Objects: 1. Regnier's new Powder-Proof. 2. A new Pendulum; and three Figures illustrative of the Reflexibility of Light.

- I. Supplement to the Paper on the philosophical Uses of a common Watch. By the Rev. W. Pearson of Lincoln - - - 189

Additional numbers for watch trains. Remarks on the clock with a short pendulum, &c.

- II. Observations on the Manners, Habits, and Natural History of the Elephant. (Continued from page 185) - - - p. 193

Narrative of incidents, which shew that the elephant is not so sagacious but that he may be retaken after having made his escape. It is a mistake to assert that this animal will not breed in a domestic state. Facts which prove to the contrary.

- III. Description and Use of a portable Instrument for comparing the Force of Gunpowder. By Cit. Regnier - - - p. 198
The

The instrument consists of two radii connected together at the angle, by a spring, in the same manner as dividers and other well-known implements. The explosion from a small cannon drives these legs asunder, and the quantity of their divergence is measured by a graduated arc and sliding piece.

- IV. An Account of some Endeavours to ascertain a Standard of Weight and Measure. By Sir George Shuckburgh Evelyn, Bart. F.R.S. and A.S. (Continued from page 146). - - - p. 200

Accurate determination of the lengths of the existing standards of longitudinal measure, kept at the apartments of the Royal Society, in the Tower of London, and in the Exchequer. Comparison of the weights used by the author, with the Troy pound constructed by order of the house of commons by Mr. Harris, in 1758. General remarks, with some observations on the prices of provisions at different periods of our history.

- V. Description of a new Arrangement of the Bars in the Gridiron Pendulum, with a Drawing. (W. N.) - - - p. 200

The novelty of this pendulum consists in the arrangement of the steel bars round a rod of zinc, and in a simple method of adjusting the lengths required for the compensation.

- VI. On the Epocha of the Discovery of the Telescope, and the Opinion of Boyle that Plants derive their Nourishment from Water only. By Cit. Boissonade - - - p. 207

Account of an ancient drawing, described by Mabillon, and also of a passage in Strabo, which have been thought to render it probable that the ancients were acquainted with the telescope. Reasons why these documents ought not to be considered as conclusive. Whether spectacles were known in the thirteenth century. Quotation in proof that the nourishment of plants, chiefly by water, was known much earlier than the time of Vanhelmont and Boyle.

- VII. Observations on the Proportion of real Acid in the three known mineral Abids, and on the Ingredients in various neutral Salts and other Compounds. By Richard Kirwan, Esq. LL.D. F.R.S. and M.R.I.A. - - - p. 210

Introductory remarks. Experiments on the changes of density in acids by heat. Tables of the quantities of real acid contained in vitriolic, nitrous, and marine acids of different densities; of the quantities of acid taken by the several alkalies and earths; of the quantities of alkalies and earths taken up by the several acids; of the quantities of neutral salt afforded by 100 parts of each of the several alkalies, earths, and acids; and also of the proportions of ingredients in these salts.

- VIII. On a submarine Forest on the east Coast of England. By Joseph Correa de Serra, LL.D. F.R.S. and A.S. - - - p. 216

Account of certain islets on the coast of Lincolnshire, which are chiefly composed of decayed trees. The same stratum of vegetable remains extend beneath large tracts of the neighbouring country. Interesting enquiry concerning the former state of this submarine forest, and the causes of its present state.

- IX. Optical Remarks, chiefly relating to the Reflexibility of the Rays of Light. By P. Prevost, Professor of Philosophy at Geneva; of the Academy of Berlin; of the Society des Curieux de la Nature; and of the Royal Society of Edinburgh - - - p. 222

Enquiry

Enquiry whether light be unequally reflexible in the sense of Sir Isaac Newton. From an examination of the circumstances it is shewn that the effect described by Newton was produced by a real difference in reflexivity, and not in consequence of refraction at the first surface of the prism, as Mr. Brougham has asserted. Examination of Mr. Brougham's experiment with a vertical prism. Enquiry whether the different rays of light be differently reflected in Mr. Brougham's acceptation of the terms; that is, whether the angles of incidence and reflection be in any case unequal. It is shewn by a diagram, that the effects observed by Mr. Brougham, may be accounted for by his own hypothesis, that the red ray is more strongly repelled than the violet, without having recourse to the supposition that the angles of incidence and reflection are unequal.

X. On the Manufacture of the Beech Oil, by Aaron Hill; and other Projects of Improvement. By a Correspondent - - p. 229

Accounts of the invention of the stocking-engine, and a number of other projects. History of the manufacture of oil from the beech-nut, at the rate of two gallons per bushel. Attempts at the establishment of a trading company for this article.

XI. Letter from Mr. Troughton respecting a Balance of his Construction p. 233

Scientific News. Account of Books, &c.

Kirwan's Geological Essays. Buée's Mathematical Enquiries concerning the Texture of Bodies. Death of Galvani. On the Acoustic Experiments of Chaldni and Jacquin. New fulminating Mercury.

S E P T E M B E R 1799.

Engravings of the following Objects: 1. The apparent Attractions of Floating Bodies: and, 2. Keith's Self-registering Thermometer.

I. A circumstantial Description of the Method of cultivating the white Beet (Runkelrube), in order to obtain the greatest Quantity of Saccharine Matter, and to prepare it for the Manufacture of Sugar. By F. C. Achard, Director of the Physical Class in the Royal Academy of Sciences - p. 237

Description of the plant. Great variations in the saccharine product. Influence of culture upon that product. Instructions and estimates. General remarks.

II. Appendix to Sir George Shuckburgh Evelyn's Paper on the Means of ascertaining a Standard of Weight and Measure - - p. 243

Determinations of the lengths and comparative accuracy of several scales of length. 1. That used by General Roy; 2. That of Mr. Aubert; 3. That of the Royal Society; and, 4. The standard of Henry VII. Reduction of thirteen several scales of authority to the measure of Troughton.

III. Observations on the Manners, Habits, and Natural History of the Elephant. By John Corfe, Esq. (Continued from page 197) - p. 247

Interesting facts and results concerning the sexual intercourse and propagation of the elephant.

IV. Extract of a Letter on the Art of Bleaching, and the Effect of Friction in Water-courses for Mills. By a Correspondent - - p. 252

- V. On Sulphuret of Lime to be used as a Substitute for Potash in Bleaching with the Oxygenated Muriatic Acid. By Mr. William Higgins, M.R.I.A. and Professor of Chemistry and Mineralogy at the Repository of the Dublin Society - - - - - p. 253
- Preparation of the sulphuret of lime. Instructions for using it in bleaching. Documents by which the advantages and economy of this method are shewn.
- VI. On the Decomposition of the Acid of Borax, or Sedative Salt. By Laurence de Crell, M.D. F.R.S. London and Edinburgh, and M.R.I.A. Translated from the German - - - - - p. 257
- Account of a process in which the acid of borax was treated by long-continued digestion with the oxygenated muriatic acid and repeated distillations.
- VII. Reflections on the Qualities of Pottery, with the Results of some Analyses of Earths and of common Pottery. By Cit. Vauquelin - - - - - p. 262
- Concerning the effects of the several earths in the composition of pottery; their mechanical division; the requisite heat of firing, and proper coating or glaze. Component parts of eight different articles or materials of pottery:
- VIII. Description of a Thermometer which marks the greatest Degree of Heat and Cold from one time of Observation to another, and may also register its own Height every Instant. By Alexander Keith, Esq. F.R.S. and A.S. Edinburgh - - - - - p. 264
- Thermometer with a float which either raises or depresses two small index pieces, or regulates a pencil for tracing the temperature on the face of a cylinder moved by clock-work.
- IX. On the Effects of apparent Attraction or Repulsion, between Bodies floating on or immersed in Fluids. By M. Monge. - - - - - p. 269
- Enumeration of the facts in which floating bodies either rush together, or are separated, in consequence of the fluid which is adherent to them, or made to form a cavity by repulsion. Investigation of the causes, with remarks and applications to certain phenomena in chemistry.
- X. Observations on the Proportion of real Acid in the three ancient known Mineral Acids, and on the Ingredients in various neutral Salts and other Compounds. By Richard Kirwan, Esq. LL.D. F.R.S. and M.R.I.A. (Continued from page 215) - - - - - p. 273
- Instructions for the use of the tables.
- XI. Experiments on certain Principles obtained from Animal Substances treated with the nitrous Acid. By Cit. Welter - - - - - p. 277
- XII. A short Account of the Life and Writings of Defausture. By A. P. Decandole - - - - - p. 278
- Scientific News. Accounts of Books, &c. - - - - - p. 282
- On Steam. Universal Writing. French Weights and Measures. Royal Institution. Kirwan's Essays.

O C T O B E R 1799.

Engravings of the following Objects: 1. Sections illustrative of the Geology of the Country near Hull; and, 2. Guyton's Apparatus for the Combustion of the Diamond in Oxygen by Solar Light.

I. Geological Observations on the Vicinity of Hull and Beverly. By John Alderfon, M. D. - - - - - p. 285

II. A circumstantial Description of the Method of cultivating the white Beet (Runkelrube), in order to obtain the greatest Quantity of saccharine Matter, and to prepare it for the Manufacture of Sugar (Concluded from p. 243) p. 288

Observations in proof of the assertion, that roots afford more saccharine matter when defended from light. Theoretical explanation from the less quantity of oxygen being separated in these circumstances. Recapitulation.

III. On the Decomposition of the Acid of Borax, or Sedative Salt. By Laurence de Crell, M.D. F.R.S. London and Edinburgh. (Concluded from page 257) - - - - - p. 292

The course of experiments shew, in a general way, that the acid of borax is decomposed, and that one of its component parts is coal or carbone. But what may be the cause why it is so difficultly separated, and does not, in its original combination, deflagrate with nitre, remains to be proved by future researches.

IV. Observations on the Means by which the Mountains in the Cevennes are rendered fertile. By Cit. Chaptal - - - - - p. 295.

The inhabitants of the Cevennes exert a surprising degree of industry and perseverance, in building walls across the ravines, and at the edges of the calcareous platforms; by means of which the vegetable earth is retained, and the whole surface of these naturally barren mountains rendered so productive as to support two or three hundred thousand people.

V. Account of certain Experiments and Inferences respecting the Combustion of the Diamond, and the Nature of its Composition. By Cit. Guyton p. 298

Phenomena of the combustion of the diamond in oxygen gas by solar heat. Method of filling large vessels with the gas, without the previous introduction of mercury or water. The diamond cannot maintain the temperature requisite to continue its own combustion. It is totally combustible, and affords pure carbonic acid, in much greater quantity than charcoal does. Theory: that diamond is pure carbone, and that plumbago, charcoal, and carbonic gas, consist of the same basis, oxygenated in different degrees. Interesting remarks and conclusions.

VI. On the apparently spontaneous Combustion of living Individuals of the Human Species. By Citizen Lair - - - - - p. 305

VII. Remarks on the Conflagration of the Odeon. By B. G. Sage, Professor and Director of the first School of Mines - - - - - p. 306

The most remarkable circumstances attending this fire, were the probable explosion of hydrogen in the great hall, and the developement of much sulphur in various parts of the building, by the disoxygenation of the plaster of sulphate of lime.

- VIII. On the chemical Action of different Metals on each other, at the common Temperature of the Atmosphere. By Cit. Fabroni - p. 308
 Mr. Fabroni is disposed, from some experiments, to conclude, that the galvanic stimuli from the application of metals to the tongue, &c. are merely the consequence of chemical actions.
- IX. On the Harp of Eolus. By Matthew Young, B.D. - p. 310
 Experiments on a single string of the Eolian harp. Enumeration of its tones. Theory.
- X. Analysis of the Chromate of Iron, of the Bastide de la Carrade. By Cit. Taflaert - - - - - p. 314
- XI. Report made to the National Institute of Sciences and Arts (at Paris), on the 29th Prairial, in the seventh Year (June 17, 1799), in the Name of the Class of Physical and Mathematical Sciences, on the Measure of the Meridian of France, and the Results which have been deduced to determine the new Metrical System - - - - - p. 316
- XII. Detached Facts, or Notices on chemical Subjects—Nitric Acid—Oxide of Azote—Indigo—Nitrate of Potash—Residue of Ether—Inflammation of Oils by Nitric Acid—Ammoniac—Putrid Water at Sea. By Cit. Proust, Professor of Chemistry at Madrid - - - - - p. 325
- Scientific News. Account of Books, &c. - - - - - p. 329

N O V E M B E R 1799.

- Engravings of the following Objects: 1. A new silent Escapement for Clocks. By Mr. Goodrich 2. The Furnace of Professor Pott. 3. Apparatus for converting Iron into Steel, by means of the Diamond; and, 4. A Lamp for burning Tallow, by Mr. William Close.
- I. Chemical Experiments and Observations on the Production of Sugar, and an useful Syrup from indigenous Plants. By Sigismund Frederic Hermbstadt - - - - - p. 333
 General observations. Facts and observations concerning the quantities of sugar obtained from the sugar-cane, the maple, Indian corn, Siberian bear's-breech, or katfeh, the juice of grapes, and the birch.
- II. Observations on the various Kinds of Manure. By John Middleton, Esq. - - - - - p. 340
 Experiments on a considerable scale with peat-ashes, coal-ashes, wood-ashes, malt-dust, foot, soap-makers' waste, the sweepings of London streets, the soil of privies, and farm-yard dung, as manures.

- III. Description of a new Escapement for Clocks. By Mr. Simon Goodrich p. 342
Escapement consisting of a crank upon the arbor of the last wheel of the train. Its advantages.
- IV. Description of the Furnace of Professor Pott - - - P. 344
- V. Process used to separate the Mucilage from Linseed Oil; Methods of dissolving Copal for Varnish, &c. By Mr. Timothy Sheldrake - - - P. 345
- VI. Analysis of the Spinel. By Professor Klaproth - - - P. 349
- VII. Verbal Process of the Conversion of Soft Iron into Cast Steel, by means of the Diamond. By Cit. Guyton - - - P. 350
- Examination of the elastic products in the common process of cementation for making steel. Processes with the diamond. A cavity was bored in a block of soft iron, and a stopper fitted of the same material. Into this cavity was put a diamond, with some filings of the iron to fill the remaining space, after which the stopper was driven in. In this state (the diamond weighing almost one sixteenth part of the mass) the whole was put into a crucible, and this placed in another; and the space between the two crucibles filled with pure siliceous sand. The external crucible was well closed and luted, and the whole exposed to the strong ignition of a blast furnace, by which the metal was fused completely; the diamond disappeared, and the compound was found to be steel.
- VIII. Notice of the Experiments made by order of the Minister of the Interior of France, relative to the Fineness of Tin or Pewter - - - p. 356
- The method recommended is to examine the specific gravity. It is less in compounds of lead and tin than that would be deduced by computation. Experimental results.
- IX. A Chemical Examination of the Bath Waters. By G. S. Gibbes, B.M. F.R.S. - - - - - P. 359
- X. Construction of a Lamp for burning Tallow. By Mr. William Close p. 363
- XI. Report made to the Institute of Sciences and Arts (at Paris), on the 29. Prairial, in the Seventh Year (June 17, 1799), in the Name of the Class of Physical and Mathematical Sciences, on the Measure of the Meridian of France, and the Results which have been deduced to determine the new Metrical System. (Concluded from page 324) - - - p. 365
- Method by which the unity of weight was deduced from the metre. A hollow cylinder of brass was constructed, and its dimensions ascertained by measuring 37 different lines of length, and 48 different diameters. It was weighed in air and in water at known temperatures, and from the difference of the weight duly corrected, the cubic decimetre of water, or true kilogramme, was found. The ratio between this weight and the marc of the pile of Charlemagne was also settled. Standards of the metre, and the kilogramme in platina, in iron, and in brass. Length of the simple pendulum for seconds at Paris.
- XII. On the Web of the Garden-Spider. By a Correspondent - - - p. 369
- Adhesive quality of the polygonal threads of the spider's web. Observations.
- XIII. Plan of Experiments which were made in the Garden of Plants p. 370
On

On the propagation of the sheep with the goat. Treatment of sheep. On various animals used for food, &c.

XIV. On the native Iron of Peru, and on Mercury contained in Sea-Salt. By
Professur Proust of Madrid - - - - - P. 374

XV. Note concerning the Earthquake in Peru. By M. Cavanilles P. 377

Account of Books - - - - - P. 379

D E C E M B E R 1799.

Engravings of the following Objects: 1. The Gazometer of Seguin; and, 2. Figures illustrative of the theory and fabrication of Spirit Levels.

I. An Inquiry concerning the Weight ascribed to Heat. By Benjamin Count of
Rumford, F.R.S. &c. - - - - - p. 381

Upwards of four thousand grains of water was counterpoised against a like quantity of weak ardent spirit, in bottles of the same magnitude, hermetically sealed; the temperature being 61° . The apparatus being then removed into a room at the temperature 29° the water froze and preponderated by upwards of one-tenth of a grain. The equilibrium was restored when the ice was fused again in the warm room. Suspicions of fallacy, however, led the inventor to other researches. By exposing the balance, loaded with two balls of gilt brads, to different temperatures, it was proved that no unequal expansion of the arms had taken place. On repeating the original experiment with mercury, instead of ardent spirit, no change of equilibrium ensued from the congelation of the water; and when again with the most minute attention to the equality of temperature in three masses, of water, of mercury, and of diluted ardent spirit which were respectively put in equilibrio at 60° and at 30° no alteration was found at either temperature. Observations and remarks.

II. On the Plants used by the Ancient People of Europe to poison their Arrows.
By C. Ch. Coquebert - - - - - P. 390

III. Experiments to determine the Quantity of Tanning Principle and Gallic Acid, contained in the Bark of various Trees. By George Biggin, Esq.
P. 392

The tanning principle was separated by quick infusion, and the gallic acid by subsequent infusion for a longer time. The quantities of the former were ascertained by its precipitation with glue, and the latter was judged of from the intensity of colour it afforded with sulphate of iron. Results of trials on twenty-one samples of barks.

IV. On the Plumb Line and Spirit Level. By M. Chezy - - - - - p. 396

Description of the plumb line. Its degree of accuracy. Spirit level described, its qualities investigated. Method of grinding its inner surface. Annotations theoretic and practical.

V. A Chemical Examination of the Bath Waters. By George Smith Gibbes,
M. D. F. R. S. (continued) - - - - - P. 403

Detection of a considerable portion of silica in the Bath waters.

- VI. Account of the new Gazometer of C. Seguin - p. 405
 This principal advantage of this instrument consists in the gases being subjected to a very regular and measurable pressure.
- VII. Instructions for making red Crayons. By A. F. Lomet p. 406
- VIII. Chemical Experiments and Observations on the production of Sugar, and an useful Syrup from indigenous Plants. By Sigismund Frederic Hermstadt, (concluded from page 339) - - - p. 410
- Facts and observations concerning the quantities of sugar obtained from the white beet, the runkel-rübe, common beet, carrots, turnips, colewort, skirrets, and parsnips.
- IX. On the silent Escapement of Mr. Goodrich. By a Correspondent p. 419
- X. Results of various Experiments for determining the Quantity of Action which Men can afford by their Daily Work, according to the different Manners in which they employ their Strength. By Citizen Coulomb - p. 419
- XI. New Researches into the Affinities which the Earths exert upon each other in the humid and in the dry Way. By Citizen Guyton - p. 419
- XII. Account of the Improvements made on the Farm in the Great Park of his Majesty, the King, at Windsor. By Nathaniel Kent, Esq. p. 422
- Account of Books, &c. - - - p. 426

J A N U A R Y 1800.

Engravings of the following Objects: 1. The Urceola, or Caout-chouc Plant.
2. The Photometer of Leslie. 3. The Hydrostatic Lamp of Keir.

I. Account of the Improvements made on the Farm in the Great Park of his Majesty, the King, at Windsor. By Nathaniel Kent, Esq. - p. 429

II. Extract of a Letter from — Petrie, Esq. on Board the Good Hope Indiaman, at Sea, Lat. $35^{\circ} 40'$ S. Long. 44° E. giving an Account of a singular Accident by Lightning. Communicated by Dr. Robert Petrie p. 432

One of the seamen was struck by lightning, and rendered insensible for some minutes. The singularity of this case, was, that a portion of the pericranium on the right side of the head of the size of a dollar, mortified and sloughed off, without any previous inflammation. Remarks and observations.

III. A Botanical Description of the Urceola Elastica, or Caout-Chouc Vine of Sumatra and Pulopinang; with an Account of the Properties of its inspissated Juice compared with those of the American Caout-Chouc. By William Roxburgh, M. D. - - - - - p. 345

IV. On the different Sorts of Lime used in Agriculture. By Smithson Tennant, F.R.S. - - - - - p. 404

The author being informed that two different kinds of lime are used in agriculture, in the neighbourhood of Doncaster; one of which gives fertility, and the other is deleterious to vegetation, was induced to examine them, and found that the first was pure lime, and the latter contained two thirds magnesia. Hence he was led to investigate the general scientific and agricultural relations of these two earths, in their various, natural, and artificial compounds.

V. Account of the strange Effects produced by Respiration of the gaseous Oxide of Azote - - - - - p. 446

From a considerable number of instances here stated, it appears that this gas acts strongly on the system, producing an increase of nervous energy, and affecting the mind in a very remarkable manner, in most cases producing great exhilaration.

VI. A chemical Examination of the Bath Waters. By George Smith Gibbes. (Concluded from page 404) - - - - - p. 452

VII. Analysis of an Iron Ore, the Composition of which has been hitherto misunderstood. By Mr. William Henry, including a Letter on Ores of Iron, addressed to Mr. Thomas Henry, F.R.S. By Charles Hatchett, Esq. F.R.S. p. 454

VIII. An Account of some Experiments on the Fecundation of Vegetables. By Thomas Andrew Knight, Esq. - - - - - p. 458

IX.

- IX. Description of an Hygrometer and Photometer. By Mr. John Leslie.
Communicated by the Inventor - - - p. 461

If of two similar thermometers, the ball of one be blacked, while the glass of the other remains clear and transparent: the temperature of the former will be more elevated by light, than that of the latter; or if the ball of one thermometer be wetted, while the other remains dry, the evaporation will diminish the temperature of the first. The thermometers may thus be made to indicate the intensity of light, or the absorption of humidity. The instrument of the inventor consists of two air thermometers, connected together by a syphon tube, in which a fluid moves, accordingly as the spring of the included air in one or the other ball is affected, by the before mentioned causes.

- X. Description of the Hydrostatic Lamp of Mr. Peter Keir - - - p. 467

Observations on the methods of supplying lamps with oil. Fountain lamp; its excellencies and defects. The lamp of Robert Hooke, which preserved the oil accurately at the same level. Hydrostatic lamp.

- XI. Reflections on the Decomposition of the Muriate of Soda by the Oxide of Lead. By Cit. Vauquelin - - - p. 470

- Philosophical News. Accounts of Books, &c. - - - p. 473

Philosophical Transactions. Lord Dundonald's Prospectus. Sugar from the White Beet. Count Rumford's Essays. Pearson's Chemical Nomenclature. Mudge's Description of a Time-Keeper. Adams' Electricity.

F E B R U A R Y 1800.

Engravings of the following Objects: 1. Apparatus for determining the Action of Fluids; and, 2. A Vessel for boiling inflammable Fluids.

- I. Experiments on Indigo. By a Correspondent - - - p. 477

Treatment of Indigo with inflammable bodies to deprive it of oxygen. New volatile product of a metallic aspect, not magnetical, nor acted upon by muriatic acid nor alcohol, nor boiling caustic alkali, nor boiling concentrated sulphuric acid; but rapidly soluble in nitrous acid, and precipitable of a white colour by alkalis, &c.—A fluid, having the qualities of laurel-water, produced by abstracting nitrous acid from Indigo. Applications of oxygen to Indigo, &c.

- II. New Researches into the Affinities which the Earths exert upon each other in the humid and dry Way. By Citizen Guyton. (Concluded from p. 422)

- III. Concerning the Influence of the Moon on the Atmosphere of the Earth. By C. Lamarck - - - p. 488

From a series of observations, the author presents the following principles as established; viz. that while the moon has north declination, the winds in this climate are south, south-west, and west, with cloudy, damp, rainy, or stormy weather; but that the opposite winds, with clear settled weather, predominate during the south declination of that planet. These effects are modified by various circumstances, such as the positions of the moon in its orbit, its conjunctions and oppositions, &c.

- IV. Directions for making the best Composition for the Metals of Reflecting Telescopes, and the Method of casting, grinding, polishing, and giving the great Speculum the true parabolic Figure. By the Rev. John Edwards, B.A.
p. 490

- V. Early Developement of the Antiphlogistian Theory of Combustion. By Robert Hooke - - - - - p. 497

Mayow's Theory was published in 1674, but Hooke's Micrographia, containing a detail of the same theory, was licensed in 1664, and published in the year following, 1665.

- VI. Experiments and Observations on Shell and Bone. By Charles Hatchett, Esq. F.R.S. - - - - - p. 500

- VII. Experiments upon the Resistance of Bodies moving in Fluids. By the Rev. Sam. Vince, A.M. F.R.S. Plumian Professor of Astronomy and Natural Philosophy in the University of Cambridge - - - p. 506

- VIII. An Account of a Kettle for boiling inflammable Fluids. In a Letter from Tho. P. Smith to Robt. Patterson - - - p. 514

His contrivance consists of a lip, spout, or rim, into which the fluid spreads itself when it rises by ebullition. By this means the expanded part becomes cooled, and checks the boiling of the rest.

- IX. Letter from Mr. Davy, Superintendant of the Pneumatic Institution, to Mr. Nicholson, on the Nitrous Oxide, or Gaseous Oxide of Azote, on certain Facts relating to Heat and Light, and on the Discovery of the Decomposition of the Carbonate and Sulphate of Ammoniac - - - p. 515

- X. Additional Remarks on the Hygrometer and Photometer. By Mr. John Leslie - - - - - p. 518

- XI. An Account of some Experiments on the Fecundation of Vegetables. By Thomas Andrew Knight, Esq. - - - - - p. 519

- Philosophical News, &c. - - - - - p. 522

On the Soldering of Glafs.—Simple Method of obviating the Resistance of the Air against the Pendulums of Clocks.—Distillation by the Application of the cooling Mixture.

M A R C H 1800.

Engravings of the following Objects: The Air Furnace of Cit. Lecour; and,
2. An Improvement in Mr. Clofe's Tallow Lamp.

I. Directions for making the best Composition for the Metals of Reflecting
Telescopes, and the Method of casting, grinding, polishing, and giving the
great Speculum the true parabolic Figure. By the Rev. John Edwards B. A.
(Concluded from p. 497) - - - p. 525

II. Experiments and Observations on Shell and Bone. By Charles Hatchett,
Esq. F. R. S. (Concluded from p. 506) - - - p. 529

III. Some Account of the Elastic Gum Vine of Prince of Wales's Island, and of
Experiments made on the milky Juice which it produces: with Hints respect-
ing the useful Purposes to which it may be applied. By James Howison, Esq.
p. 534

IV. Miscellaneous Observations relative to the Western Parts of Pennsylvania,
particularly those in the Neighbourhood of Lake Erie. By Andrew Ellicott
p. 539

V. Account of the Pearl Fishery in the Gulph of Manar, in March and April,
1797. By Henry J. Le Beck, Esq. - - - p. 542

VI. Description of the Air Furnace of Cit. Lecour - - - p. 546

VII. On the Lamp for Tallow, and the Combustion of that Material. By Mr.
William Clofe - - - p. 547

ERRATA.

Page 179, *note*, read *Journal de Physique, An. VII. p. 114.*

365, in the note, for *succeeded* read *superseded.*

476, bottom, for *Galearies* read *Galvanism.*

497, line 22, for 1675, read 1665.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

APRIL 1799.

ARTICLE I.

*A Statical Inquiry into the Source of Nutrition in Succulent Vegetables. By MR. J. GOUGH.**

Kendall, Feb. 7, 1799.

SUCCULENT plants are commonly supposed to derive a greater proportion of nutriment from the atmosphere, than vegetables of a less humid constitution; and this idea is supported, at least in appearance, by a singularity in the economy of the plants under consideration. If the sempervivum tectorum, or any of our indigenous sedums, be suspended in the dry air of a chamber, they will live for a long time; whereas, other vegetables wither soon after their connection with the ground is interrupted. Many persons, attending to the preceding facts, and reasoning from analogy, have concluded, that sempervivum and sedum, with the rest of the succulent tribe, derive this supply not from the earth but from the atmosphere, because they can subsist independent of the former: but natural history points out a second analogy, which, although it is found in the animal kingdom, should not be overlooked, on account of its strict coincidence with the foregoing deviation from the prevailing habits of the inferior class of organized bodies. The chameleon bears hunger for months together, when in confinement: this peculiarity, added to the difficulty of observing it take its prey, persuaded the ancients that the reptile in question is nourished by the air alone, but the industry of later naturalists †, has shewn this notion to be false, by proving it to feed on flies; and that the same powers of abstinence are extended to serpents, spiders, and many more

* Communicated by the author.

† Vide Raii Synop. animalium Quadrup.

animals, whose opportunities to procure food are precarious. Now as succulent plants are intended to grow on rocks, and in other dry situations, where their supplies of humidity suffer frequent and long interruptions, in all probability they bear the same relation to the vegetable tribes, which reptiles bear to other animated beings.

The question whether of the two opinions now stated is true, will be best resolved by a set of statical experiments, made with succulent plants surrounded by dry air. Should they appear to gain weight when treated in this manner, the former notion must be adopted; but should they happen to lose part of their substance, the latter will have the preference: for the act of vegetating implies the growth of parts already formed, and the production of new organs to complete the plant. The following experiments were made, in order to decide the question under consideration; which I shall now relate without further apology.

Experiment 1.* Three plants of *sedum acre*, weighing $69\frac{1}{4}$ grains, were suspended in a proper manner before a window, on the 6th of September, 1790: being again examined on the 25th of the same month, their weight was reduced to 42 grains. They were then placed in water for 24 hours, and afterwards exposed to the light and air for half that period, to give the water adhering to the plants an opportunity to evaporate. At the end of this time, their weight was $63\frac{1}{2}$ grains; which is $6\frac{1}{2}$ grains less than their original weight. Perhaps the preceding deficiency is owing in part to the plants not being weighed before part of their sap was lost together with the superfluous water; but the following circumstance should also be taken into the account. If a glass of limewater be placed under a glass jar containing a plant of *sedum*, the lime will be precipitated in the course of three or four days; from which we may conclude with reason, that part of the carbon of the plant is converted, in all similar cases, into gas by the oxygen of the atmosphere. The same experiment being repeated with *sedum reflexum*, as well as *sedum acre*, a considerable loss of weight took place in both trials; and I think the tendency and uniformity of the result will justify the succeeding remarks and conclusions.

The *sedum acre*, in all probability, draws no more nutriment from the air than other plants; at the same time, the herb in question, as well as the rest of the succulent tribe, can evidently retain life for an unusual length of time, in situations incapable of affording them nutrition in the common way. The vegetative principle even continues to act in the *sedum*, when suspended by the roots, apparently with a view to its preservation; for the branches of the plants used for the experiment curled, by turning their tips, contrary to the general habit of vegetables, from the window: by this provident singularity, much light was avoided, and with it an unnecessary expenditure of sap. The roots in the mean time produced a number of fine filaments, as if in search of humidity; for they seemed, in this instance, to imitate trees growing on old buildings, which have been known to connect themselves with the ground, by means of roots descending along the sides of the walls. This peculiarity in the constitutions of the *sedums* is absolutely necessary to their existence; for these plants being intended to grow upon rocks and walls but slightly covered with earths, would be burned up and destroyed by the droughts of summer, but their many succulent leaves obviate the disaster, by performing the

* The substance of this experiment is inserted in *Wither. Bot. Arrangement*, edit. 3d. Vide art. *Sedum*.

office of so many receptacles of sap sufficient to answer the demands of these trying seasons; and which are recruited in a greater or less degree by every shower. These magazines of food are emptied in the flowering season, probably to feed the expanding capsules; for by the time the seeds are ripe, the plants are exhausted, and suffer what may be called a vegetable death, by a natural process.

Experiment 2. The *sempervivum tectorum*, which resembles the sedums in habit, also agrees with them in the property in question: for an offset of *sempervivum*, of the weight of 250 grains, being left in a window from the 29th of April to the third of June, lost 116 grains, and appeared to be in a very exhausted condition, but its faculty to vegetate remained unimpaired; for the same offset was afterwards placed for an hour every other day in a glass of rain-water to the end of the month, by which it was so much recruited, as to produce a new shoot, and the joint weight of parent and offspring amounted to 170 grains. Notwithstanding the conclusive result of the two last experiments, it may, perhaps, be alleged, that the dry air of a chamber does not give succulent plants that opportunity to procure water which the atmosphere affords them during fair weather, especially in the nights, when the dews are formed: but this objection seems to be overturned by the succeeding paragraph.

Experiment 3. I suspended different offsets of *sempervivum tectorum* in glass-bells, having their mouths placed in water; which did not rise in the vessels far enough to touch the plant. The air which surrounded the offsets was kept so moist by this sort of confinement that the insides of the jars appeared constantly covered with vapour: at the same time care was taken not to expose the apparatus to the direct rays of the sun. A plant treated in this manner in June, lost a sixth part of its weight in a fortnight. Another plant, subjected to a similar trial during the two last weeks of September, lost but $\frac{1}{4}$ part of its weight. The loss of substance which is here remarked, will be best explained by the following fact.—Dew does not form with equal facility on all kinds of bodies; especially in a temperature of 50°, or higher: for instance, glass collects dew from air, which is not in a state to impart the same to varnished wood or polished metals; and were we to form a scale of these affinities by experiment, green vegetables would take their respective stations, somewhere between glass and some one of the metals; because, when the three substances in question are exposed to the open air, glass is covered with dew in the first place; and the vapour fixes on clean metallic surfaces, after the leaves of living shrubs. On this account, the water contained by the air used in the last experiment, attached itself to the sides of the jars, in preference to the offsets of *sempervivum*; which being deprived of dew, could not repair the loss they suffered by evaporation for want of aqueous particles in a condensed form. The uniform result of the preceding experiments seems to prove, beyond a possibility of doubt, that the succulent vegetables of Europe receive their portion of nutrition by the common vehicle of water conveyed to them in the common way by the earth, or deposited on their leaves in the form of rain; and I have reason to suppose that plants of the same description, natives of tropical countries, are regulated by the same principle of economy; for a plant of *aloe perfoliata*, which was suspended for 52 days, about the conclusion of autumn, in a room with a northern aspect, lost something more than one-fourth of its weight in this dry situation; but no attempts were made to repair the loss by the methods pursued in the first experiment.

II.

Description of a Pocket Ribband-machine, for charging a small coated Phial with Electricity.
By J. TILL. ULLOUSTONE.

FIG. 1 and 2, Pl. I, are representations of this instrument of its real size; A B C D, fig. 1 and 2, are two flat mahogany boards, each three inches long, two inches broad, and three-eighths of an inch thick. In the holes E and F, fig. 2, which are two inches from each other, and equidistant from the sides and ends of the boards, are glued fast two cylindrical pins of box-wood, each one-fourth of an inch in diameter; these pins project about one inch and a quarter, perpendicularly, from the opposite surface of the board, and have their ends formed into male screws about one-half of an inch long. Two cylindrical wooden pins of one-eighth of an inch in diameter, but without screws, are fixed at each end of the same board, and in the same transverse lines with the other pins, but near the sides, as at G H I J. They stand parallel to the other pins on the same surface of the board, and are about a quarter of an inch shorter. These pins pass through six holes in the board, fig. 1, when both boards are applied together. Two flat nuts of boxwood, K and L, fig. 1, of five-eighths of an inch in diameter each, are screwed upon the middle pins to press the boards together. Each board has one of its faces covered with a cushion, consisting of six or eight pieces of flannel slightly stitched together, with perforations for the pins to pass through. The cushions are covered with a mole-skin laid with the fur outwards, and the hair of each inclining to the edges of the boards, in the direction of the arrow in fig. 1; holes being also made through the skins to admit the pins. When the boards are screwed pretty close together by means of the nuts, with the cushions between them, the redundant edge or border of each skin is glued to the edge of its respective board, and they thus completely form two elastic cushions. A piece of brass wire, about one tenth of an inch in diameter, is bent into the form M N O, fig. 2, and fixed upon the inner side of this board, which is faced with the fur, and in the situation represented in the drawing, by means of two screws which pass through pieces of flat brass, soldered on the wire. To the parts M S O T of this wire, a tin tube is soldered, open at both ends, and fitted to receive the lower part of a very thin ounce or ounce and half phial or jar, coated on both sides with tin-foil, excepting $1\frac{1}{4}$ of an inch of its top. This phial, when placed firm in the tin tube, has its wire parallel to, and $1\frac{1}{2}$ inch from, the side of one of the boards, and likewise in the same plane with that produced by the contact of the two cushions. The wire proceeding from the inside coating of the phial is fixed firmly in its neck, by means of a narrow silk ribband dipped in melted electric cement, and rolled lightly round the same wire to a proper degree of thickness. The space U V X W, fig. 1, is without tin, and is rather more than half the circumference of the tube. Thus the charged phial may be removed from the machine, without the hazard of discharging, by pushing it forward a little with the thumb applied against its bottom, and then taking hold of it by its coating. R is a curved wire, with a ball to it, which by moving on E as a centre, and standing at any angle, it is raised to answer the end of an electrometer and discharger of the phial. The figure of an arrow is made on the

the board, fig. 1, with its point in the same direction to which the fur of its cushion inclines, in order to shew the proper application which is also secured by the pins. The ends of a silk ribband, two inches broad, and a yard or 32 inches long, are neatly sewed together with silk, and the ribband afterwards steeped in weak seed-oil varnish. Inclose the ribband betwixt the cushions, the arrow pointing towards the wire of the jar. The machine being exposed to the moderate warmth of a fire for ten or twelve minutes, especially in damp weather, and the cushions made to press gently against the ribband, set the balls a quarter of an inch from each other, then hold the instrument by the boards in the left hand, and with the right hand, at a pretty good distance from the wire in the phial, draw the ribband gently, keeping the silk under the wire of the jar, and in contact with it. At six revolutions of the ribband, the phial will generally discharge itself at the above-mentioned distance of the balls. Twelve revolutions of the ribband, when the excitation is powerful, and the balls separated at a considerable distance, will produce a charge of the jar, which few persons would choose to receive a second time. Mole-skin, with the hair on, excites silk, or the resinous plate of an electrophorus, better than the fur of any indigenous animal the writer of this has tried: this he discovered by accident. The machine may be carried conveniently in the pocket in an oval case made of pasteboard.

This instrument is not to be considered solely as a philosophical plaything; it may be used when a common electrical machine is not at hand, as an auxiliary in recovering persons apparently dead by drowning, and other kinds of suffocation; and when the unfortunate objects are at some distance, it will be found not a little convenient, on account of its portable size. It may likewise be very advantageously applied in some cases of menstrual obstruction, and in several morbid affections where small shocks are indicated. In suspended animation from submersion, shocks of a proper degree of strength may be given conveniently by this instrument in the following manner, passing each charge through the breast, at the moment the lungs are expanded by an assistant. Apply the knob of the charged phial, held by its coating in one hand to the region of the heart of the patient, at the same time that a finger of the other hand is in contact with the spine. To avoid the shock which the operator receives in this case, the electrical circuit may be completed by means of a portable conductor formed of a gold, silver, or copper thread, neatly inclosed in a silk ribband, except its ends. When the phial is about to be discharged, perhaps it may be right not to suffer any part of the patient's body to come in contact with this conductor, but the spine; and this may be effected by means of an assistant, and one hand of the operator.

III.

On the Corundum-stone from Asia. By the Right Honourable CHARLES GREVILLE, F.R.S.

(Concluded from page 544, vol. II.)

WE frequently see small rhombs traced on the surface of the planes, on the ends of the hexaedral prism, fig. 10, Pl. XXIV, vol. II. This, no doubt, is occasioned also by the intersection of the laminae, on the planes of the primitive rhomboidal parallelopiped. But these rhombs,

rhombs, formed by the reunion of lines that join in angles of 60° and 120° , instead of 86° and 94° (like those we have seen traced on the faces which correspond with those of the rhomboidal parallelopiped), form angles of 60° and 120° . It would, therefore, be an error to consider them as indications of the form of the elements of crystallization, as we are tempted to do from a simple inspection of the crystal. These same lines form equilateral triangles with one another, as may be seen in fig. 10.

The cause of these small equilateral triangles, which sometimes project a little over the planes on the end of the prism, must now be obvious. If, during the superposition of the crystalline laminæ on all the planes of the rhomboidal parallelopiped, it has happened from any cause whatever, that the laminæ deposited on the three faces of the same summit, have not fallen exactly on those which preceded them, or that they have experienced some deviation, or have not had the same decrease, as all the others, at the angle of 86° , these triangles must necessarily occur; in the same manner it must be obvious, why these small equilateral triangular projections are frequently placed on one of the sides of the crystal.

The primitive form of the corundum crystal is therefore a rhomboidal parallelopiped, whose solid angle at the summit is $84^\circ 31'$, and that formed by the reunion of the bases is $95^\circ 29'$.

The crystalline laminæ are rhombs of 86° and 94° : these, in my opinion, are double crystalline molecules; the single molecules I apprehend to be isosceles triangles, of 86° at the angle of the summit, and of 47° at those of the base*.

Although the rhomboidal parallelopiped of 86° and 94° is the primitive form of the corundum crystal, yet it is rare to meet with that substance under this perfect and determined form; and in most mineral substances, it is more rare to meet with their primitive crystals, than their different modifications. Amongst Mr. Greville's numerous specimens of corundum, I have met with only one which has this primitive form, and it is doubtful whether even this may not be a fragment.

The corundum crystal presents another modification, under which the regular hexaedral prism, instead of having three alternate solid angles, at each of its ends (on which solid angles are placed isosceles triangular planes, forming a solid angle of $122^\circ 34'$ with the planes at the extremities upon which they are inclined), has also its angles supplied by isosceles triangular planes; but these planes, instead of $122^\circ 34'$, form solid angles of $160^\circ 42'$ with the said planes on the extremities. (See fig. 11 and 12.) These new planes, which constitute a new modification of the primitive form of corundum, are the result of a different order, in the decrease of the laminæ; which, in the primitive form, are deposited on the planes of its primitive rhomboid by single rows of crystalline molecules, and increase the planes which ter-

* I am at present preparing a work, in which I shall, if circumstances permit me to finish it, give the result of my observations, and my own opinion on this interesting part of mineralogy. I shall only observe here, that although double molecules, square and rhomboidal, are frequently formed in the process of crystallization, yet the real form of the crystalline molecules seems to be triangular. By observing the progress of the rhomboidal parallelopiped, in its passage to the form of an hexaedral prism (fig 4 and 5), and by considering the prism terminated, it seems evident, that the last laminæ which had been deposited, after the progressive decrease in the rows of crystalline molecules to one single molecule, must necessarily have been triangular. B.

minate the hexaedron: whereas, in this second modification, the decrease of molecules is by two rows, which gives a more obtuse inclination, and forms new planes. The surface is usually striated, parallel to the sides of the planes which terminate this crystal; an appearance always announcing imperfection in the crystallization, arising either from a change in the order of decrease or increase, or from a less perfect union of the crystalline laminæ. A section would shew gradual risings or steps, as appears in fig. 14, which is a section of fig. 13, in the line A D B. These striæ are not to be confounded with those in numberless substances, as in tourmalines, schorls, &c. which arise from the longitudinal union of numberless distinct crystals. The crystal resulting from this new mode of decrease in the crystalline laminæ, will represent one or other of the varieties shewn in fig. 11, 12, and 13, according to the period when such decrease has begun in the process of the crystallization; and, if it has begun very late, the new faces will only be small, nay, almost imperceptible, isosceles triangles, forming solid angles of $160^{\circ} 42'$ with the planes of the extremities of the prism, as in fig. 5; the measure of the angles however must be excepted.

If this irregular mode of decrease had begun with the first crystalline laminæ, which were deposited on the primitive rhomboidal parallelopiped, the hexaedral prism resulting therefrom would have been terminated by two very obtuse triedral pyramids, whose planes would have been rhombs; and they would have been placed in a contrary direction to each other, as may be seen in fig. 12, by the dotted lines. I have not met with this variety, but its existence may be supposed.

It happens sometimes, that the crystallization has not been so perfect as to destroy every appearance of the faces of the primitive rhomboidal parallelopiped; in this case there remains, on the solid angle of 112° , formed by the junction of the new faces with the edges of the prism, a small isosceles triangle, as in fig. 13, which corresponds to those in fig. 5, of the preceding modification.

The crystals which explained the second modification, form also a part of Mr. Greville's collection: one in particular, is highly worthy of notice; it is the most perfect crystal I have ever seen of this substance. The surface of the faces of the prism, although rough, is infinitely less so than that of the others, and much more brilliant. The planes on the ends have the usual polish of crystals; its colour is a pale red, and its transparency may be compared to that of wax.

This substance presents a third modification, in which the hexaedral prism diminishes in diameter, as is apparent by comparing the diameter of its two ends; in some, it appears like a regular hexaedral pyramid truncated (fig. 15). The crystals of this modification are usually irregular, and seldom admit of a certain measure of their angles; but among the numerous specimens, in Mr. Greville's collection, I have been able to ascertain, in the greater part, that the hexagonal plane at the top, forms angles of about 120° , with the planes of the pyramid; and the hexagonal plane at the base, forms angles of about 78° with the planes of the pyramid. In other instances, the form of the pyramid varies greatly; in some the angle at the upper plane was 110° , and the angle at the base about 70° ; in others, the angle at the upper plane was about 100° , and the one at the lower plane about 80° .

In

In these three varieties, the crystalline laminæ can be separated, as in the hexagonal prism, at the three solid alternate angles of each end, but in a contrary direction to each other. The planes which appear when the laminæ are detached regularly, form solid angles of $22^{\circ} 34'$ with the planes of the extremity: this arrangement is analogous to that of the hexaedral prism. The difference of form arises from the crystalline laminæ deposited on the planes of the primitive rhomboid, decreasing by more than one row of molecules, on the planes of one of the triedral pyramids of the rhomboid, and by less than one row, on the planes of its other pyramid. This general observation, on the manner in which this primitive crystal of corundum passes to the different varieties just mentioned, is the only one I have established with any degree of certainty at present. Specimens with perfect crystals, whose angles may be measured with accuracy, will probably arrive from India, and give further demonstration, as to these and other varieties of modifications of corundum. We may conceive, that if in this modification the crystallization had ceased before the entire formation of the crystal, there would have remained small isosceles triangular planes, on three of the alternate solid angles, formed by the junction of the planes on the ends, with the edges of the truncated pyramid. These isosceles triangular planes resemble those we have seen in the first modification (fig. 4 and 5), and form, in the same manner, solid angles of $122^{\circ} 34'$ with the planes on the ends of the prism (fig. 16.)

Finally, if during the formation of the crystal, in this modification, it should happen that the laminæ deposited on the three planes of the rhomboidal parallelopiped, on the side where they undergo a greater decrease, do not undergo the decrease of one row of molecules at the acute angle of the summit, the crystal will be a real hexaedral pyramid (fig. 17), whose acute angle at the summit, measured on the sides, will be nearly 24° , in one of the varieties; 40° for the most obtuse; and 20° for the most acute variety: the angle of their triangular planes, in the first instance, $13^{\circ} 41'$; in the second, $22^{\circ} 20'$; and $11^{\circ} 28'$ in the third. I have not seen any perfect pyramids; but in many the hexagonal plane terminating the pyramid is so small, that it renders its total suppression probable.

This decrease necessarily produces a single pyramid, as above mentioned; nevertheless, there are instances of crystals of corundum, belonging to the variety where the terminal planes make, with the planes of the pyramid, a solid angle of about 100° , in which two pyramids of the same dimensions, having their summit replaced by a small hexagonal plane, are placed base to base.

I have also observed among the crystals of the obtuse variety above mentioned, in Mr. Greville's collection, an instance of the decrease taking place by several rows, on one three-sided pyramid of the primitive rhomboid, and by single rows on the other. Consequently, the crystal is a short regular hexaedral prism, terminating on one end only by an hexaedral pyramid; the planes of which, as well as of the prism, are alternately broad and narrow, and almost perfect, its apex being replaced by a very small plane.

I shall conclude, by mentioning a variety of corundum, described by the Abbé Hauy, in the *Journal des Mines*, No. 28; in which the edges of the terminal planes of the hexaedral prism are replaced by planes which form an angle of $116^{\circ} 31'$ with the terminal planes; but in the numerous collection of Mr. Greville I have not seen this variety. One crystal had an appearance

pearance of such planes; but, on examination, it was clearly accidental. The authority of the Abbé Hauy in crystallography, is so great, that the existence of such modification ought not to be denied without further examination; though I cannot, in this instance, adopt it: he derives this variety, which he calls subpyramidal, from a decrease of three rows of molecules, at the angles of the base of the two pyramids of the primitive rhomboid; and he seems to attribute the same formation to the pyramidal variety with double pyramid, which he supposes may exist.

The primitive crystals, and the first and second modifications of corundum, are from the peninsula of India. The third modification, or the pyramidal variety is from China; nothing approaching this form being among the specimens which Mr. Greville received from the peninsula of India.

The preceding observations, and particularly the last-mentioned modification of corundum, compared with the best description of the sapphire, suggest the further examination of the degree of connection, if not of identity, of these oriental stones.

In both, the hexaedral pyramids are usually incomplete in their apex, and they vary in acuteness. I have stated the degree in which the solid angles of the pyramid (taken as complete) vary, in corundum, to be from 20° to 40° .

Romé de L'Isle states, that the sapphire varies from 20° to 30° . The Abbé Hauy (Journal de Physique, Aug. 1793) mentions two varieties of the sapphire, one measuring at the solid angle of the pyramid $40^{\circ} 6'$, the other $57^{\circ} 24'$. I never saw a sapphire with so obtuse an angle as the last, but many whose angle at the top, if the pyramid had been complete, would have been the same as that of the corundum. Besides the analogy between the crystals of corundum and the sapphire, by the union of two hexaedral pyramids at their base, it also exists by the measure of their angles; and both substances are subject to the same irregularity, sometimes appearing as a single hexaedral pyramid, and sometimes as an hexaedral prism: moreover, the sapphire sometimes has on its solid angles, alternately, the same triangular planes, (fig. 5.), and also the prominent triangles on the planes of the extremities (fig. 10.), which often appear in the crystals of corundum. The Abbé Hauy, in the Journal de Physique, August 1793, names this variety, *orientale enneagone* which is represented in the annexed plate (fig. 18.), and says, that the small triangular planes make with the terminal planes an angle of $122^{\circ} 18'$; and, in the description of the same triangular planes in the corundum (fig. 16.), it appears that these planes are the remains of the planes of the primitive rhomboid, and form, with the terminal planes, an angle of $122^{\circ} 34'$.

Perhaps the rhomboidal crystal, which Romé de L'Isle had given as one of the forms of the sapphire, should be restored to it. He had examined it at M. Jacquemin's, jeweller to the crown (Cristallographie, 1 edit. p. 221), and he suppressed it in his second edition, but often expressed to me his regret in having made the alteration. I have before me a letter from that celebrated naturalist, dated September, 1784,* in which he inclosed, for my opinion, a copy of a letter he had received from Mr. Werner, with models of some crystals; among them two called by him rubies; one a rhomboid, of which the angles of the summit are substituted by planes (fig. 19.), the other is precisely the same as fig. 3, 4, and 5, of the annexed plate.

* A letter to the same effect was written to Mr. la Metherie, and published in the Journal de Physique, May, 1787.

The following is a translation of Romé de L'Isle's words: "The first of these rubies has exactly the same form as I have represented in plate IV. fig. 60, of my *Cristallographie*, viz. a rhomboidal parallelopiped, truncated at each of its obtuse angles, by an equilateral triangular plane."

"You will have a correct idea of the other crystal, if you suppose the crystal, represented in Plate IV. fig. 87, truncated at each of the summits of its pyramids, by an equilateral triangular Plane, as in the preceding modification, but deeper, and in so great a degree, that the three rhombic planes of each pyramid disappear with the exception of three isosceles triangles: this modification differs from the first only by the hexaedral prism, and the deeper truncature at the summits of the pyramids."

It is therefore clear, that if the primitive rhomboid of corundum decreased only at the superior angles of its laminæ, it would exhibit exactly the first of these varieties of Mr. Werner's ruby, as in the annexed fig. 19.

As to the second variety of Mr. Werner's ruby, it is equally clear, if in fig. 87, referred to by Romé de L'Isle (represented by the annexed fig. 20.), no more of the pyramid was left than the three small triangles *b, a, c*, there would be precisely one of the forms of corundum before described, to which the annexed figure 5 belongs.

It may, perhaps, be objected, that the laminæ appear to be parallel to the terminal planes, in the sapphire, and inclined in the corundum. There are crystals of corundum, in which, very frequently, the laminæ appear parallel to the terminal plane; I was at first, and for some time, deceived by that appearance. In other corundum crystals, the laminæ appear to be parallel to the prismatic planes; and to conclude the instances of analogy, the superposition of rhomboidal laminæ is sometimes observable in oriental rubies and sapphires. It was by this appearance, Mr. Greville was led to try the effect of cutting the forementioned stones *en cabochon*; whereby a similar effect of triple reflection, which formed stars of six rays from a common centre, was produced in the oriental ruby, in the sapphire, and in the corundum.

It is to be lamented that Mr. Werner did not send, with his models, the specific gravity of each of the rubies, and the measures of their angles: we should then have had data to decide whether the rubies, sent by Mr. Werner, were, as I suppose them to be, oriental rubies, or sapphires; and, with equal certainty, whether the parallelopiped rhomboid corresponds precisely with that of the corundum: by this the perfect identity or analogy, between the corundum and the sapphire, would have been no longer doubtful.

Table of the Specific Gravity of the Corundum, Sapphire, Topaz, Ruby, and Diamond, on different authorities.

Corundum.			
Hatchett and Greville	-	-	2,768 * Matrix of corundum. Coast
H. and G.	-	-	2,785 * Lump of corundum. Coast
Klaproth	-	-	3,075
Klaproth	-	-	3,710
Blumenbach	-	-	3,808
Briffon	-	-	3,873
Hatchett and Greville	-	-	3,876 * Corone. Bengal
Lichtenberg	-	-	3,908
Groß	-	-	3,935
Hatchett and Greville	-	-	3,950 * Crystal of corundum. Coast
H. and G.	-	-	3,954 * Ditto, with vitreous cross fracture
Coast			
H. and G.	-	-	3,959 * Ruby-coloured. Coast
H. and G.	-	-	3,959 * Chatoyant. China
H. and G.	-	-	3,962 * Crystal. China
Klaproth	-	-	4,180
Sapphire.			
Briffon	-	-	3,130 Brazilian; probably a topaz
Bergman	-	-	3,650 †
Quist	-	-	3,800 †
Bergman	-	-	3,940 †
Klaproth	-	-	3,950 †
Bergman	-	-	3,974 †
Briffon	-	-	3,991 † White oriental
Briffon	-	-	3,994 †
Blumenbach	-	-	3,994 † Blue
Hatchett and Greville	-	-	4,000 †* Greyish star-stone
Werner	-	-	4,000 †
Blumenbach	-	-	4,010 †
Hatchett and Greville	-	-	4,035 † Blue star-stone
Briffon	-	-	4,076 † From Puy-en-Velay
Blumenbach	-	-	4,083 † Crimson
Hatchett and Greville	-	-	4,083 †* Pale blue crystal
Muschenbroek	-	-	4,090 †
Blumenbach	-	-	4,100 † Yellow
La Metherie	-	-	4,200 †
Quist	-	-	4,200 †

Topaz.

La Metherie	-	-	-	2,690	Siberia
Bergman	-	-	-	3,460	
Werner	-	-	-	3,464	Light blue. Brazil
Quist	-	-	-	3,500	
Werner	-	-	-	3,521	Eibensfocher
Briffon	-	-	-	3,531	Red. Brazil
Briffon	-	-	-	3,536	Dark yellow. Brazil
Werner	-	-	-	3,540	Dark yellow. Brazil
Briffon	-	-	-	3,548	Oriental
Werner	-	-	-	3,556	Schneckensteiner
Briffon	-	-	-	3,564	Schneckensteiner
Briffon	-	-	-	4,010 †	Oriental
Bergman	-	-	-	4,560 †	

Ruby.

Bergman	-	-	-	3,180	
Muschenbroeck	-	-	-	3,180	
Quist	-	-	-	3,400	Spinel
Blumenbach	-	-	-	3,454	Ceylon
Quist	-	-	-	3,500	Brazil
Briffon	-	-	-	3,531	Brazil
Klaproth	-	-	-	3,570	
Hatchett and Greville	-	-	-	3,571 *	Octoedral crystal
H. and G.	-	-	-	3,625 *	Macle of Octoedral crystal
Blumenbach	-	-	-	3,645	
Blumenbach	-	-	-	3,760	
Briffon	-	-	-	3,760	
Hatchett and Greville	-	-	-	4,166 *	Salam Ruby. Star-stone. Coast
Quist	-	-	-	4,200 †	
Bergman	-	-	-	4,240 †	

Diamond.

Hatchett and Greville	-	-	-	3,356	Perfect crystal
Wallerius	-	-	-	3,400	
Hatchett and Greville	-	-	-	3,471	Aggregate crystal
Cronstedt	-	-	-	3,500	
Muschenbroeck	-	-	-	3,518	
La Metherie	-	-	-	3,520	
Briffon	-	-	-	3,521	
Werner	-	-	-	3,600	

The Mark * distinguishes the specimens in my collection, to which I have referred in the foregoing paper.

The mark † distinguishes the stones, which, from their specific gravity, I think belong to the genus of corundum.

The generic name corundum, I am in the habit of giving to those sorts which have a sparry, or a granulated fracture. When corundum has a vitreous cross fracture, I call it sapphire; and distinguish its varieties by their colours, white, red, blue, yellow, green; and by the accidental reflection of light from their laminæ: when in one direction, I call the sapphire chatoyant; when the reflection is compounded of rays which intersect each other, and appear to diverge from a common centre, I call them star-stones, as red, blue, or greyish star-stones, or star sapphires.

IV.

Experimental Researches concerning the Principle of the lateral Communication of Motion in Fluids, applied to the Explanation of various Hydraulic Phenomena. By Citizen J. B. VENTURI, Professor of Experimental Philosophy at Modena, Member of the Italian Society, of the Institute of Bologna, the Agrarian Society of Turin, &c.

(Concluded from page 494, vol. II.)

PROPOSITION XI.

If the water of a reservoir, which flows through an horizontal aperture, be influenced by any foreign motion, it will form an hollow whirl above the orifice itself.

CITIZEN Bossut, has given a very good description of this kind of eddy*. It is of a different nature from those considered in the foregoing proposition; but the causes of both are, in some respects, similar, for which reason I propose to attend to them more particularly in this place.

Let D Q fig. 18 (plate XXII, vol. II), represent an horizontal plane near the orifice E F, through which the fluid of the reservoir M N flows. A fluid particle D, situated in this plane, has a motion D B, inclined to the axis A B. This motion may be decomposed into two, D C, C B; let us suppose that plane D Q to descend parallel to itself along the axis, with the motion C B; the motion D C of the particle D on the plane D Q, remains to be examined. This motion impresses upon all the particles, situated in the plane D Q, a centripetal force, towards the centre C.

Let any other horizontal motion whatever, not coincident in direction with D C, be impressed upon the same particles: Under the government of these two forces, the particles will describe round the centre C areas proportional to the times, and by the equilibrium of these motions, they may assume an horizontal circular rotation.

Let us imagine, that during this horizontal circulation, the particle D, in its approach toward the centre C, as in a spiral, shall describe circular orbits, of which the diameter is successively diminished; let us call the velocity of rotation of the particle $D=v$; its distance from the centre $=r$; the time of one revolution $=t$; and since the areas must be as the times, we shall

* Hydrodyn. No. 432.

have nearly $v = \frac{1}{r}$; $t = r^2$; and the centrifugal force of the particle D will be $= \frac{1}{r^3}$. When we attentively observe the particles which revolve at the surface of the funnel, at M N, we see that the effect which really takes place in nature, is nearly $t = r^2$. Since, therefore, the centrifugal force in approaching the centre C increases as $\frac{1}{r^3}$, it will become equal to forming an equilibrium against the upper pressure S D, which produces the centripetal force D C. A cavity, K R T H P v, will therefore be formed, round which the whirling fluid will support itself by the centrifugal force of its rotation.

Let D Q P R represent a circular fluid zone, the particles of which turn round the cavity R P, according to the law here indicated. Let the gravity of a fluid particle be $= g$; C R $= a$; R D $= b$; D X $= z$; X Z $= dz$; and the velocity of the particle D $= v$. If the centrifugal force of the particle D were equal to its gravity, its velocity, by the theorems of Huygens, would be equal to that of a body falling by gravity alone, through the space $\frac{a+b}{2}$. And since an heavy body falls in one second through the space of 181 inches $= S$, the velocity of the particle D on the same supposition would be $= \sqrt{2 S (a+b)}$. The centrifugal force in the circle is as v^2 ; the centrifugal force of D will therefore really be $= \frac{v^2 g}{2 S (a+b)}$. And

since the centrifugal force is $= \frac{1}{r^3}$; taking $\frac{1}{(a+b)^3} : \frac{1}{(a+b-z)^3} = \frac{v^2 g}{2 S (a+b)} : a$ fourth term, we shall have the centrifugal force of the element of D X in $X = \frac{v^2 g (a+b)^2 dz}{2 S (a+b-z)^3}$;

and that of the filament D X $= A + \frac{v^2 g (a+b)^2}{4 S (a+b-z)^2}$. When $z = 0$ the integral is $= 0$; whence

$A = \frac{-v^2 g}{4 S}$. Taking $z = b$, the centrifugal force of the filament D R will be $= \frac{bgv^2}{4 a^2 S} (2 a + b)$. The quantity bg is the gravity itself of the filament D R. The gravity of this filament is therefore to its centrifugal force $= v^2 (2 a + b) : 4 a^2 S$.

When the fluid zone, D R P Q, is nearer the aperture E F, the pressure S D increases; whence the centrifugal force of the zone must also be increased by diminishing the radius of the cavity R C: hence we may determine the nature of the curve which forms the perpendicular section of the cavity K R T. For greater simplicity, let us suppose that the sides of the vessel have the same form M D as that of the cavity itself, so that D R $= b$ may be constant. Let A C $= x$; and C R $= y$. Let us substitute y instead of a in the preceding formula. And since the gravity of the filament D R, is to the gravity of the filament S D $= b : x$, we shall have by composition of ratios, the centrifugal force of the filament D R, to the pressure S D $= b^2 v (2 y + b) : 4 x y^2 S$. These quantities must be equal, in order to afford an equilibrium.

We have therefore $x y^2 \frac{b v^2 y}{2 S} = \frac{b^2 v^2}{4 S}$ for the equation of the curve K R T. This is the sixty-fourth species in the enumeration of lines at the third order, by Sir Isaac Newton. Its convexity is turned towards the axis; it has two asymptotes, one of which is the axis A Y, and the other is in M N, supposing the two points M N to be infinitely distant.

If the assumed positions in this theory do not absolutely coincide with nature, they approach its effects very nearly. It is not only possible, but there does exist in nature, a whirling stream, of which the cavity turns its convex part to the axis, and in which $t \propto r^2$ very nearly, as is shewn by experiment.

Experiment xxvi. Let the orifice E F be opened, and any motion whatever be impressed on the fluid, independent of that which its gravity, and the pressure of the circumambient particles, tend to produce; the turning immediately begins, and is seen to be more rapid in those parts of the fluid which are nearest the bottom. The cause of this is, that the motion D B is more convergent and perceptible in those parts which are nearest the orifice E F*. The centripetal force D C, produces its effect there rather than at the upper parts. These last afterwards fall into the cavity which begins to be formed below, by which means they also acquire a centripetal force, and the funnel or cavity opens to a much greater height, than that in which the convergence of fluid filaments is observed towards the orifice E F, in water which is less agitated.

Experiment xxvii. Place a floating body at the surface of the fluid, of sufficient magnitude to prevent the formation of the cavity. If the fluid be much agitated, the cavity will take place at the lower part, and air will introduce itself through the opening E F. Whence it follows, that the pressure of the atmosphere on the upper surface of the fluid is not the cause of the cavity, which assumes the shape of a funnel. The air does not enter but because it finds an empty space formed by a centrifugal force.

Experiment xxviii. When the fluid remains in a state of tranquillity without eddies, the vessel empties itself in forty seconds; but when the circular motion takes place, the evacuation is accomplished in fifty seconds, more or less. It cannot, therefore, be said in general terms, that the whirling stream absorbs and draws down bodies through the opening E F with more force than if no such circulation took place.

Experiment xxix. Pour a stratum of oil upon the water of the vessel. As soon as the funnel forms itself, the oil rushes down, and issues out before the greatest part of the lower water, upon which it rested. The portions of oil partake less of the rotation of the lower water; having less density, they likewise recede less from the axis than the water; in consequence of which, as they occupy the interior part of the funnel, and are unsupported, they flow out first.

Experiment xxx. Every other small body which floats on the water in the vessel, acts in the same manner as the oil, provided its dimensions be very small. If the volume of the body be somewhat greater, while it approaches the cavity, to fall therein, its extremity, which is nearest the axis, comes into a place where the circulation is more rapid. This rapidity of motion impressed at one extremity of the floating body, is transported by the laws of mechanics, to its centre of gravity, which is more remote from the axis, in a situation where the circular motion is slower, consequently the body recedes from the edge of the cavity into which it was about to fall. It returns a short time afterwards, is again repelled, and these alternate motions continue as long as the circumstances which produced them. Lastly, if the body which floats

* Bernouilli. Hydrot. sect. 4, §. 3. Bossut, art. 427.

at the surface of the liquor after the funnel has been formed, be of sufficient size to cover the whole cavity, it destroys the funnel in the upper part, and sometimes also in the lower. The reason is, that the body itself cannot turn round its centre but according to the law $v=r$; it therefore destroys by friction the law $v=\frac{1}{r}$ in the parts of the fluid in contact with it, and consequently it destroys the funnel itself.

PROPOSITION XII.

The lateral communication of motion takes place in the air as well as in water.

The stream of air which moves in the midst of a body of air at rest, produces undulations and eddies round its current in the same manner as in water. These may be observed in the smoke which rises from a furnace, and produces a remarkable aspect, when it issues like a dark tree from an agitated volcano. They may likewise be seen in the particles which float in an obscure chamber, when a ray of the sun shines in, and the observer blows through them.

If a general wind comes, for example, from the south, it frequently happens that the north side of a mountain is at the same time struck by a north wind. This partial and local wind is nothing but the eddy produced by the mountain itself acting as an obstacle, against the principal wind, from the south. It is probably from the same reason, that the wind sometimes acts in the contrary direction on the sails of a vessel, when they are too obliquely presented to its stream.

The vapour of water which issues from the eolipile carries the surrounding air with it, and drives it against the burning coals opposite to the stream of aqueous vapour. It must not, therefore, be concluded that the aqueous vapour is itself in this case decomposed to maintain the combustion of the charcoal.

It is known that the flues of chimnies assist the rising of smoke by their figure; concerning which, we have drawn some inductions, in the seventh proposition.

In organ pipes, the air which issues out of the side opening (*lumiere*) rubs laterally against the extremity of the column of the air included in the pipe. It rubs it on one side in the longitudinal direction, and is, as it were, an elastic file, acting upon an elastic surface. Though the column of air be fluid, its parts are, however, so far intermixed together, that the tremulous motion excited at the place of friction, is soon communicated laterally through the whole thickness of the column, which receives vibrations of such a kind, that they are an equilibrium with each other, and with the velocity of the stream which affords the friction. For this effect, it is requisite that the column should divide itself at different points or nodes distributed through the length of the tube*. It is by repeated actions that the wind which issues from the side aperture impresses at length upon the whole column contained in the pipe, a movement of vibration greater than that which the laws of impulse, and of the lateral communication, would permit it to make by a single impulse. In the hautboy, and other similar instruments, having a mouth-piece, or reed, the cause which excites the tremulous motions, does not act sideways on the air contained in the pipe, but strikes the column directly in the middle: for which reason, it communicates its vibrations with so much the more effect to the whole mass.

* *Memoires de l'Acad. an. 1762, page 431.*

In like circumstances the force of sound, which is propagated in the atmosphere, depends on the magnitude of the section of the air which is at the extremity of the pipe, and the amplitude of the vibrations of this section. It is this surface which strikes the atmosphere, and communicates the pulsations*. For this reason, conical divergent pipes afford a stronger sound than those which are cylindrical; and these last afford a stronger sound than pipes which are conically convergent. The first cause of the sound which acts at the mouth end of the pipe would never, of itself, excite such strong pulsations in the atmosphere, as it does excite by the lateral communication in the air contained in a divergent conical pipe.

The explanation of this phenomenon may be understood by observing, 1st, That if a number of elastic bodies be disposed in progression, the first will impress upon the last, by the intermedium of the others, more velocity than would be communicated by the immediate stroke. 2. The vibrations excited in the pipe, have a certain permanence which permits them to receive an increase of force by the united effect of successive impulsions; whereas, in the open atmosphere every pulsation is transient and alone.

Is not the augmentation of sound in the speaking trumpet, in part owing to the same cause of the lateral communication of motion, rather than to the mere reflection of the sonorous lines from the sides of the tube itself?

I call those *resonant* vibrations, which take place in a tube when sound is excited; and I call those *propagated* vibrations, or pulsations, which transmit the sound through the atmosphere. I have already pointed out a difference, which appears to me to take place between these two kinds of vibrations; namely, that the first have a certain permanence and connection with each other, so that the latter excites, supports, and reinforces the former; whereas, the pulsations which succeed each other in the atmosphere by the repeated action of the resonant body are single, and independant of each other.

But the following is a much more remarkable difference between these two kinds of vibrations. When at the extremity of a pipe A B C, a resonant vibration is made in the section of air, B C, fig. 2. Plate VIII, vol. II. experience shews that this vibration becomes the centre of pulsations propagated all round in P S Q. For on whatever side the observer is placed, whether at P or at Q, he will hear the sound of the pipe A B C nearly as much as at S. But when there is no pipe, and the vibration at C B is a simple pulsation propagated through the open air from A to B, in this case, the pulsation is not propagated laterally and completely to P and Q like the resonant vibration; but is contained almost entirely in the limits B Z and P Y, with a divergence of between 15 and 20 degrees. This fact has been disputed by various philosophers; but it cannot be questioned, since it is well known that we do not hear the echo, or reflected sound, from a plain surface, unless we place ourselves in the line of reflection, or very near it. If the pulsation of the echo were propagated all round, before the reflecting surface, diverging from thence as centre, ought we not to hear the echo in every situation whatever before that reflecting surface? We must therefore admit, with regard to sonorous pulsations propagated in the atmosphere, certain exceptions, and even limits, with regard to the lateral communication of motion which we have pointed out in the first proposition, and in the fifth, with regard to water.

* It is known that the material of which a pipe is made does not perceptibly affect the sound.

Addition respecting the contracted Vein.

Much has been written concerning the convergent directions assumed by the particles of a fluid contained in a vessel, previous to their being emitted through an aperture in the side of the vessel itself, and concerning the form of the contracted vein which is thus produced. The reflections and experiments, which I shall proceed to give, may afford some farther explanation in this respect.

I shall begin by defending the fundamental doctrine of hydraulics against the opinion of a learned man, distinguished by his labours and his zeal for the advancement of science: Lorgna, the founder of the Italian society. He pretends* that the contracted vein is nothing else but the continuation of the Newtonian cataract, and that the celerity of the fluid, issuing from an orifice in a thin plate, is much less than that of a body which falls from the height of the charge.

Let M D, fig. 22, Pl. XXII, vol. II, represent the axis of the vein which issues from B. The radius of the circular orifice B C = B D = 1; M B = a . Lorgna pretends that $0,472 a = H B$, is the height which would produce, in an heavy body, the velocity of efflux in B C; he supports this proposition by computations deduced from the mutual action of the particles of the fluid contained in the vessel. But after having seen the failure of the efforts of the greatest geometers on this subject, we ought to mistrust all these demonstrations founded on mechanical principle, very true in themselves, but of which the application to an infinity of bodies, which move and are pressed in every direction, becomes extremely difficult, if not impossible. Let us see whether the theory of Lorgna agrees with experiment. Supposing the velocity of the fluid at B, arising from the elevation $H B = 0,472 a$, the velocity of the same fluid in D will be increased in the ratio of $\sqrt{HB} : \sqrt{HD}$; and the vein in D will be contracted in the same ratio: whence $DE = \sqrt[4]{\frac{0,472 a}{1 + 0,472 a}}$; which is the formula of the hyperbolic conoid of Newton. If this be the sole cause of the contraction, the dimensions of D E ought, very nearly, to agree with this figure when examined by experiment. But they, in reality, differ from it very much, as may be seen in the following table.

Authors of the experiments.	Value of D E found by actual measurement.	Value of D E calculated by the preceding formula.
Poleni (de Castellis § 35.)	. 0,79 .	. 0,97
Michelotti (Sperim. Idraul. tom. I. exper. 46; tom. II. exper. 4) 0,80 .	. 0,99
Boffut (Hydrodyn. art. 437 exper. 5) 0,818 .	. 0,99
Myself, with 35 inches charge and an horizontal circular orifice of 18 lines in diameter	. 0,798 .	. 0,984

* (1) Mem. della societa Italiana, vol. IV.

It is evident that the contraction of the vein, as found by experiment, is incomparably greater than can be produced by the acceleration of gravity, even in descending streams. But what can we say of horizontal and ascending jets, in which, assuredly, the acceleration of gravity does not take place, but in which, nevertheless, the contraction is observed nearly in the same manner as in descending currents? The contraction of the stream is, therefore, very different from the Newtonian hyperboloid.

Desirous of proving that the vein does not possess the whole velocity arising from the height of the fluid above the centre of the orifice, Lorgna relates the experiments of Kraft*, which are not applicable to the question, because they were made with cylindrical pipes; and we have seen, that such pipes always destroy part of the velocity of the fluid; consequently, we cannot establish any rule from them which shall apply to orifices through thin plates†. He wishes not to determine the velocity of ascending jets by the height to which they rise, because he is apprehensive that the preceding part of the stream or jet is urged, and supported by the succeeding part nearly to the height of the charge. Nevertheless, if we interrupt the jet all at once, the last portions of water fly to the same height as those which preceded them, without having any continued column of the fluid below to follow and support them: these last portions must consequently have received, at their passage through the orifice, all the velocity which was necessary to raise them nearly to the surface of the fluid in the reservoir.

Let us confine ourselves, if it be thought proper, to horizontal jets; the experiment, which I have related as a term of comparison, appears to me to be decisive. Under the charge of 32,5 inches the vertical line PM, fig. 1, Pl. VIII, vol. II, being 54 inches, the horizontal line MN was always 81,5 inches, which was only 2 inches less than it would have been if the jet had preserved in the direction of the horizon, all the velocity which a heavy body acquires in falling from the height of 32,5 inches. The diameter of the contracted vein was 14,3 lines very nearly. Since the quantity of 81,5 inches in MN supposes in the contracted vein a velocity of 149,5 inches per second; this number multiplied by the area of the contracted vein itself, gives the expenditure of 4 cubic feet in 41 seconds of time, which is also the result of experiment. We have, therefore, three measures determined by experiment, which agree and mutually confirm each other; namely, the quantity MN, the contraction of the stream, and the time of expenditure. And since the quantities observed by Bossut, Michelotti, and Poleni, give nearly the same results, it can no longer be doubted; 1, That the contraction of the stream is nearly 0,64 of the orifice; 2, That the velocity of the contracted view is nearly the same as that of a heavy body which may have fallen through the height of the charge.

These two experimental principles are true in all cases where the orifice is considerably small in proportion to the section of the reservoir, where that orifice is made through a thin plate, and the internal afflux of the fluid filaments is made in an uniform manner round the orifice itself. But what would be the consequence if this internal afflux should be modified in a manner different from what usually happens? The following experiments were made with the in-

* Acta Petrop. vol. 8.

† Torricelli took notice of this difference at page 168, of his works, "Quotiescumque autem aqua per tubum latentem decurrens per angustias transire debuerit, falsa omnia reperies."

tention of ascertaining some of the most remarkable effects of these particular modifications in the direction of the fluid filaments, which press each other in order to pass through the orifice.

Experiment xxxi. In the orifice $A C B D$, fig. 21, Pl. XXII, vol. II, the two sides $A B$ are parallel to the horizon; the extremities $C D$ are rounded: the width of this aperture is less than two lines, its length 18 lines, and the charge 32,5 inches. The section of the stream, which issues from this orifice, first assumes the form $E F$; after which, the two extremities $E F$, approaching nearer and nearer to swell the middle part of the section of the stream, at 4,5 inches distance from the orifice, acquires the quadrangular form $G H$. The stream afterwards extends itself in the perpendicular direction in the form of a large fan $K L$.

I have repeated the experiment by placing the longitudinal axis of the orifice $E D$ vertically. In this case the same phenomena were produced; $E F$ becoming vertical, and $K L$ horizontal, both preserving their form.

The fluid filaments which, issuing out of the orifice, pass near the two opposite borders A, B , are very near each other, being convergent, tend to unite at a very short distance from the orifice itself. The filaments C, D , are more remote, and, perhaps, less convergent; they cannot unite but at a greater distance than the two former. In this case, therefore, there are movements which tend to form two contractions, the one nearer, and the other more remote from the orifice. These two contractions counterbalance each other in part; their mutual opposition carries the effect $G H$ to a distance five times greater than that of the contracted vein of a circular orifice, having a diameter of the same breadth as that of this orifice.

In this experiment we see the cause of a phenomenon, which has been observed in some particular cases by Poleni and others, without giving the explanation. In every orifice of a right-lined figure through a thin plate, the angles of the contracted vein answer to the sides of the orifice, and the contrary. When the quadrangular orifice has the situation $M N O P$, the greatest contraction of the stream is made at a greater distance than in a circular aperture; it assumes the form and situation $Q R S T$. The reason is, that the opposite angles, M, P , are more remote from each other than the sides I, V , whence the same thing happens as in the long orifice $A C B D$. In the same manner, the triangular orifice in the situation X produces a contraction of the form and in the situation Z , &c.

Experiment xxxii. The orifice being the horizontal cleft $C D$, fig. 21, the place $G H$, or most contracted point of the stream, was found to be distant from the orifice as in the following table:

Height of the charge above the orifice $C D$.	Distance of the greatest contraction $G H$.
Inches.	Lines.
32, 5	53
18	48
10	40
6	36

The long orifice $C D$ exhibits to us, under an enlarged dimension, the distance of the contracted vein from the orifice. By this means, the foregoing table shews us, in a very sensible manner,

manner, that the contraction of the stream takes place at a greater distance under strong charges, than in those which have but little elevation.

Experiment xxxiii. To the centre of the circular orifice A B, fig. 23, formed in a thin plate, I disposed, within the reservoir, the cone of metal D G E, with a cylindrical part C F G D, in such a manner that the cone was moveable along its own axis I V, and its summit, E, could be protruded more or less through the orifice A B, approaching or receding from the point V. The measures in lines were A B=18; I E=24; D G=27; C D=8. This apparatus was applied to the orifice P, fig. 1, Plate VIII; the charge being 32,5 inches. The results were as in the following table.

Quantity E X. by which the summit of the cone projects beyond the line A B.	Distance of the con- tracted vein.	Distance of M N.	Time of expenditure of 4 cubic feet.
Lines.	Lines.	Inches.	
11,1	9,1	76	85"
6,6	12,3	77,5	53
0	14	78,5	43
The cone removed.	14,3	81,5	41

I intend to repeat and vary this experiment, in order to discover the cause of the singular phenomena which it presents.

Experiment xxxiv. The orifice being a semi-circle, Plate XXII, fig. 24, vol. II, having the diameter A B, 11,2 lines, I applied within the vessel a plane Q A B, perpendicular to the plate in which the orifice was made. The line A B was perpendicular to the horizon, and the charge 32,5 inches. The jet deviated in the horizontal direction in P F G, departing from the axis C E towards the side on which the plane Q P was placed. The angle F C E was $9^{\circ} 5'$, and the angle F C G was 36° . The vertical section of the jet had the form K L, so that the largest part of the jet was M F. The four cubical feet of water issued out in 206 seconds.

The results of this experiment are analogous to those of the experiments xxxi and xxxiii.

Experiment xxxv. Citizen Borda, in a very interesting memoir*, relates a peculiar phenomenon, of which he has given a very simple demonstration, from the principle of the equality of pressure, which fluids exert in every direction. It is that, if the extremity of a cylindrical tube be pushed into the interior part of the reservoir, the contraction of the vein is greater, and the expenditure less, than if the same tube be applied to the side of the vessel. I have repeated this experiment, and observed a similar result when the tube was cylindrical from one end to the other, like that made use of by the author, and when the water was made to flow out in a full stream. I afterwards gave to the interior extremity of the pipe the form A C, fig. 4, Plate VIII, vol. II, of the common contracted veins; in this case, there was no longer any remark-

* Memoires de l'Acad. 1766.

able difference between the two expenditures, in the two situations of the tube. For when the end AC was pushed into the interior part of the reservoir, the full tube afforded, in 81 seconds, the same quantity of water as was furnished in 80 seconds, when it was applied to the side of the vessel. It may be presumed, that if the part AC had more perfectly possessed the form of the contracted vein, this slight difference of one second would have disappeared*.

IV.

On the Mechanical Fabrication of Lint, and the Manufacture of Hats. By N. L.

TO MR. NICHOLSON.

SIR,

ON the subject of dressing lint, by engines, the result of my inquiries is the following:—It was attempted by a Mr. John Swan, of Whitehaven, Cumberland: the objection made against it was, that it broke the lint: could any of your correspondents, in that part of the country, obtain a drawing or model of the machine; as it may be very likely some alteration in it might accomplish the purpose? On the other subject of inquiry—Hats, a Mr. Saxton, 45 and 46, Queen's street, Southwark, is concerned with a house in the country, who makes hats with engines, but *where* he may possibly inform, I do not know myself. Messrs. Davis, Welts, and Martin, late of Stockport, now somewhere in Kent, make use of engines for plaiting hats. There is also, I understand, another company at Stockport, names unknown, who use engines to make soldiers' hats: how far might all these be improved, so as to complete the process? Hats are divided into three sorts, *felts*, those made of plain wool, cordies, cordebacks, writ cordies, those covered or otherwise with cordies wool (wool stript from the skins of those lambs that die in yeanning, called by the Scotch, *mort wool*); and the third sort is called *stuff*, *Caroline*, or *castor-hats*, that is, hats covered with beaver, or beaver and hares' wool mixed: how did the second sort, cordies, obtain that name? The reason, I have heard, is, they were first made in a town of France, whose name comes near that this kind of hat has obtained: if so, what town is it? Why are the third sort called *Carolines*? These questions may be thought, at first, trifling; but when the history of a manufactory, from its origin, is sought for, it sometimes happens that the various names of the article, in question, lead us to the discovery. Masters, in general, are divided about the use of galls in their dying: it would be well, if a regular plan could be adopted for the proportion of ingredients, whether of wool or stuff; for this we must look up to the chemists of the present day. Allow me to ask them a question, in the resolving of which, the dye-houses in the country, are much interested. By what mixture of ingredients, do the London dye-houses obtain a black colour so much superior to those of the country? Dr.

* Here concludes the interesting researches of Citizen Venturi on fluids. At the end of his publication he has given a short summary of the contents, which, on account of the length of this concluding part, I shall defer to the next number. I take this opportunity of announcing, that Mr. Taylor, bookseller in Holborn, has applied to me for permission to reprint the translation from this Journal, and that it will accordingly appear in the form of an octavo pamphlet.

Aikin,

Aikin, in his history of Manchester, says, that in that part of the country, the hatters boil the felt hats, after bowing and basoning in a mixture of "ingredients of native growth." I wish the doctor would inform us, what he means by that phrase? as the cordies about Manchester, are better *got up*, than those in many other places, and it may be owing to some vegetable put into the pan during boiling.

I am, Sir,

Your's, &c.

N. L.

V.

Report and Observations on the Art of Hat-making. W. N.

BY a private letter of recommendation from the respectable author of the foregoing communication, I visited the manufactory of Messrs. Collinsons', hatters, in Gravel-lane, Southwark; and conclude it will be interesting to many of my readers, to see an account of the process.

The materials for making hats, are rabbits' fur, cut off from the skin, after the hairs have been plucked out, together with wool, and beaver. The two former are mixed in various proportions, and of different qualities, according to the value of the article intended to be made; and the latter is universally, as I take it, used for facing the finer articles, and never for the body, or main stuff. Experience has shewn that these materials cannot be evenly, and well felted together, unless all the fibres be first separated, or put into the same state with regard to each other. This is the object of the first process, called bowing. The material, without any previous preparation *, is laid upon a platform of wood, or of wire, somewhat more than four feet square†, called a hurdle, which is fixed against the wall of the work-shop, and is enlightened by a small window, and separated by two side partitions from other hurdles, which occupy the rest of the space along the wall. The hurdle, if of wood, is made of deal planks, not quite three inches wide, disposed parallel to the wall, and at the distance of one-fortieth, or one-fiftieth of an inch from each other, for the purpose of suffering the dust, and other impurities of the stuff, to pass through; a purpose still more effectually answered by the hurdle of wire.

The workman is provided with a bow, a bow-pin, a basket, and several cloths. The bow is a pole of yellow deal-wood, between seven and eight feet long, to which are fixed two bridges, somewhat like that which receives the hair in the bow of the violin. Over these is stretched a catgut, about one-twelfth part of an inch in thickness. The bow-pin is a stick with a knob, and is used for plucking the bow-string. The basket is a square piece of ozier work, consisting of open strait bars with no crossing or interweaving. Its length across the

* Some writers mention a partial wetting of the fur while on the skin, by lightly smearing it with a solution of nitrate of mercury to give it a curl. Messrs. Collinsons do not use it, nor any other preparation.

† I give the numerical estimates, not from measure, but by memory.

bars may be about two feet, and its breadth eighteen inches. The sides into which the bars are fixed, are slightly bended into a circular curve, so that the basket may be set upright on one of these edges near the right hand end of the hurdle, where it usually stands. The cloths are linen, and dyed of a dark olive brown. Besides these implements, the workman is also provided with brown paper.

The *bowing* commences by shovelling the material towards the right hand partition with the basket, upon which, the workman holding the bow horizontally in his left hand, and the bow-pin in his right, lightly places the bow-string, and gives it a pluck with the pin. The string, in its return, strikes part of the fur, and causes it to rise, and fly partly across the hurdle in a light open form. By repeated strokes, the whole is thus subjected to the bow, and this beating is repeated till all the original clots or masses of the filaments are perfectly opened and obliterated. The quantity thus treated at once, is called a *batt*, and never exceeds half the quantity required to make one hat.

When the batt is sufficiently bowed, it is ready for *hardening*, which term denotes the first commencement of felting. The prepared material being evenly disposed on the hurdle, is first pressed down by the convex side of the basket, then covered with a cloth, and pressed successively in its various parts by the hands of the workman. The pressure is gentle, and the hands are very slightly moved back and forwards at the same time through a space of, perhaps, a quarter of an inch, to favour the hardening or entangling of the fibres*. In a very short time, indeed, the stuff acquires sufficient firmness to bear careful handling. The cloth is then taken off, and a sheet of paper, with its corners doubled in, so as to give it a triangular outline, is laid upon the batt, which last is folded over the paper as it lies, and its edges, meeting one over the other, form a conical cap. The joining is soon made good by pressure with the hands on the cloth. Another batt, ready hardened, is in the next place laid on the hurdle, and the cap here mentioned placed upon it with the joining downwards. This last batt being also folded up, will consequently have its place of junction diametrically opposite that of the inner felt, which it must therefore greatly tend to strengthen. The principal part of the hat is thus put together, and now requires to be worked with the hands a considerable time upon the hurdle, the cloth being also occasionally sprinkled with clear water. During the whole of this operation, which is called *bafoning*, the article becomes firmer and firmer, and contracts in its dimensions. It may easily be understood, that the chief use of the paper is to prevent the sides from felting together.

The bafoning is followed by a still more effectual continuation of the felting, called *working*. This is done in another shop, at an apparatus called a *battery*, consisting of a *kettle* (containing water slightly acidulated with sulphuric acid, to which, for beaver hats, a quantity of the grounds of beer is added, or else plain water for rinsing out), and eight *planks* of wood joined together in the form of a frustum of a pyramid, and meeting in the kettle at the middle. The outer or upper edge of each plank is about two feet broad, and rises a little more than two feet and a half above the ground; and the slope towards the kettle is considerably rapid, so that the whole battery is little more than six feet in diameter. The quantity of sul-

*. For the causes and mechanism of felting, see the note in *Philos. Journal*, I. 400.

phuric acid added to the liquor is not sufficient to give a sour taste, but only renders it rough to the tongue. In this liquor, heated rather higher than unpractised hands could bear, the article is dipped from time to time, and then worked on the planks with a roller, and also by folding or rolling it up, and opening it again; in all which, a certain degree of care is at first necessary to prevent the sides from felting together; of which, in the more advanced stages of the operation, there is no danger. The imperfections of the work now present themselves to the eye of the workman, who picks out knots and other hard substances with a bodkin, and adds more felt upon all such parts as require strengthening. This added felt is patted down with a wet brush, and soon incorporates with the rest. The beaver is laid on towards the conclusion of this kind of working. I could not distinctly learn why the beer grounds were used with beaver-hats. Some workmen said that, by rendering the liquor more tenacious, the hat was enabled to hold a greater quantity of it for a longer time; but others said that the mere acid and water would not adhere to the beaver facing, but would roll off immediately when the article was laid on the plank. It is probable that the manufacturers who now follow the established practice, may not have tried what are the inconveniences this addition is calculated to remove.

The acid, no doubt, gives a roughness to the surface of the hair which facilitates the mechanical action of felting. Mr. Collinson informed me that they use nitrous acid in a process called *carrotting*. In this operation, the material is put into a mixture of the nitrous and sulphuric acids in water, and kept in the digesting heat of a stove all night. The hair acquires a ruddy or yellow colour, and loses part of its strength. I did not see any part of this process, nor of the material so treated; neither did I gather any further information respecting its utility, than that, for some kinds of work, the carrotted stuff is better.

It must be remembered that our hat still possesses the form of a cone, and that the whole of the several actions it has undergone, have only converted it into a soft flexible felt, capable of being extended, though with some difficulty, in every direction. The next thing to be done is to give it the form required by the wearer. For this purpose, the workman turns up the edge or rim to the depth of about an inch and a half, and then returns the point back again through the centre or axis of the cap, so far as not to take out this fold, but to produce another inner fold of the same depth. The point being returned back again in the same manner, produces a third fold; and thus the workman proceeds, until the whole has acquired the appearance of a flat circular piece, consisting of a number of concentric undulations or folds, with the point in the centre. This is laid upon the plank, where the workman keeping the piece wet with the liquor, pulls out the point with his fingers, and presses it down with his hand, at the same time turning it round on its centre in contact with the plank, till he has, by this means, rubbed out a flat portion equal to the intended crown of the hat. In the next place he takes a block, to the crown of which he applies the flat central portion of the felt, and by forcing a string down the sides of the block, he causes the next part to assume the figure of the crown, which he continues to wet and work, until it has properly disposed itself round the block. The rim now appears like a flounced or puckered appendage round the edge of the crown; but the block being set upright on the plank, the requisite figure is soon given by working, rubbing, and extending this part. Water only is used in this operation of

Vol. III.—APRIL 1799. E *fashioning*

fashioning or blocking, at the conclusion of which it is pressed out by the blunt edge of a copper implement for that purpose.

Previous to the dying, the nap of the hat is raised or loosened out with a wire brush, or carding instrument, as I understood; but I did not see this done. The fibres are too rotten after the dying to bear this operation. The dying materials are logwood, and a mixture of the sulphates of iron and of copper, known in the market by the names of green copperas and blue vitriol. As the time of Mr. Collinson was limited, and my attention was more particularly directed to the mechanical processes, I did not go into the dye-house, but have no doubt that the hats are boiled with the logwood, and afterwards immersed in the saline solution. I particularly asked whether galls were used, and was answered in the negative. From the candid and obliging manner of Mr. Collinson, I am convinced that his information was correct, and that if he had any secret to reserve, he would not have hesitated in telling me so.

The dyed hats are, in the next place, taken to the stiffening shop. One workman, assisted by a boy, does this part of the business. He has two vessels, or boilers, the one containing the grounds of strong beer, which costs seven shillings per barrel, and is, as I presume, used in this and other stages of the manufactory, as the cheapest mucilage which can be procured; and the other vessel containing melted glue, a little thinner than it is used by carpenters. I particularly asked whether this last solution contained any other ingredient besides glue, and was assured that it did not. The beer grounds are applied in the inside of the crown to prevent the glue from coming through to the face, and also, as I suppose, to give the requisite firmness at a less expence than could be produced by glue alone. If the glue were to pass through the hat in different places, it might, I imagine, be more difficult to produce an even gloss upon the face in the subsequent finishing. The glue stiffening is applied after the beer grounds are dried, and then only upon the lower face of the flap, and the inside of the crown. For this purpose, the hat is put into another hat, called a stiffening hat, the crown of which is notched, or slit open in various directions. These are then placed in a hole in a deal board, which supports the flap, and the glue is applied with a brush.

The dry hat, after this operation, is very rigid, and its figure irregular. The last dressing is given by the application of moisture and heat, and the use of the brush, and a hot iron, somewhat in the shape of that used by taylor, but shorter and broader on the face. The hat being softened by exposure to steam, is drawn upon a block, to which it is securely applied by the former method of forcing a string down from the crown to the commencement of the rim. The judgment of the workman is employed in moistening, brushing, and ironing the hat, in order to give and preserve the proper figure. When the rim of the hat is not intended to be of an equal width throughout, I conclude that it is cut by means of a wooden, or, perhaps, metallic pattern; but as no such hats are now in fashion, I saw only the tool for cutting them round. The contrivance is very ingenious and simple. A number of notches are made in one edge of a flat piece of wood for the purpose of inserting the point of a knife, and from one side or edge of this piece of wood there proceeds a strait handle, which lies parallel to the notched side, forming an angle somewhat like that of a carpenter's square. When the legs of this angle are applied to the outside of the crown, and the board lies flat on the rim of the hat, the notched edge will lie nearly in the direction of the radius, or line pointing

to the centre of the hat. A knife being therefore inserted in one of the notches, it is easy to draw it round by leaning the tool against the crown, and it will cut the border very regular and true. This cut is made before the hat is quite finished, and is not carried intirely through, so that one of the last operations consists in tearing off the redundant part, which by that means leaves an edging of beaver round the external face of the flap. When the hat is completely finished, the crown is tied up in gauze paper, which is neatly ironed down. It is then ready for the subsequent operations of lining, &c.

The art of invention, as Leibnitz has long ago remarked, does not consist of lucky thoughts and intuitive conceptions, but is a regular operation grounded on a science which may be taught, and of which the rules are most assuredly investigated by every man who afterwards succeeds in his researches. The inventor of commercial objects ought, as I apprehend, first to ascertain whether the process, he means to improve, is capable of rewarding his exertions; and, secondly, what may be the physical and moral difficulties that obstruct his purpose. With a view to these respective departments of inquiry, I asked Mr. Collinson what he conceived might be the proportion of the charge of raw materials, rent, and other expences, compared with that which is applied to the mere fabrication of the article. He seemed to think, that this last might amount to one-third of the whole cost, and that the bowing and basoning might be one-sixth. Now if we take, as an extreme supposition, that the inventor of a machine could accomplish the whole operation at a charge altogether inconsiderable, and that the profits of the manufacturer and shopkeeper may be respectively 20 per cent, we shall find, by a simple calculation, that such an inventor, if he were to be content with the same profit, would be able to under-sell other hatters by about 23 per cent, or if his article were as good or better than theirs, he would have no occasion to lower his price, but would realize an extra profit of 28 per cent as a manufacturer. If his machinery and working processes were to call for half the present disbursement, the difference would only be $11\frac{1}{2}$ per cent in the market; and if he could only do the bowing and basoning, the sale price would be only affected to the amount of about 5 per cent, or six, considered as manufacturers profit; a difference scarcely more than may reasonably be supposed to take place in various manufactories, from the mere differences in skill and diligence between one manufacturer and another. It may, therefore, become a serious question (according to the simplicity, efficacy, and profit of a new plan, which may require a manufacturer to interrupt and lay aside his old practices), whether he should not, in prudence, endeavour rather to improve the state of his affairs by the safe and ordinary methods of industry, punctuality, and economy, than to unsettle his mind, and risk his fortune and tranquillity in this new pursuit. And still more will it become a mere speculative mechanic to consider how far he ought to pursue a scheme, for the reward of which he must either enter, most probably, with borrowed capital, into a business of which he knows nothing, or depend on the knowledge, the candour, and the integrity of a manufacturer for his gains, or the repayment of his labour and expence.

These are the motives for caution which ought certainly to present themselves in the first contemplation of an undertaking of this kind. But, on the other hand, it is too well known in this active manufacturing country, that improvements, if made and conducted with skill and in-

telligence, are productive of wealth, and a very honourable degree of independence and public estimation. In the solution of a problem of this kind, experiments must necessarily be made; and as these are attended with expence, and require to be frequently repeated, in order to avoid deception, the inventor will naturally be led to investigate and examine the general stock of facts, which, in other branches of manufacture, or departments in natural philosophy, may bear any relation to his own pursuit. Hence it appears to be of the highest importance that he should possess an extensive acquaintance with incidents and events of this kind. In the pursuit before us, for example, he should learn all the methods by which wool, fur, and vegetable filaments are manufactured. The processes of beating, bowing, carding, felting, fulling, spinning, weaving, knitting, and knotting, will pass in review before him. He will consider which may be most applicable to his purpose, and what new modifications or successive applications he may make of these respective operations, of the principles on which they are founded, and of the materials, as well as the mechanism used for carrying them into effect. The certainty of these operations, which have been practised for ages, will give strength and firmness to his new combinations, and if he possess enough of this kind of knowledge, he will, probably, enjoy the satisfaction of seeing his attempts answer his expectations, instead of being repeatedly mortified and distressed by loss of time in alterations. In the present instance, on which I have meditated but little, I am unwilling to speculate in the way of conjecture. Yet I am inclined to suggest the enquiry, whether carding, which is rapidly and mechanically done, be inferior to bowing, which does not promise much facility for mechanical operation? Whether a succession of batts or cardings might be thrown round a fluted cone, which rapidly revolving, in contact with three or more cylinders, might perform the hardening, and even the working, with much more precision and speed than they are now done by hand? Whether blocking or shaping be not an operation extremely well calculated for the operation of one or more machines? Whether loose weaving and subsequent felting might not produce a lighter, cheaper, and stronger article? And how far the mechanical felting, which is not confined merely to the hairs of animals, might be applied to this art? These, and many other questions, might be put and illustrated by incidents in the private history of manufactures which have come to my knowledge: but such a detail could not be of any advantage, unless it were more amply extended than the nature of the present publication can allow.

The moral impediments to the success of inventions, exclusive of those which arise from co-partnerships, are principally such as may be excited by rival manufacturers, or may arise from the difficulty of introducing an article which must pass through many hands in its progress from the manufacturer to the public. The manufacture of hats is, as I understand, carried on with capitals much differing in magnitude, and the manufacturer may either sell to the vender, or vend the article himself, though I do not find that this is usually the case. The possessor of a new invention for making hats might, therefore, stand alone between the seller of the raw material, and the consumer of the finished article, without being necessarily dependent on a warehouse-man or retailer. In this respect, therefore, the pursuit appears to be liable to no objection.

VII.

A popular Account of Experiments which have been made or attempted for the Purpose of obtaining an invariable Measure of Length, from the Difference between the Lengths of the same Pendulum, when adjusted to measure different known Portions of Time. (W. N.)

ABOUT the beginning of the present century, and for a considerable time afterwards, the attention of scientific men, namely Huyghens, Sir Christopher Wren, and others, was directed to the useful application of the pendulum, as an universal measure capable of being obtained from the mensuration of time, by any number of independent observers, who should carefully follow the same processes of observation. Every one knows that the vibrations of a pendulum will be slower the greater its length; and that by the repeated measurement of the same portion of time, the deviations from any given fraction of a day, as for example a second, will be shown with extreme accuracy. Thus in the pendulum which shall vibrate seconds, the difference of less than one-thousandth of an inch in the length will be shewn by a variation of one second per day; and if the pendulum, intended to be used as a measure, were adjusted so as to keep time without varying one second in the week, it should seem to follow, that our general or universal measure would be obtainable to less than the seven-thousandth part of an inch, or the two hundred-thousandth part of the whole. But many difficulties present themselves upon a nearer practical view of the subject. 1. If the expansion and contraction from change of temperature be not provided for, we still have a cause of error, which, in a steel rod, would amount to a second a day for every four degrees of the thermometer*; and it scarcely can happen in any common situation, that the thermometer will not vary many degrees in the 24 hours. 2. The wheel-work applied to keep account and to overcome the effect of the assistance of the air will in most, and, perhaps, in every case alter the measure of the vibration. 3. If the point of suspension have any spring or mobility, the vibration will be slower than corresponds with the apparent-length of the pendulum. Without entering into minute deductions respecting this variation and its quantity, according to different assumed laws, this effect may be easily shewn by hanging any weight to a string, and holding it in the hand while it vibrates. The vibration will be much quicker if the hand be held still, than if it be suffered to follow the motion of the pendulous body. 4. When a body moves in a right line, the whole of its motion may be considered as if it resided in the centre of gravity; but if it move in a curve, one part of the body will move swifter than the other, and the centre of motion, or place at which an obstacle would stop it without occasioning rotation, will be nearer to the outer side of the extreme curve than the centre of gravity itself. This centre in pendulums is called the centre of oscillation. It is usual in public lectures to illustrate this doctrine by means of a strait stick. If the stick be held by one end, and the observer strikes an obstacle downwards with the middle or centre of gravity of the stick, his

* Philof. Journal, I. 58.

hand will be jarred by a blow upwards, owing to the tendency of the outer half of the stick to proceed downwards by virtue of its greater velocity. If the obstacle be struck with the outer end of the stick, the jar will be in the contrary direction, because the whole, or the greatest part of the force, is exerted within the obstacle; but if the blow be given at the distance of one-third part of the outer extremity, it will be more effectual than in other cases, and the greater velocity of the external portion compensating for the greater mass of the part nearest the hand, no blow will be felt, because there is no tendency to rotation. The stick therefore moves, and if hung up would vibrate as if its whole mass were collected at the distance of two-thirds of its length from the point of suspension. And it is accordingly found, that a small ball hung by a thread of that length will vibrate in equal times with the stick. Mathematicians, particularly Huyghens, have determined the places of the centres of oscillation of bodies, variously figured and suspended. It is enough for our present purpose to have shewn, that the effectual length of a pendulum cannot be determined without admitting the consideration of the figure and magnitude of the pendulous body as an element in the process; or to state the effect more immediately, we may observe, that if two balls were separately suspended, and set to vibrate through equal arcs, having the distance between their centres and points of suspension respectively equal, the larger ball would vibrate slowest, because its centre of oscillation would be furthest from its centre of gravity. Hence it must follow, that our two observers, who are supposed to be in search of one and the same measure, would, independently of the other difficult requisites, find it necessary to use balls either equal or proportioned to each other in a known ratio, a condition which is precisely the difficulty this method is meant to obviate. It will, moreover, be required, in order that the determinations may hold good, that the material, whether metal or any other substance, should be of uniform density throughout, as well as that it should possess great truth of figure; both which conditions scarcely require any remark with regard to their practical difficulties. 5. The force of gravitation is different according to the latitude of the place of observation, and as far as observations have yet been carried, this variation is not governed by a perfectly regular law. It is not the same in the northern and southern hemispheres, and is probably affected by the vicinity of mountains, and the position of the plane of vibration with regard to them. These causes, which do not affect the utility of an instrument for measuring time, are of great consequence in the deduction of our linear measure. 6. Whether there be any notable irregularity in the rotation of the earth, from which our measures of time are derived, may be questioned. It is probable, from the evidence of astronomical clocks, some of which have performed with wonderful accuracy, that there is not; and this evidence is still more confirmed by the correspondence of the rotations of Jupiter and Mars with respect to that of the earth, concerning which, I have heard of no irregularity, excepting in a paper of Dr. Herschel, in the *Philosophical Transactions* for 1784. 7. The mechanical determination of the centre of suspension is not altogether an easy task.

From these, and, perhaps, other difficulties, the admeasurement of length by the pendulum had been, for a considerable number of years, disregarded, when in the year 1774, the society in London, for the encouragement of arts, manufactures, and commerce, offered a reward of one hundred guineas, for a mode of ascertaining invariable standards for weights and measures, communicable at all times and to all nations, which offer being continued for three succeeding years,

years, was at length productive of competition on the part of five candidates, one of whom, Mr. John Hatton, watchmaker, of London, proposed in March, 1779, to obtain a measure by applying a moveable point of suspension to one and the same pendulum, in order to ascertain the difference of length between two portions successively made to vibrate, and measure different known parts of time. This attempt, whether from its imperfection in principle, or the difficulties found by its author in carrying it into effect, was not considered by the Society as intitled to their reward; but as an encouragement for the farther prosecution of the subject, they presented the inventor with thirty guineas. After this time, the pursuit remained unattended to for some years, till Mr. Hatton's plan was taken up by the late John Whitehurst, F.R.S. an ingenious mechanic and worthy man, but possessed of very little science, at whose house I saw the machine going in the year 1786, which he afterwards described in a pamphlet on that express object*. He was, at that time, of opinion, that his pendulum, which consisted of a ball of lead suspended by the fine flattened steel wire, called pendulum wire (of which the length was determined by an adjustable clip), would give a measure by the difference of its lengths, as before explained, which would not be affected by the dimensions of the weight itself; and his weight was accordingly a very rude-fashioned round piece of metal. It was not without some difficulty that I persuaded him to the contrary, which is a point concerning which men of information cannot have two opinions; but afterwards, when he became convinced, he proposed that the ball should be made of a diameter answering to some aliquot part of the measure expected to be obtained, or deduced from crude experiments, and afterwards corrected from the actual result; which corrected ball being used to obtain a second result, should be again corrected, and this process of approximation continued till the results were not found to differ by any perceptible quantity. I have reason to believe, however, that he never used this method, nor even took the trouble to give any precision of figure to his ball, with which he made the experiments recorded in his pamphlet.

After the death of Mr. Whitehurst, his apparatus came, by purchase, into the hands of Dr. George Fordyce, F.R.S. who considerably improved it, and gave a description of the same to the royal society†. The most important improvement consisted in a contrivance to prevent the effect of variations of temperature upon the pendulum, which the reader will perceive to be highly valuable for the mechanical skill displayed in the execution, and in principle somewhat resembling the fixed apparatus described at page 61 of our first volume. As the similitude of the two apparatus of Whitehurst and Fordyce must render many particulars of the description of the same in both, I have given no copy of the drawings of the former; but shall speak of both conjunctively.

Fig. 3, Plate II, will afford a more perfect notion of the apparatus than may, perhaps, have been conveyed by the verbal descriptions already given. O represents the pendulous ball sus-

* An attempt towards obtaining invariable measures of length, capacity, and weight, from the mensuration of time, independent of the mechanical operations requisite to ascertain the centre of oscillation, or the true length of pendulums. By John Whitehurst, F.R.S. 4to. 34 pages, with three plates. London, 1787.

† *Account of a new pendulum*, Phil. Transf. 1794; from which our drawing is copied of a smaller size, with some amendments of the perspective.

pended from Q by the fine flat steel wire O N. In Mr. Whitehurst's machine, the ball weighed 12251 grains, or upwards of two pounds troy, and the wire, which was flat steel *tempered*, was of such a thickness, that 80 inches in length weighed nearly three grains. The piece W I affords a notch through which the wire passes, and which limits the length of the effective pendulum. This piece is fixed to the frame-plate of a train of wheels, similar to that of an eight-day clock, which, by means of a crutch of communication applied to the pendulum-wire, serves to maintain the vibration, and keep account of the time: The escapement is the dead beat of Graham *, with the addition of an arm springing from the axis of the pallets upwards, and carrying an adjustable weight; the use of which is to set the pallets themselves to the same vibration as the pendulum is intended to perform: a condition which Mr. Whitehurst, in common with other artists, supposed to be of value to prevent the maintaining power from influencing the measure of time, though this, in fact, depends more particularly on the structure and figure of the pallets †.

The principal support to which Mr. Whitehurst's apparatus was attached, consisted ‡ of two planks of deal, two inches thick, nine inches wide, and nearly six feet long, which being framed edge-wise together, left a groove to admit certain bolts, or sliding parts, from the movement S S, by the help of which it can be raised or lowered, or fixed at any desired elevation. In the actual experiment, therefore, the movement is to be slid up and fixed near the top of the frame, and the pendulum then adjusted to the longer vibration by means of the screw A, which raises or lowers the cock E G N. This, in Mr. Whitehurst's process, was one forty-second part of a minute. There is a brass rule or piece 5 feet 2 inches long, and one quarter of an inch thick, fixed at its lower end in one of the planks in a vertical position to receive and preserve the measure. On this brass piece, at the temperature of 60°, a fine stroke was then drawn by means of a proper tool applied against the edge of the plate of the movement. In the next place he slid the movement about five feet lower, and, by means of a screw applied to the bottom of the frame-plate, the piece I W was adjusted to the wire till the vibrations became 84 in the minute. At this period, another stroke was drawn as before on the brass rule, at the same temperature of 60°, which is supposed to have been kept up during the whole of the operation from first to last. This supposition of an equal or steady temperature of the air in an apartment, can hardly be admitted, except with regard to the mean temperature in a close subterraneous place, which is most steadily kept during summer, because the warmer air from above has then no tendency to descend.

Mr. Whitehurst's machine was fixed in the corner of a first-floor room, in Bolt-Court, Fleet-street, which, I think, was wainscotted. The measure obtained as the difference of the two lengths aforesaid, vibrating through semi-arcs of 3° 20', was 59,892 $\frac{1}{2}$ inches, and the length of seconds pendulum for London, thence resulting, proves 39,1196 inches, instead

* Philof. Journal, II. 52.

† Ibid, p. 49, 50.

‡ Dr. Fordyce retained this part in his improved machine. Philof. Transf. 1794, p. 16.

§ Sir G. Shuckburgh Evelyn, who afterwards measured it, found it about 0,0015 inches longer at 64°, which is nearly equivalent to the expansion of brass from 60° to 64°. See Philof. Journal I. 58, and Phil. Transf. 1798, p. 135.

of the commonly supposed measure of 39,2 inches*. He thinks the shortness of his measure a proof, that the maintaining power did not affect the pendulum. But, on this it may be remarked, that this power may either accelerate or retard the time of vibration, according to circumstances, and that there were other modifying events that would tend to shorten his pendulum, such as the unsteadiness of his house, his slight frame, and the mode of fixing, as well as the loose connection of the piece, I W, fig. 3, with that frame, the extent of his arc, and the slight spring of his wire at the place of flexure.

Notwithstanding all the objections which may be brought against this method of measuring the difference of lengths of two pendulums, instead of the direct length of one, it obviously possesses considerable advantages over this last method; and though the experiment requires uncommon precautions before unexceptionable results can be expected, yet, upon the general consideration of the limits of error in a measure obtained by this process, and that which might be had by deduction from the astronomical observations of angles, it appears more than probable, that Hatton's process would prove the most accurate. This was, no doubt, a leading motive with Dr. Fordyce to improve the machine of Whitehurst, and is the chief reason why I have thought it might be acceptable to the reader, to possess an account of facts which are only to be met with in books of confined circulation.

One of the most considerable, if not the greatest impediment to accuracy, in Mr. Whitehurst's experiments, must have consisted in the variations of temperature to which his apartment was undoubtedly subjected. Dr. Fordyce amended the construction of the clock so far, that these variations became of no consequence during its adjustments for time and the subsequent trials of rate. The wire N O, fig. 3, must lengthen by heat and contract by cold; in consequence of which, the ball O must rise and fall, and the length of the effective pendulum between I and V will be more changeable than that of a simple pendulum. The remedy of Dr. George Fordyce was founded on the consideration, that if the piece G G were supported upon a metallic rod or other piece of such a length as that the expansion of this piece upwards, by any increase of temperature, should be precisely such as to counteract the expansion of the wire and preserve the length of the effective pendulum unvaried—the frame-plate, S S, would, when the adjustment was completed, occupy that station, with regard to the brass bar, when at 60 degrees, or any other standard temperature, as it would have occupied if that temperature had been permanent during the whole of the adjustment; and, consequently, that the marks drawn on the brass while at that temperature, according to the previous directions, would not be affected in their truth from changes of this kind. In fig. 2, the mass A A A, represents the wooden support of the clock: P, represents the head of the screw, which is denoted by the same letter in fig. 3. The screw, and consequently the piece E G N of fig. 3, is supported entirely by the extremity of the lever N H P. This lever, the inflexibility of which is secured by the triangular framing M L K, rests upon a brass tube I I, and is provided with a counterpoise, O, equal in weight to that of the pendulum, and serving to secure the tube I I from any strain or flexure that might arise from the

† George Graham's numerous experiments, loosely quoted, without reference, by Desaguliers, in his *Course of Lectures*, I. 437, gave the measure between 39,133 and 39,125 inches by comparison with the standard yards at Guildhall and at the Exchequer.

weight being all on one side. The evident advantages of the metallic support I I being made in the form of a tube are, that it will be more rigid than the same mass any otherwise disposed, and that it will more speedily, on account of its surface, acquire the temperature of the surrounding air. B B is a brass support to which the tube C C is firmly fixed or soldered. This last tube is slit longitudinally, and fits upon the tube I I, so as perfectly to steady it, but, at the same time, to leave it at liberty to slide up and down with considerably facility. A stronger piece E D F surrounds the tube C C, and when the thumb-screw G is tightened, it causes C to embrace I firmly, and confine it from sliding at all. Imagine the tube I I to be continued downwards; and at some distance lower upon the wooden frame of support there is screwed the piece A D, fig. 1. This piece, at its outer face, affords a dove-tail groove for a sliding part, G G, to move in. The sliding part carries a piece which forms two half holes at K, which embrace the tube I I, and hold it firmly when the thumb-screws L L are screwed up: the adjusting screw H H is previously used to set the sliding piece to any desired position. If we now connect the whole together, and suppose the tube I to be grasped by the piece K while it remains unconfined at D, in fig. 2, the whole pendulum will be raised and lowered by the expansions and contractions of the tube I I, which is above K. The centre of suspension V, fig. 3, will also be liable to some change of place from thermometrical variations of the wooden frame, and the ball of the pendulum will be lowered by the expansion of the steel-wire from the same changes as caused them to rise from the expansion of the tube I I. Whatever may be the metal of the tube, it is easy to conceive, that if it be very long, its effect will predominate over that of the expansion of the wire, and, on the contrary, that if it be shorter than a certain length, the contrary effect will take place. That the tube shall accurately compensate for the changes in the pendulum is a matter of experiment or trial; and as brass expands more than most other metals in common use, it is clear, that a shorter tube or brass will answer the purpose better than one of any of the other common metals. To make the adjustment, nothing more is required than, in the first place, to screw the tube fast by the nut G, fig. 2, and afterwards loosen the piece K, fig. 1. The sliding piece G G may then be moved upwards or downwards, by means of the screw H H, accordingly as the observed effect of temperature upon the rate may have indicated the necessity of shortening or lengthening the acting part of the tube. The screws L L, fig. 1, may then be again screwed up, and the screw G, fig. 2, loosened.

It is rather unfortunate, that neither the publication of Mr. Whitehurst, nor that of Dr. Fordyce, contain any register of the going of this clock. The former had no transit instrument, but carried his time from his neighbour, Mr. Mudge. The latter, who had mislaid the particulars of his observations, kept his instrument going at the vibration of seconds for about nine months, during which, the thermometer had fallen as low as 15° of Fahrenheit, and risen as high as 84° . The greatest difference from its rate of going was, in the words of the Doctor; "Counting on according to the rate of its going, during the whole time, it never exceeded the sum half a second, nor was ever less than half a second, whether it was taken from day to day, month to month, or from any one to any other period during the observation," which, I think, is at least as accurate, if not more so, than the performance of any other time-piece.

It is still more unfortunate, that Sir George Shuckburgh Evelyn, in his late researches to ascertain

ascertain a standard of weight and measure, did not find any wire which would support the weight of the pendulum in this machine during any considerable period of time, while it was in his possession in 1796, though he used wire nearly twice as thick as that of Mr. Whitehurst. From this incident, I should have suspected that Mr. Whitehurst himself might have annealed his wire to a certain point, as he said it was *tempered*, if it did not appear from a passage in Dr. Fordyce's paper, that he himself used it from the bobbin. For, he says, that the curvature was not entirely unfolded for some months, as he concludes from the clock having lost in its rate during that time.

Upon the whole, then, though the apparatus for making experiments with pendulums is brought to a considerable degree of perfection, yet it appears that the fundamental experiments still remain to be made, in order to afford a standard measure; the old result of Whitehurst being the only one we are, at present, in possession of.

VIII.

On the Vibration of the Wings of a Fly. By S. R.

To Mr. NICHOLSON.

SIR,

THE connection of the different departments of science with each other is often shewn in researches where it might least be suspected, and it frequently happens, that subjects of no apparent or immediate utility may give birth to reflections which afford an innocent and interesting source of entertainment, and often lead to more valuable consequences. Some time, in the course of last summer, my thoughts were accidentally directed to the solution of the problem of the frequency with which flies and other small creatures move their wings. If we were to examine the facts upon the general principles of mechanics, taking for our data the size and inclination of the wing, and the velocity of flight, we should certainly obtain a very rapid series of strokes; but there are two other methods of direct experiment, the first deduced from the doctrine of sound, and the latter from optical principles, which give an immediate result. As the number of vibrations performed by a sonorous body in a second, to produce a given note are known, and no insect has any other voice than that which is afforded by the immediate vibration of its wings or other mechanical action, we may estimate the frequency of vibration in the bee, the fly, and the gnat, from the respective notes with which their tones are in unison. But, into this enquiry I shall not enter, because the other optical method has answered my purpose.

M. D'Arcy*, Dr. Watson†, and others, have shewn that the impression of light upon the eye endures for a time after the object has ceased to exert its action, and this truth is familiarly illustrated by the common experiment of whirling the end of a lighted stick; the ignited extremity of which will appear to form a circle, if the motion be quick enough to bring the end round to its first place, before the original impression has ceased. When a common fly moves horizontally towards the sun, and the observer views him in a line at right angles to his flight

* Memoirs of the Paris Academy, 1765.

† On time.

(both which events are continually presenting themselves), the strokes of his wings may be observed from the reflections of the light, which exhibit a row of bright specks or stars. When the fly moves at the rate of about five feet per second, the intervals are about ten in the inch; but in very quick flight, to avoid danger, he can advance near three quarters of an inch per stroke. Now $5 \times 12 \times 10 = 600$ strokes per second, in the first case. I could not estimate the velocity in the second case; but suppose it might be six or seven times as much, or about 30 feet per second*. No sound was produced in either case; whence, I suppose, the sound of the wing, when the fly is caught, or the shrill tone of an gnat when it sticks itself in the melted tallow of a candle, are produced by much stronger and quicker actions.

If you should think these observations may be acceptable to your readers, they are at your service; but if not, excuse the trouble I give you, and suppress them.

I remain, Sir,

Your obliged reader,

S. R.

IX.

An Account of several Veins of Sulphate of Strontian or Strontites, found in the Neighbourhood of Bristol, with an Analysis of the different Varieties. By WILLIAM CLAYFIELD†.

THE first specimen of sulphate of strontian was shewn to me by Mr. Tobin, about three years since. At that time it was generally believed to be merely a variety of sulphate of barites. It had been found at Redland, a short time before, in a vein of considerable thickness. The greater part of this vein has received a red tinge from the iron stone on which it lies, and exhibits but slight traces of any regular crystallization. In some few situations, however, it is entirely free from colour, and appears to be composed of a confused mass of bevelled tables, loosely adhering together. Its specific gravity varies from 3,51, to 3,87. Walking along the beach at Aust-passage, in June, 1797, I met with a similar substance, and soon discovered several detached veins in different parts of the cliff. The strata in which these veins are found are nearly horizontal, consisting of limestone of different degrees of hardness, and argillaceous sandstone, intermixed with clay and gypsum. The whole cliff, as well as the surrounding country, have evidently been produced by aqueous deposition, since which period, the level of the water in the main channel having been considerably lowered, the Severn current has acquired sufficient force to deepen the bed of the river by plowing up the strata which had been previously formed.

A second deposition of the soil towards the mouth of the river, by forming diagonal fissures in the cliff, has occasioned an inequality of five or six feet in the level of the strata.

These fissures are mostly filled up with veins of strontian, from three to twelve inches in thickness, consisting of an assemblage of semi-transparent crystals, flanked up on each side by a thin layer of a fibrous fracture; both of a delicately blue tinge. This last variety was observed

* A race-horse, at first starting, will clear 90 feet per second, making about four, or, perhaps, five leaps each second.

† From "Contributions to physical and medical Knowledge, &c. &c." collected by Thomas Beddoes, M.D. See the next article.

about three months since, by Mr. Deriabin (inspector of the Russian mines), who was immediately struck with its resemblance to a substance of the same nature found in Pennsylvania.

The crystallization of the middle vein is either that of bevelled tables, or rhomboidal cubes of nearly an inch in diameter; the transparency of the latter exceeds that of every other species; the specific gravity of the cubes varied from 3,88 to 3,96, while that of the fibrous was about 3,91.

Wishing to obtain some muriate of barites, about nine months since, I reduced a portion of the spar to the state of a sulphure, and dissolved the earth in marine acid; the great solubility of the salt, with its needle-formed crystals, soon indicated the presence of strontian.

Several trials which were then made with it, fully confirmed the result of the first experiment. Shortly after this, Dr. Beddoes informed me of his having met with a paper of Klaproth's, containing an analysis of the American fulphate.

Since the first discovery of this rare production, Mr. Bright has furnished me with specimens of another variety from the neighbourhood of Ham-green, where it is found breaking through the soil in such large masses that it has been made use of in mending the roads. The crystallization of the latter, like that of the Redland, consists of bevelled tables; it does not, however, partake either of its tinge, or semi-transparency: its specific gravity is between 3,60 and 3,68.

The present state of the arts furnishes continual instances of the refuse of one manufacture forming the basis of a second. While this continues to take place, it is evident that every mere production must claim a full investigation. The peculiar properties of this earth renders it probable, that its affinities may shortly be made to furnish us with those productions from the raw materials of our own island, which we can now only obtain with considerable difficulty from other countries.

The following analysis was undertaken at the solicitation of some chemical friends; more leisure would doubtless have contributed to greater accuracy. What is now stated is the mean result of several experiments, the differences of which have rarely amounted to more than two or three grains. Considering the fibrous variety as the most deserving attention, it was the first subjected to analysis.

To find whether it contained any portion of water, or other volatile material, 500 grains were exposed to a red heat under a muffle: the loss, amounting to no more than four grains, proves that the quantity of water, if any, must have been very trifling.

1. 200 grains of the powdered spar, in its original state, were digested with a solution of carbonate of pot-ash (obtained by deflagrating nitre and tartar); the powder, when dried in a red heat, weighed 163,5 grains.

2. A solution of this powder in diluted marine acid extricated 47 grains of carbonic acid, leaving about one grain undissolved: this was afterwards taken up by the alternate application of carbonate of pot-ash and marine acid. From this it appears that the whole quantity of earth must have been very nearly 116,5 grains.

3. The solution, No. 2, was then fully charged with caustic ammoniacal gas, which produced scarcely any traces of precipitation; the addition of carbonate of ammonia immediately threw down a precipitate, which, dried as before, weighed nearly 160 grains, the difference
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in the weight of the precipitate, and that of the 162,5 grains taken up by the marine acid, arose from decanting the solution into different vessels.

4. To detect any small portion of barites which might be contained in the precipitate, a quantity of marine acid was poured on it, sufficient to dissolve only a few grains of the earth. Had any barites been present, it would have been taken up in preference to the strontian, from its superior affinity for the acid; the solution, however, after digestion for several hours, still crystallized in needles, and afforded a copious precipitate to baritic lime-water.

5. 164 grains of the precipitated carbonate of strontian were thrown into marine acid, which extricated, as before, about 47 grains of carbonic acid, leaving nearly one grain undissolved: the addition of sulphuric acid to the solution reproduced 200,8 grains of sulphate of strontian.

6. The solution of pot-ash, No. 1, was then taken, and the whole of the carbonic acid remaining in it disengaged by an excess of marine acid: the addition of muriate of barites afforded a precipitate, which, after drying in a red heat, weighed nearly 240 grains: had the whole of the 200 grains been decomposed, the solution would have furnished nearly 250 grains of sulphate of barites.

Klaproth and Dr. Withering having estimated the quantity of sulphuric acid contained in artificial sulphate at 33, and Fourcroy at 35, of the specific gravity of 2,24, or that contained in sulphate of pot-ash; before the concentration of the acid contained in sulphate of strontian could be known, it was necessary to make the following experiments:

7. 218,5 grains of artificial sulphate of barites were decomposed by digesting with a solution of carbonate of pot-ash, producing 190 grains of carbonate of barites, from which marine acid separated 42 grains of carbonic acid, leaving nearly 14 grains of earth in solution: from this experiment it appears that sulphate of barites contains about 32,2 per cent of acid.

To find the concentration of this acid, 124 grains of sulphuric acid of 1,843 specific gravity, containing (according to Kirwan's table, in the Irish Transactions) 109,12 grains of standard, or 97,42 grains of 2,24, were precipitated by baritic lime-water, producing 283,3 grains of sulphate of barites, containing nearly 34,4 per cent of acid of 2,24 specific gravity.

9. 92,2 grains of the same acid were precipitated by a solution of muriate of barites, the sulphate of barites weighed nearly 212 grains, containing about 34,1 per cent of acid of 2,24. By taking the mean of these experiments, we may estimate the quantity of acid contained in sulphate of barites at 33 per cent of the specific gravity of 2,24.

According to this calculation, the 250 grains of sulphate of barites, No. 6, would furnish 82,5 grains of acid of the above strength.

10. To ascertain the difference between native and artificial sulphate of strontian, 204,2 grains of sulphuric acid of 1,843 (containing 160,44 grains of 2,24 specific gravity) were precipitated by strontian lime-water, producing 360 grains of sulphate of strontian, containing about 44,5 per cent of acid. This accounts for the 200,8 of sulphate being produced from 163 grains of carbonate of strontian, No. 5. Hence the proportion of acid in the artificial will exceed that of the native nearly 2 per cent.

11. To determine whether the solution, No. 2, contained any calcareous earth, a small quantity of oxalic acid was added to it; no precipitation, however, took place.

12. Prussiate

12. Prussiate of pot-ash occasioned a slight blue tinge.

The different varieties containing, so nearly, an equal proportion of earth and acid, the statement of a single analysis will be sufficient for the whole. Should there be any difference between these, it will probably be found in the Ham-green varieties, containing rather more acid; the quantity of sulphate of barites produced from it amounting to nearly 252 grains.

From the foregoing experiments, it appears that 200 grains of the fibrous variety contains :

Strontian	116,5
Acid of 2,24	83,5
With a small proportion of iron	
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	200,0

In addition to the modes of distinguishing the two earths already noticed, we may state the crystallization of the sulphures.

A warm solution of the sulphure of barites deposits, on cooling, an assemblage of several very thin layers of inclined oval plates, terminating in points, and radiating from a centre, while the sulphure of strontian runs into a base line supporting a number of parallel perpendiculars, gradually lessening so as to form the diagonal of a square.

The strontian earth in a state of purity frequently varies in its crystallization, sometimes depositing solitary tables, and at others, arranging them in irregular lines. Both barites and strontian combine with phosphorus, and exhibit familiar appearances to the phosphure of lime. Mixed with a few grains of oxygenated muriate of pot-ash, and triturated in a mortar, an explosion took place.

Note by the Editor. (Dr. Beddoes.)

Mr. Clayfield has lately been informed that another variety of sulphate of strontites is found near Sodbury. Mr. Derabin has seen a blueish fibrous variety, from a coal-pit, near Dumbarton. On a professional journey to the north, I was struck at Keswick with a specimen in Mr. Hutton's collection, labelled *friated gypsum*. It is an exceedingly beautiful white sulphate of strontian, from Alston, as the label bears. From Mr. Hutton, I have also received a blueish specimen, crystallized in rhomboidal tables, which I took for sulphate of strontian; but Mr. Clayfield finds it to be barites. It comes from Cleter-moor, Cumberland. I have another from Newlands, Cumberland, having the exact appearance of the sulphate of strontian from Ham-green, which requires further examination. Many specimens, supposed to be baritic, will doubtless, on examination, prove to be strontitic. But the distinction will require nice inspection, even from those most versed in the external characters of fossils.

The following experiments were made, at my request, by a friend :—Twelve grains of carbonate of barites were given to a two-months old rabbit. In half an hour not much affected — in an hour nearly dead.—The barites had acted violently as a cathartic, and had produced almost general paralysis. In about two hours, the rabbit died much convulsed. The stomach was greatly inflamed; the inner coat separated from that below, lying in folds, and as if half macerated.

macerated. Five grains of barites in a second experiment, and two in a third, killed similar rabbits with the same effect on the stomach, only the fæces were not so much softened. Forty grains of sulphate of barites had no effect; twelve grains of carbonate of strontites, obtained from the Bristol sulphate, acted as a cathartic only: of the sulphate itself, twelve grains had no effect. This seems to show that strontites might be tried with little risk, as a medicine, if any analogy should afford hope of benefit from it.

At my further request the following experiments were tried. Five grains of carbonate of barites, mixed with five of Cayenne pepper, were given to a similar rabbit, which lived five hours, and was not much affected, till within an hour of its death, when it suffered as in the first experiment, and the stomach was similarly injured. Considering that sulphate of barites has no action, by reason, probably, of its insolubility, I thought it possible, that innoxious absorbents might engage the acid of the stomach, and prevent the effect of carbonate of barites; in which case, under certain circumstances, we should be provided with an antidote for this deadly poison. The following experiments shew the conjecture to be erroneous. Five grains of barites, with five grains of prepared potash, were given to a rabbit; the animal seemed immediately affected, in less than an hour was scarce able to move, and in less than two hours died convulsed, with a stomach extremely injured, as before. Five grains of carbonate of barites, with twenty of chalk, destroyed a rabbit in five hours: the animal died tranquilly. Carbonate of barites five grains, with olive oil, killed a rabbit in less than two hours. The animal having remained all night unopened, the inner coat of the stomach was so much loosened, as to shake out with its contents. Happening to try calcareous liver of sulphur as an antidote, my friend observed the following phenomena.

Four grains of carbonate of barites, with a teaspoonful of a solution of sulphure of lime, being given to a rabbit two months old, the animal died immediately. The stomach being instantly examined, was found of a dark colour, and at its curvature (qu. the great curvature?) converted in a yellow spongy substance, which looked as if burned by a caustic, and when removed, left but a very thin membrane. The contents of the stomach near the darkened part, which seemed bounded by a whitish line, were also dark coloured. The *whole* inner coat was destroyed. As this rabbit, two days before, had been subjected to an experiment with sulphate of barites, it was necessary to ascertain whether this had not some share in the unexpected effect. A teaspoonful, therefore, of the same solution of calcareous sulphure was given to a full grown rabbit; it had the same immediate effect in destroying life; and the state of the stomach was found to be the same. On the 26th of January, 1799, a piece of solid sulphure of pot-ash, about the size of a pea, was given to a rabbit; and an hour afterwards, another piece of the same size. A few minutes after the first dose, the animal had convulsions of the skin, which were considerably increased by the second. The whole head then began to swell, and in two hours the tongue was so enlarged, as to be frequently bitten in attempts to masticate. In two hours and a half, the swelling was so large, and the breathing attended with such difficulty, and croup-like noise, that it seemed probable, the animal had but few minutes to live. The operator, heartily sick of these cruel experiments, and unwilling to sacrifice more victims, gave the rabbit two teaspoonfuls of olive oil, and was much pleased to find that in ten minutes it appeared much relieved, and that the noise in respiration, had

had ceased. The animal remained very still, and after oil had been applied to the swellings, was left all night in a basket of hay, in a warm situation.

January 27. The rabbit had left his basket; a little warm milk was given him. In the evening he eat his parsley, and drank his water heartily. On the evening of the 28th, the power of the antidote being supposed to be ascertained, the rabbit was killed, and a surgeon was desired to examine the swellings. He found that they arose from an enlargement of the maxillary glands, and of the lymphatics. The stomach was covered with a number of small specks, and seemed a little softened. It should be observed that this rabbit had been kept on dry food, which may account for the comparatively slow action of the sulphure. An accident confirmed this: in the course of the experiment, having put his nose into water, he received some on his tongue, which being swallowed, greatly increased his agonies. It happened shortly before the oil was given. The sulphure had been carefully prepared. The experiment with the solution of calcareous sulphure was twice repeated: each time the rabbit died in less than two minutes. The stomach was black and yellow, spongy, and much corroded.

X.

Letter from Dr. Beddoes, respecting the Discovery of Sulphate of Strontian, announced at Page 535, Vol. II. of this Journal.

TO MR. NICHOLSON.

SIR,

I SEND you the West-country contributions, supposing that it falls within your plan to announce the publication. You will do this the more readily, as an article in your last number erroneously represents Dr. Gibbes as the discoverer of the sulphate of strontian in this neighbourhood. This mistake you will see corrected in a note* to my table of contents. Whether Mr. Richardson told Dr. Gibbes what he had learned from Mr. Notcutt, viz. *that the specimen he gave the doctor was sulphate of strontian*, I have not yet learned; but I suppose he did, because it is so very natural to do so, in presenting a specimen; and because Dr. Gibbes, as it appears, immediately directed his experiment according to that supposition. The title of the paper, no doubt (Discovery of Sulphate of Strontian, near Sodbury, by T. S. Gibbes,

* The note here alluded to is as follows.—As Dr. Gibbes has related an experiment of his own on the Sodbury sulphate of strontian in Mr. Nicholson's Journal, March, 1799, without referring to any other person's previous observations, it seems but just to mention the following circumstances: In the collection of the Rev. Mr. Richardson, at Bath, a friend of mine (Mr. Notcutt), instructed by Mr. W. Clayfield's specimens, pointed out some sulphate of strontian. Mr. Richardson gave a piece to Dr. Gibbes, in January last. I had exhibited this substance, as found in other places near Bristol, to a large audience, nine months before, viz. in Spring, 1798, and had sent specimens to Mr. W. Henry, who communicated the fact to the Philosophical Society, at Manchester. Dr. Gibbes does not, undoubtedly, mean to claim the discovery. He knew last year that Mr. Clayfield was analysing the fossil. Hundreds of persons might have anticipated Mr. C. in announcing the fact. It is, indeed, I observe, distinctly noticed in the *Appendix to the Monthly Review*, vol. xxv, p. 580, June, 1798. The manner in which this sulphate was detected in this neighbourhood is exactly related below.

M. B.), comes from you*. The thing itself is of small importance. Nobody thinks it of less than the real discoverer of the great quantities of this mineral, near Bristol. But unless rightful claims are respected in matters of this sort, the confidence of private communication will be destroyed, than which few events could be more baneful to philosophy. I leave this disagreeable subject, with sincere regret that justice could not be done to the parties by private explanation.

The second edition of my introductory lecture will shew you that the project for giving select lectures on anatomy and physiology, to a mixed audience, succeeded. We have now had two such courses. I intend (if I can find time) to give an extensive course of physiological lectures, principally with a view to instruct parents in the means of preventing sickness and disease.

Last spring, I published a proposal for instructing mechanics in the principles of chemistry. But the advantage of the scheme not being felt, it was not carried into execution. But since the *Institution for diffusing Economical and Mechanical Improvements*, has received such respectable sanction in London, I should hope that another attempt may succeed at Bristol.

The *Medical Pneumatic Institution* is open for out-patients; and soon will be for in-patients. I shall immediately circulate a prospectus. I hope for further subscriptions. Under the superintendence of a young man of such extraordinary genius, as Mr. Davy, I think we may expect useful and curious discoveries.

I am, Sir,

Your obedient Servant,

THOMAS BEDDOES.

Clifton, March 24, 1799.

* In every case where it has been expedient for me to prefix a title to the communications of correspondents, I have endeavoured, and I hope with effect, to shew that I was the writer. In the present instance, the title seems to be appropriate to the narration, which appears to state Dr. Gibbs to be the discoverer what the substance was.

SCIENTIFIC NEWS AND ACCOUNT OF BOOKS.

Rectification of Ether.

ONE principal object in the rectification of ether, is to deprive it of the sulphureous acid ; for which purpose the addition of a re-agent is necessary. It has been usual to add an alkali. Dizé has found it much more advantageous to add a substance which might afford the requisite quantity of oxygen to convert this into the sulphuric acid ; in which state it is not disposed to rise and come over. Various metallic oxydes were tried, among which the black oxyde of manganese proved the best and the cheapest. His process is as follows :

The sulphureous acid contained in unrectified ether being neutralized with oxyde of manganese, the fluid is decanted into a pewter vessel of the capacity of fifty ounces, which is placed on a water bath. To this vessel a head and worm are adapted, the latter of which passes through a refrigeratory constantly supplied with water in a stream from below, which causes the heated water to flow off above. The distillation is then performed by raising the bath to a temperature of 36° (113° Fahrenheit if the decimal thermometer be here meant). The rectification by this treatment usually requires a day to complete it. The flavour of the ether is of the best kind, and the product about one-sixth more than in the usual method with retort and receiver. Dizé has practised this method with success for three years.

Journal de Physique, April, 1798.

Effects of the Gases in producing Sound through a Pipe.

PROFESSOR CHLADNI, well known for his numerous discoveries relating to the theory of sound, engaged professor Jacquin, of Vienna, to make some experiments on the properties of the different gases considered as sonorous bodies, particularly those gases which constitute our atmosphere, and serve to produce vocal sounds. A glass bell was furnished with a metallic cork cemented to a neck at the top ; and in the bore of this cock, within the glass, a small flute or pewter (etain) about six inches in length was fixed. The glass being then placed on the shelf of the pneumatic vessel, and filled with any particular kind of gas, a bladder also filled with the same gas, and provided with a cock, was adapted to the external aperture of the cock belonging to the bell-glass. In this disposition of the apparatus, the flute was made to sound by gently pressing the bladder. Comparative experiments were made with atmospheric gas, oxygen, hydrogen, carbonic acid, and nitrous gas. The intensity of the sound did not vary ; but when compared with that produced by atmospheric air, the oxygen gas gave a sound half a tone lower ; azotic gas, prepared by different methods, constantly gave a sound half a tone lower ; hydrogen gas gave nine or eleven tones higher ; carbonic acid gas one-third lower, and nitrous gas also very nearly a third lower. A mixture of oxygen gas and azote, in the proportions of the atmospheric air, afforded the tone of this last ; that is to say, it was half a tone higher than each of the component parts alone. When the two gases were not uniformly mixed, the sound was abominably harsh. Chladni intends to give a fuller account of these interesting experiments.

Journal de Physique, Vol. IV. N.S. p. 57.

Contributions to Physical and Medical Knowledge, principally from the West of England. Collected by Thomas Beddoes, M.D. octavo, 539 pages. Bristol, printed for Longman and Rees, London.

I received this work too late in the month to give it that attentive perusal which is requisite to enable me to give an account of its contents. The reader has already seen an extract in the present number.

Tables Portatives de Logarithms, &c. or Portable Tables of Logarithms; containing the logarithms of numbers, from 1 to 108,000; the logarithms of sines and tangents for every second in the first five degrees, and for every ten seconds of the remaining degrees of the quadrant; and also for every ten-thousandth part of the arc, according to the new centesimal divisions of the quadrant (to seven decimal places): with a preliminary discourse on the explication, use, and summation of logarithms, and their application to astronomy, navigation, practical geometry, and the computation of interest: to which are added, a table of logistic logarithms, with other tables of use for computing the longitude at sea. By Francis Callet, stereotype edition; engraved, cast, and printed by Firmin Didot, 1795, in the third republican year. Sold at Paris by Didot, price 14 livres, and in London by Taylor and De Boffe, price 18s.

In addition to the acceptable information to men of science, that a good edition of this useful work is at present to be had, I shall take this opportunity of noticing the stereotype method of printing, which Didot has pursued with much spirit, and with the advantage, as I understand, of support from the French government in these tables. The term stereotype is derived from the words στερεος, *firmus*, τυπος, *nota*; doubtless on account of the immutable connection between all the parts of the form from which the impression is to be given. I have not heard what may be the peculiarities of the method of Didot, for every ingenious man will make his improvements in the art he undertakes to carry into effect; but I scarcely need take notice to those who are acquainted with printing, that the project of folding a whole form together, or of casting a new form from an impression made by a general system of types, or page ready composed, is not in itself new. Rochon mentions* the Sallust of Ged, under the title, *C. Crispi Sallustii Catilinarium & Jugurthinum Historia*, *Edinburgi, Guill. Ged, Auri Faber edinensis non typis mobilibus ut vulgo fieri solet, sed tabellis seu laminis fuis excudebat, 1744, in 16mo.* which, he says, is well printed, and perfectly similar to a book printed with moveable types. Didot now, it seems, follows the same process as Ged, that is to say, he casts plates from which he afterwards makes his impressions; but the logarithms are from folded types. The advantage of Ged's method is, that only a few original types, comparatively speaking, are necessary to form the page, which is to serve as the pattern for the subsequent casts; that these casts or plates, setting the type at liberty to be again used in other works, may be preserved for the purpose of printing as many impressions of the work as may afterwards be wanted; that the risk of any greater number of impressions than the public may actually call for, is, by this invention, done away, and that even the

* Journal de Physique, IV. Dr. S. 364. See also Professor Wilson's paper in Philol. Journal, 11,000.

plates themselves may become an article of commerce, not only for shop-bills, advertisements, and other similar articles, but also for books of constant sale and invariable structure, such as mathematical works, the classics, religious books, and the works of celebrated authors deceased. How far this invention might be of value, with regard to books which are altered and improved in every subsequent edition, may, perhaps, be questioned; but on a loose consideration of the subject, it seems as if it would, in every case, be advantageous to a bookseller to print a few copies of a work, and keep the press standing to print others as they may be wanted;—I say it would be advantageous if it were not for the immense value in types, which would, by that means, be locked up. But the stereotype method has all the advantage of keeping the press standing, with none of its inconveniences, unless the weight and charge of metal in the plates should approach to that of the types they represent. To form some judgment of this, it may be stated, that the works of Virgil, printed by Didot, in 18mo. which I have had from De Boffe, form a beautiful volume of 418 pages, of 35 lines each. The character ranges line for line with that called bourgeois, No. 2. in Callon's book of specimens, the face of the letter being rather smaller; and we are told * that the price of the plates of this work is twelve hundred francs, or 50l. sterling. From this fact some judgment may be formed of the commercial question, but I am not at present provided with the means of making the statement.

Besides the Virgil and the Logarithms, Didot has printed *, the fables of La Fontaine, Cornelius Nepos, Phaedrus, the Vicar of Wakefield, the works of Racine, 5 vols.—of Boileau, 3 vols.—of J. B. Rousseau, 2 vols.—and Lady Mary Wortley Montague's Letters. He is now employed on an edition of Voltaire's works. All the works here mentioned are of the eighteen size, which, I suppose, to be better adapted to the present state of the art than the larger sizes, perhaps from the difficulty of insuring perfect casts in the latter.

Proposals for forming by Subscription, in the Metropolis of the British Empire, a Public Institution for diffusing the Knowledge, and facilitating the general Introduction of useful mechanical Inventions and Improvements, and for teaching by Courses of Philosophical Lectures and Experiments, the Application of Science to the common Purposes of Life, by Benjamin, Count of Rumford, F.R.S. M.R.I.A. &c. Octavo, 50 Pages. Sold by Cadell and Davies in London, Price 6d.

This pamphlet consists of an introduction, followed by the proposals of Count Rumford, which have been adopted by the society. In the introduction he states, how slow the advancement of useful improvements has ever been, compared with the rapid adoption of those changes which form the object of fashionable caprice; that the force of habit operates in favour of old methods; that novelty is offensive, and men ashamed to learn in departments where they have supposed their knowledge to be complete; and that the practical introduction of new improvements is rendered more difficult by the ignorance of workmen who continually blunder, as well as their presumption which leads them to alter what they know nothing of. Hence he deduces the great utility of a general collection of models of things really excellent; not only because the

* La Decade Philosophique, &c. No. 6. An. VII.

instruction of artists would be the immediate consequence, but likewise because even the most powerful imaginations would be wonderfully assisted by such an exhibition. The moral consequences are likewise adduced in favour of an establishment for this purpose. Such a reputable institution would be above the suspicions of interested motives in their pursuits. Jealousy and envy, which never fail to attack such active individuals as exert themselves in disinterested projects for the public welfare, and in too many instances deter men from laudable exertions;—these mean energies would have less power to attack the managers of a public institution, and the managers themselves, on the other hand, would have the firmness, the spirit, and the power to resist them. The obstacles to discovery are not confined merely to objects of art. Science also suffers from the different station, habits, and motives which give energy to philosophers and practical artists. The philosopher seeks fame; the manufacturer, profit. Various inducements tend to prevent a mutual communication between these classes of men, though both would be highly benefited by a cordial intercourse. The new institution has this great object in view. One circumstance more is mentioned by the Count, in addition to the foregoing remarks; namely, that we are indifferent to improvements, and think them of little value, because our forefathers did very well without them; and on this topic he justly remarks, that the argument is of no value, since it may be brought in defence of the most savage state of human existence; the only difference between this state and that of highest culture and civilization being, that in the latter a proper attention has been paid to mechanical improvements. The introductory part concludes with an historical sketch of the origin of the plan now submitted to the public, into which, for the sake of brevity, I must forbear to enter.

The same motive induces me considerably to abridge the proposals, or outlines of the plan. The object or purpose of the institution is clearly expressed in its title. In the execution of the plan, the first step of the managers will consist in preparing spacious and airy rooms, for the reception and public exhibition of all such new and mechanical inventions and improvements, as shall be thought worthy of the public notice. The models or machines will, as much as possible, be of the full size, and exhibited in action as in their real application. A complete laboratory and philosophical apparatus will be provided for making experiments; and men of the first eminence, will be engaged as lecturers.

The funds are to be supplied according to the following constitutional regulations: 1. By the subscriptions which shall be received from the original founders, and sole proprietors of the institution, at 50 guineas each person, to be but once paid. 2. By the sums contributed by a second class of subscribers, at 10 guineas each person, to be but once paid. 3. By annual subscriptions at 2 guineas each. 4. By donations and legacies. And, 5. By sums received at the door from occasional visitors.

The subscribers at 50 guineas are the proprietors of the institution, which is to be vested in them, their heirs, and assigns; individually; but no assignment can be valid till ratified by the managers for the time being. One half of the amount of their subscriptions will be permanently vested in government stock, or freehold property, and no part of the money will be demanded of the subscribers, nor any steps taken for carrying the plan into execution, before a charter shall have been obtained, which will effectually defend the members of the institu-

tion

tion from the general consequences of co-partnership. The proprietors alone will have the privilege of voting for the appointment of managers and visitors, and they alone will be capable of serving in either of those offices. They will have two transferable tickets of perpetual admission into every part of the establishment, and two transferable tickets of admission to all the public philosophical lectures and experiments.

The subscribers of ten guineas each will receive one ticket of admission to every part of the institution, and another ticket for the public lectures and experiments. These tickets will be for life, but not transferable.

The annual subscribers will also receive two tickets for the same purposes, but they will be for one year only, and not transferable. An annual subscriber may become a subscriber for life, by paying eight guineas at any time before the expiration of the year he has subscribed for.

A member of any of the three classes will be entitled to copies or drawings of any of the models for the use of themselves or friends, at their own expence. The institution will be governed by nine managers, chosen by ballot by the proprietors from their own body. These managers will receive no pay or emolument. They are not to dispose of any of the property in premiums. The committee of visitors will also consist of nine proprietors. Their business is to inspect and report upon the state of the institution.

The list of proprietary subscribers on the 7th day of March, was as follows :

Sir Robert Ainslie, bart.
J. J. Angustein, esq.
Rt. hon. sir Jos. Banks, K.B.
Tho. Bernard, esq.
Scrope Bernard, esq. M.P.
The earl of Beſborough.
Rowland Burdon, esq. M.P.
James Burton, esq.
Timothy Brent, esq.
Henry Cavendish, esq.
Rich. Clark, esq. chamb. of London.
Sir John Colpoys, K.B.
John Craufurd, esq.
The duke of Devonshire, K.G.
Andrew Douglas, esq.
The lord bishop of Durham.
The earl of Egremont.
George Ellis, esq. M.P.
Jos. Grote, esq.
Sir Rob. Bateſon Harvey, bart.
Sir John Cox Hippeſley, bart.
Henry Hoare, esq.
Lord Hobart.
Lord Holland.
Henry Hope, esq.
Thomas Hope, esq.
Lord Keith, K.B.
Will. Luſhington, esq. M.P.
Sir John Macpherſon, bart. M.P.

Will. Manning, esq. M.P.
The earl of Mansfield.
The earl of Morton, K.T.
Lord Offulſton.
Thomas Palmer, esq.
The lord viſcount Palmerſton, M.P.
Edward Parry, esq.
Rt. Hon. Tho. Pelham, M.P.
John Penn, esq.
Will. Morton Pitt, esq. M.P.
Sir Ja. Pulteney, bart. M.P.
Sir J. Buchanan Riddell, bart.
Count Rumford.
Sir J. Sinclair, bart. M.P.
Lord Somerville.
John Spalding, esq. M.P.
The earl Spencer, K.G.
Sir George Staunton, bart.
John Sullivan, esq.
Rich. Joſ. Sullivan, esq.
Lord Teignmouth.
John Thomſon, esq.
Samuel Thornton, esq. M.P.
Henry Thornton, esq. M.P.
George Vanſittart, esq. M.P.
Will. Wilberforce, esq. M.P.
The earl of Winchiſlea.
Hon. Ja. Stuart Wortley, M.P.
Sir Will. Young, bart. M.P. *

* The number of proprietary ſubſcribers on the 23d of March amounted to ſeventy-eight.

The present managers are,

For three years.

The earl Spencer.

Count Rumford.

Richard Clark, esq.

For two years.

The earl of Egremont.

Rt. hon. Sir. J. Banks.

Richard Jos. Sullivan, esq.

For one year.

The earl of Morton.

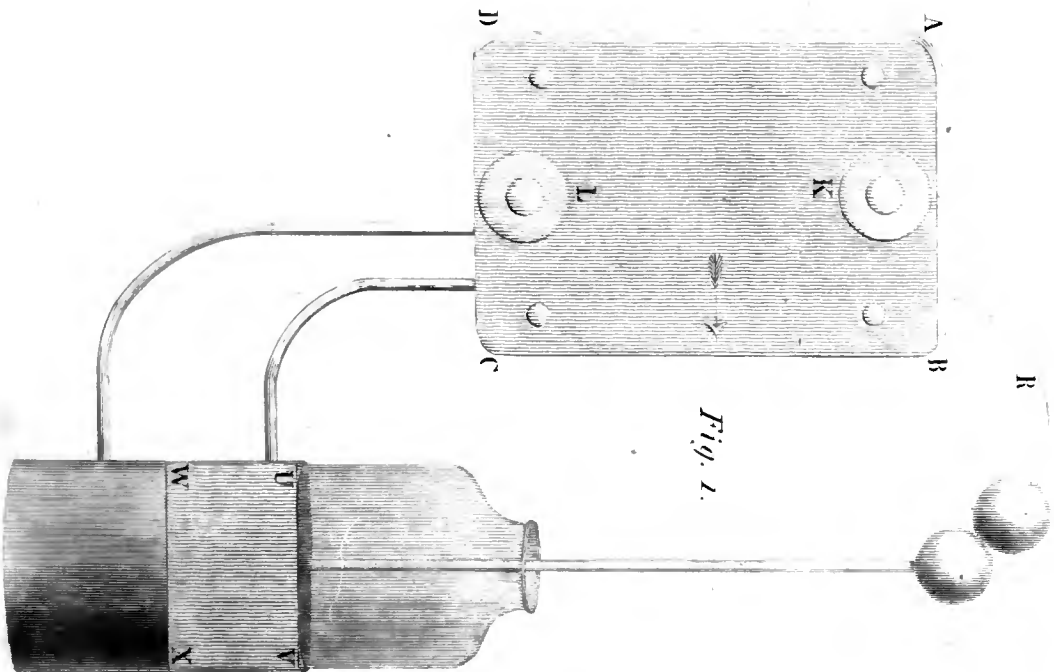
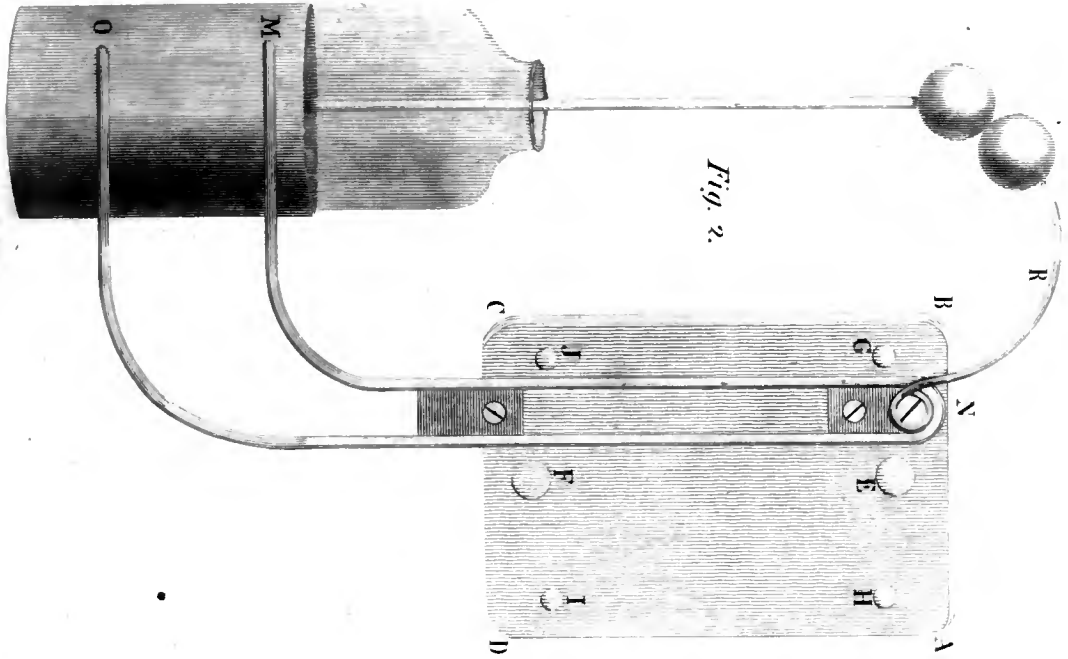
The rt. hon. Tho. Pelham.

Tho. Bernard, esq.

By a resolution of the 9th of March, Sir J. Banks was appointed chairman of all the meetings of the managers which shall be held before a charter shall have been obtained.

At the end of this pamphlet there is a blank form to be filled up by those who intend to subscribe. In this form, as well as in the body of the pamphlet, those who are desirous of becoming proprietors, are requested to consider themselves as candidates for proprietors' places, until they shall have been elected as such by a majority of the managers. But I have since heard, that the regulation is, that every subscriber of 50 guineas, who shall be recommended by a manager, will be admitted of course. Forty per cent of the subscription-money will be wanted immediately, that is to say, after obtaining the charter; and the remainder may be paid in three equal half-yearly payments, unless the proprietor should prefer paying the whole sum at once.

I am much obliged to Mr. Sheldrake, of the Strand, for his offer of a drawing and description of a machine for ruling lines for the use of engravers, which was constructed by him some years ago: but from his general account of the principal parts, namely, a steel rule and a screw, and a ratchet wheel, as wide as from the confined nature of the object, I doubt whether another engraving would be acceptable to the public, however judicious his arrangement may have been.





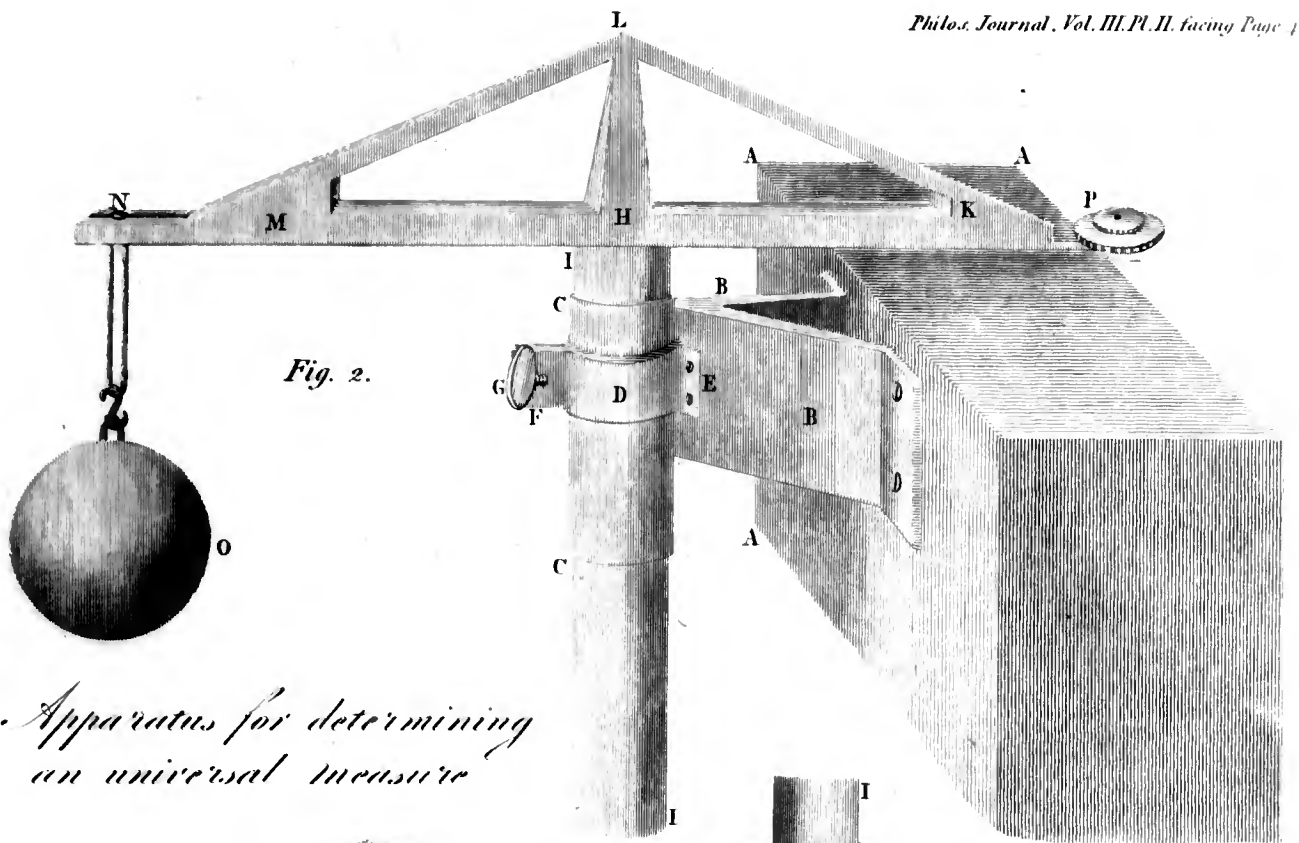


Fig. 2.

*Apparatus for determining
an universal measure*

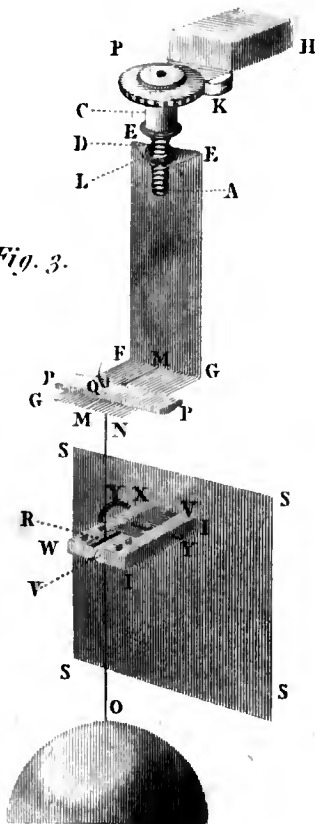
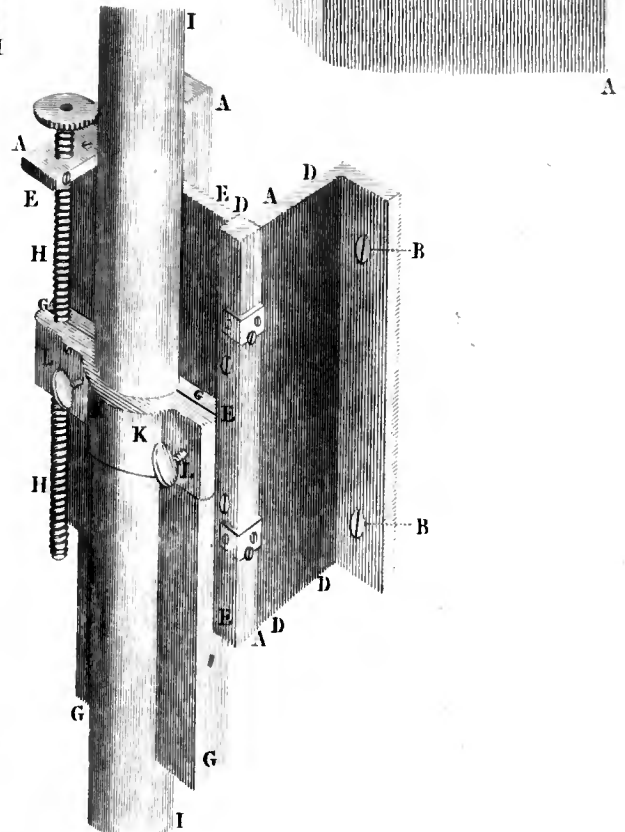


Fig. 3.

Fig. 1.





A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

MAY 1799.

ARTICLE I.

On the philosophical Uses of a common Pocket Watch.—By the Rev. W. PEARSON.

THE theoretic philosopher, who ranges through the variegated fields of science, may gather the sweets as he passes along, and gratify his taste with fruits that he has had no share in rearing, at a very inconsiderable expence; but the pleasure which the practical man of science derives from the results of his successful experiments is much more exquisite: the previous impression made by contemplating the harmony that appears in the laws of nature; the satisfaction of proving the agreement or discrepancy between theory and practice; the prospect of benefiting society in some shape, and, perhaps, also the gratification of a certain degree of vanity, all concur in stimulating his exertions. If he should labour under any inconvenience in procuring the best means of prosecuting his labours, he will naturally avail himself of the best substitutes that the circumstances of his situation will allow. From this consideration it becomes a matter of surprize that a more general attention is not paid to the philosophical uses that may be made of a common Pocket Watch. There are many observations and experiments in different departments of science, the accuracy of which depend greatly, and some of them entirely, on the accurate measurement of *minute* portions of time; such, for instance, as the determination of the velocity of sound, the nature of the descent of falling bodies, the measure of the sun's diameter, the distance of two contiguous, or at least apparently contiguous,

heavenly bodies taken at their passage over the meridian, and the distance of places from the difference of the velocity of light and sound. A pendulum to swing seconds has usually been applied for these and similar purposes, and in an observatory is found to be very convenient ; but a watch by being more portable is calculated to be more general in its application, and will measure *smaller* portions of time than any other instrument that has been invented* ; besides, it possesses this peculiar advantage, that in all situations the *beats* thereof may be counted by the ear, at the *same time* that the object of observation is viewed by the eye, so that no *loss* is incurred, as must inevitably happen, when the eye is used to view both the object and pendulum or second-index in *succession*, though it be ever so quick.—But it will be objected here, no doubt, that few watches measure time accurately, and also that, from the different constructions of watches, the times corresponding to their beats vary in a very considerable degree. I allow these objections to be true, and conceive that the reason may be attributed to them, why the beat of a watch is not generally applied as the measure of the lowest denomination of subdivisions of time : I shall therefore endeavour in this paper to obviate these objections, by shewing how any tolerably good watch, whatever be its construction, may be applied with advantage to many philosophical purposes.

We must, in the first place, consider that the portions of time which I propose to have measured by a watch are *small* portions only, and those to be counted not by a second-hand, as is the custom with medical men, but altogether by the beats ; in which case, if the watch be not liable to lose or gain time considerably in a day, the error in the rate of going will be extremely minute in the time corresponding to any number of beats that the memory can retain, or that the purposes to which I propose the application to be made will require ; and even if the error in the rate of going be considerable, so as to amount to many minutes in a day, as it is uniform, it may easily be allowed for by a correction†. Hence the first objection, which relates to the error occasioned by the rate of going of any watch, will constitute no real obstacle to its application in the ascertaining of small portions of time, provided a sudden change of temperature be avoided at the time of using it ; for it will be necessary that the rate of going be estimated when the temperature is the same, or very nearly the same, as when the watch is used for philosophical purposes ; so that if it is usually worn in the pocket, it may be held in the hand to the ear, but if it be hanging in a room or in the open air where the rate of going is ascertained, it must be hung near the ear, under similar circumstances, where any observation is intended to be made by it.

As to the other objection, which applies to the variation in the lengths of the beats of two different watches, owing to the difference of their constructions, though they indicate hours and minutes alike, it may be removed very readily. All common watches have the same num-

* The beat of a watch is quicker than that of any other chronometer in general use ; but there have been instruments made to divide the second into 100 parts. One of these, made by Whitehurst, and regulated by a fly, repeatedly measured the time of fall of a leaden bullet (in some experiments which I saw) with no greater variation than one hundredth part of the second.—N.

† If the error were five minutes per day, the allowance would be less than one three-hundredth part.—N.

ber of wheels and pinions, which are known by the same names, and placed, no matter how variously, so as to act together without interruption; but all watches have not their corresponding wheels and pinions divided into the same number of teeth and spaces, and to this circumstance it is entirely owing that the beats of different watches differ from each other. As the rate of going of a watch is regulated by the lengthening or shortening of a spring, without any regard being paid to the numbers which compose the teeth of the wheels and pinions, a great latitude is allowable in the calculation of those numbers; of which the different makers avail themselves according as the numbers on the engines they use for cutting the teeth require: but whatever the numbers may be of which the wheelwork consists, if we divide double the product of all the wheels, from the centre wheel to the crown wheel inclusively, by the product of all the pinions with which they act, the quotient will be invariably the number of beats of the watch in question in one hour; and again, if we divide this quotient by 3600, the number of seconds in an hour, this latter quotient will be the number of beats in every second, which may be carried to any number of places in decimals, and be copied upon the watch paper for inspection whenever it may be wanted.

When any particular watch is cleaned, the workman may be directed to count, and return in writing, the numbers of the centre wheel, the third wheel, the contrate wheel, and the crown (balance) wheel, and also of the three pinions which they actuate, respectively, from which the calculation of the length of a beat is easily made by the rule just given, and when once made will apply in all instances where that individual watch is used. It will be remarked here, that no notice is taken of the wheels and pinions which constitute the *dial-work*, nor yet of the *great wheel* and pinion with which it acts: the use of the former of these is only to make the hour and minute hands revolve in their respective times, and may or may not be the same in all watches; and the use of the latter, the great wheel and its pinion, is to determine, in conjunction with the number of spirals on the fusee, the number of hours that the watch shall continue to go, at one winding up of the chain round the barrel of the main spring: all these wheels and pinions therefore, it will be perceived, are unnecessary to be taken into the account in calculating the beats per hour. The reason why *double* the product of the wheels specified is taken in the calculation is this, that one tooth of the crown wheel completely escapes the palats at every two beats or vibrations of the balance. A few examples will render the general rule perfectly intelligible. Let us take for the first example the numbers of a common watch given by Mr. Emerson in his "Tracts," which, according to his method of arrangement, stand thus,

48 great wheel,

12—54 centre wheel

6—48 third wheel

6—48 contrate wheel

6—15 crown wheel

2 palats.

H 2

Now,

Now, omitting the great wheel and its pinion of 12, we have $54 \times 48 \times 48 \times 15 \times 2 = 3732480$ for double the product of the specified wheels, and $6 \times 6 \times 6 = 216$ for the product of the specified pinions; also $\frac{3732480}{216} = 17280$ are the number of beats in an hour, and $\frac{17280}{3600} = 4.8$ the exact number of beats per second: accordingly, Mr. Emerson says that this watch makes "about $4\frac{1}{2}$ beats in a second." The number of spirals on the fusee is 7; therefore, $7 \times \frac{48}{12} = 28$ is the number of hours that the watch will go at one winding up: likewise the dial-work $\frac{40}{10} \times \frac{36}{12} = \frac{1440}{120} = 12$ shews that whilst the first driving pinion of 10 goes twelve times round, the last wheel of 36 goes only once; whence the angular velocity of two hands carried by their hollow axles are to each other as 12 to 1.

For a second example, I will take a watch which is in my own possession, the numbers of which in the calculation of beats per second will be thus: $60 \times 60 \times 60 \times 13 \times 2 = 5616000$, double the product of the wheels; and $8 \times 8 \times 6 = 384$ the product of the pinions; then $\frac{5616000}{384} = 14625$ will be the beats in an hour, and $\frac{14625}{3600} = 4.0625$, the beats per second.

Besides this I have examined two other common watches, one of which requires this calculation: $54 \times 52 \times 52 \times 13 \times 2 = 3796416$ for double the product of the wheels, and $6 \times 6 \times 6 = 216$ for the product of the pinions; therefore $\frac{3796416}{216} = 17576$ are the beats in an hour, and $\frac{17576}{3600} = 4.882$, the beats per second by this watch: also double the product of the wheels of the other, viz. $56 \times 51 \times 50 \times 13 \times 2$ is 3712800, and the product of the pinions, as in the last, $6 \times 6 \times 6 = 216$; consequently $\frac{3712800}{216}$ gives 17188 beats in an hour, which divided by 3600 gives 4.7746 for the beats per second.

These four examples, it is presumed, will render the method of ascertaining the beats per second in any watch sufficiently easy for any person who is acquainted with common arithmetic.

It remains now for an instance or two to be adduced for the application of the beats of a watch to philosophical purposes, in order to shew the practical utility of the method here proposed of measuring very small portions of time.

Let us suppose, for one instance; with Dr. Herschel, that the annual parallax of the fixed stars may be ascertained by observing how the angle between two stars, very near to each other, varies in opposite parts of the year. For the purpose of determining an angle of this kind, where an accurate micrometer is wanting, let a telescope that has cross-wires be directed to the stars when passing the meridian, in such a manner that the upright wire may be perpendicular to the horizon, and let it remain unmoved as soon as the former of the two stars is just coming into the field of view; then fixing the eye to the telescope and the watch to the ear, repeat the word *one* along with every beat of the watch before the star is arrived at the perpendicular hair, until it is in conjunction with it, from which beat go on *two, three, four, &c.* putting down a finger of either hand at every *twenty* till the second star is seen in the same situation

that

that the leading one occupied at the commencement of the counting; then these beats divided by the beats per second marked on the watch-paper, will give the exact number of uncorrected seconds, by which the following star passes later over the meridian than the leading one: when these seconds and parts of a second are ascertained, we have the following analogy for determining the angle, which includes also the correction, namely, as the time of a sidereal rotation of the earth (which at a mean rate* may be taken at 23h. 56' 4",098, but more or less accordingly as the earth is near the aphelion or perihelion of its orbit), the daily error in the rate of going, is to 360°, so is the observed number of seconds (of time) to the quantity of the required angles. The watch is here supposed to be regulated to shew solar time; but if it should be regulated exactly for sidereal time, instead of 23h. 56' 4",098 we must use exactly 24 hours in the analogy.

As a second instance, let it be required to ascertain the distance of the nearer of two electrified clouds from an observer, when there are successive peals of thunder to be heard: a little time before the expected repetition of a flash of lightning place the watch at the ear, and commence the numbering of the beats at the instant the flash is seen, as before directed, and take care to cease with the beginning of the report; then the beats converted into seconds, with the proportional part of the daily error added or subtracted, will give the difference of time taken up by the motion of the light and sound: if, lastly, we suppose light to be instantaneous at small distances, the distance of the nearer cloud will be had by multiplying the distance that sound is known to pass through in a second by the number of observed seconds obtained from the beats that were counted.

Many more instances might be here pointed out, in which the beats of a good watch would be extremely serviceable in the practical branches of philosophy; but the occurrence of such instances will always point out the propriety of the application, when it is once known and practised.

I shall therefore only mention one further advantage which seems peculiar to this mode of counting a limited number of seconds by a watch, which is, that it is free from any error that might arise from the graduations of a dial-plate, or unequal divisions in the teeth of wheels and pinions, where the seconds are counted by a hand.

In order to introduce this method of measuring small portions of time accurately, it is desirable that a watch be constructed so as to make an exact number of beats per second without a fraction, for then the reduction of beats into seconds would be more readily made. With a view of promoting this object, I have calculated numbers for a watch, which will produce the desired effect, and which, as they are equally practicable as those in use, I shall insert in this paper, in hopes of hearing at some future period, that they have been adopted by some good workman. By the method of arrangement already given, the numbers proper for such a watch, as will indicate hours, minutes, and seconds, by three hands, and also make just four beats per second, will stand thus, viz.

* I am not aware of any *observable* variation in the time of the earth's rotation. In theory it may vary from the variable disturbing forces of sun and moon, as well as from other causes which may affect its mean diameter, See *Philos. Journal*, I. 198.—II. 40.—and III. 30.—N.

50 great wheel

10—60 centre wheel

8—64 third wheel

8—48 contrate wheel

6—15 crown wheel

2 pivots.

Dial-work as usual.

Six spirals on the barrel—to go 30 hours.

By the preceding general rule for ascertaining the beats per second in any watch, the calculation of these numbers will be thus: $60 \times 64 \times 48 \times 15 \times 2 = 5529600$, and $8 \times 8 \times 6 = 384$; then $\frac{5529600}{384} = 14400$, the beats in an hour, and $\frac{14400}{3600} = 4$ exactly, for the beats per second; which agreement with the rule is a proof of the accuracy of the numbers.

Whilst I am upon this subject, I shall take the liberty of cautioning medical gentlemen against an imposition which some, I hope not many, watchmakers practise towards them in the sale of watches; and I the more readily make this caution public, because the health of thousands of individuals is connected with the imposition, which is this, that a second-hand, with a stop, and an appropriate face, are sometimes put to a watch, the wheelwork of which is not calculated to indicate seconds. The watch which is the second mentioned in this paper as being in my own possession, is one of this kind; I bought it of a clock-maker, who had it made in town, with his own name enamelled on the face, but unfortunately I kept it too long before its imperfection was discovered, so that I am now under the necessity of using it. Upon enquiry, I found that more of the same kind have been sold to medical gentlemen and others for the purpose of ascertaining the number of pulsations of invalids in a minute, in order that they may be treated accordingly. At first I suspected that the disagreement in the motions of the second and minute hands, which I observed might be owing to some inequality, or shake, as the workmen call it, in the teeth and spaces of the wheelwork; but upon counting the numbers I afterwards detected the real cause; that part of the train which lies between the axle of the centre wheel and the axle of the contrate wheel, on which the hands are placed, viz. $\frac{60}{8} \times \frac{60}{8}$ is equal to only 56,25 instead of *60, so that $3\frac{3}{4}$ seconds are in defect in every minute, which is equal to a whole revolution of the second-hand in every 16 minutes: hence, if the pulsations of any patient in a fever were really 120 in a minute, the determination by the second-hand of the watch in question would be only 112 $\frac{1}{2}$, consequently the judgment of the physician or apothecary would be proportionably biased in drawing a conclusion from the pulse upon the state of the fever, and would undoubtedly prescribe medicines accordingly. It is to be hoped, therefore, that the observations here made upon the construction of a watch may, exclusively of philosophical purposes, prove useful in directing the choice of such gentlemen as may have occasion to purchase a stop-watch, and conse-

* If a wheel of 64 be substituted for either of those of 60 each, the seconds will be truly indicated.—P. frequently

quently may obviate the deception which has hitherto been practised by certain makers and vendors of watches.

Lincoln, April 5th, 1799.

II.

Letter from Mr. H. DAVY, introductory to the Experiments contained in the subsequent Article, and on other Subjects relative to the Progress of Science.

SIR,

I SEND you for your Physical Journal, experiments and observations on the flint contained in the epidermis and other parts of certain vegetables: these experiments have been made within the last fortnight. I am induced to make them public so speedily, from the hopes that others, who have greater opportunities and more leisure, may be stimulated to further researches on this interesting subject. I doubt not that other plants will be found to contain flint, as well as the reeds, the canes, and the grasses.

In the Pneumatic Institution we have lately made some experiments on the nitrous phosoxyd (gaseous oxyd of azote), the principle of contagion of Mitchill. When it is mingled with $\frac{1}{2}$ of phosxygen (oxygen gas), animals live in it without suffering any injury. I have made two inspirations of it pure, without any disagreeable effects. I have breathed it mingled with an equal quantity of phosxygen (oxygen gas) for some minutes; the effects produced by it were very peculiar: should they be confirmed by future experiments, it will probably prove a valuable medicine.

In my essay on heat, light, &c. in the West-country Contributions, p. 101. ob. 4, an inaccuracy occurs with respect to the production of this gas. It is said "no light is produced during the decomposition of phosnitrate of ammoniac by heat." In the experiments from which that observation was deduced, no luminous appearance was visible, as the phosnitrate was mingled with a large quantity of siliceous sand, and slowly decomposed by the heat of an Argand lamp. Lately I have made the experiment in the large way: when the heat is quickly applied and the phosnitrate mingled with but a small quantity of sand, a vivid luminous appearance is uniformly perceived. A paper on the nitrous phosoxyd will appear in the next volume of Dr. Beddoes' contributions.

With the hopes of discovering a cheap substitute for nitre, I have lately made the phosmuriates (oxygenated muriates) of strontian and barytes. The properties of the first correspond with the account I have given of them, in the West-country Contributions, art. Combinations of the muriatic Phosacid, except that its solubility is not so great as I at first suspected. The phosmuriate of barytes crystallises in plates, and detonates very slightly with charcoal and phosphorus. A solution of it in water, like that of the phosmuriate of strontian, becomes luminous when the sulphuric acid is poured into it.

I would wish to observe, that no affectation of singularity induces me to use a new nomenclature. Theory or arrangement of facts depends altogether on language, i. e. in applying certain

certain general terms to a number of similar ideas, or similar trains of ideas; consequently there cannot well be a new theory without a new nomenclature. Till my experiments on heat and light are rendered inconclusive, by numerous contradictory facts, or experiments, I consider myself as entitled to use my present nomenclature for the combinations of light.

Dr. Beddoes desires me to inform you, that the Pneumatic Institution, so far as we can yet judge, promises to do well. We were apprehensive that prejudice would prevent applications; but no sooner was its opening notified to the people, than they began to resort to it. We have already upwards of fifty patients. By degrees we shall provide apparatus for trying, in every way, such gases or vapours as promise any thing, or ought to be tried. He requests further, that you would notify that a considerable error has been discovered in the copy from which page 381, line 9, of the "West-country Contributions" was printed: it should be "calomel, three grains," instead of "calomel, eight grains." I remain, Sir, with wishes for the success of your academical undertaking,

Yours, with great respect,

HUMPHRY DAVY.

Clifton, April 111b.

TO MR. NICHOLSON.

III.

Experiments and Observations on the Siliceous composing the Epidermis, or external Bark, and contained in other Parts of certain Vegetables. By HUMPHRY DAVY.

D. 1. A FEW days ago, Mr. Coates, of Clifton, informed me that his son, accidentally rubbing two pieces of bonnet-cane together, in the dark, had perceived a luminous appearance. This phenomenon was sufficiently novel and curious to induce me to examine it. I found that all canes of this kind, when briskly rubbed together, produced sparks of white light. The luminous appearance was much more vivid on collision. When the canes were violently struck together, sparks nearly as vivid as those from the gun-lock were produced. At the same time a strong smell, similar to that generated by the collision of flint, or the excitement of the electric fluid, was perceived.

D. 2. I first thought that the phenomenon was electric, and depended on some resinous matter in the cane. The electrometer, however, was not sensibly affected during the experiment. When the cane was struck against wood of any kind, no light was perceived. When a cane was struck violently against quartz, agate, or any siliceous stone, the light was as brilliant as when two canes were struck together. The luminous appearance was produced when sharp steel was struck against the cane. When the cane was struck against sulphate of strontian, or barytes, or carbonate of lime, no light appeared.

D. 3. These circumstances induced me to suppose that the phenomenon depended on siliceous earth in the epidermis, or in the whole of the cane. To determine this, I took off a small quantity of epidermis from one of the canes. It was hard, white, and had something the

the appearance of pulverized glass. When the epidermis was removed, the canes no longer possessed the property of giving out light on collision.

D. 4. To ascertain, with certainty, the nature of the epidermis, I obtained from 280 grains of cane 22 grains of epidermis; this was exposed, in a crucible, to the strong heat of an air-furnace for half an hour. It had lost three grains, was very white, infusible by the heat of the blow-pipe, and insoluble in any of the mineral acids. Ten grains of it were kept in fusion with caustic potash, in a silver crucible, for a quarter of an hour. The compound was white and semi-pellucid. It was perfectly soluble in water, without communicating to it any turbidity. When muriatic acid was poured into the aqueous solution, a copious white flocculent precipitate was produced. This precipitate collected, weighed about nine grains, and had every property of silex.

D. 5. To determine whether the wood and internal bark of the cane contained any silex, I burnt 240 grains, carefully deprived of the epidermis, for an hour. The ashes were perfectly white, and weighed about seven grains. When muriatic acid was poured upon them, a portion was dissolved with effervescence. This portion was chiefly carbonate of potash; the insoluble part, collected, weighed about two grains, and was apparently silex.

D. 6. Having ascertained, by these experiments, that the epidermis of the bonnet-cane was chiefly composed of flint, and that the luminous appearance above mentioned depended on this composition, I thought it probable that the other canes, particularly the sugar-cane, *arundo saccharifera*, and the bamboo, or *arundo indica* *, were similar in their organization. When two bamboos were struck together, I could perceive no luminous appearance. Four ounces of this cane only afforded seven grains of true epidermis. This exposed to a strong heat, left five grains of white matter, which had all the properties of flint. The reason why these canes produce no light, on collision, is, that the flint of the epidermis is too small in quantity, and too thinly diffused. The epidermis of the sugar-cane contained a still smaller proportion of flint: 200 grains of this gave five grains of white ashes, of which only one grain was insoluble in muriatic acid: the four grains of soluble matter appeared to be carbonate of lime. A large piece of bamboo (the weight of which I am ignorant of) deprived of the epidermis, gave a considerable quantity of white ashes, of which about two-thirds were soluble in the muriatic acid: the insoluble part was silex. The ashes of the sugar-cane, deprived of the epidermis, appeared to be chiefly composed of carbonate of lime, and carbonate of potash.

D. 7. The analogy between the English reeds and grasses, and the canes, and particularly the similarity of the appearance of the epidermis, induced me to suppose that they might likewise contain silex. On this supposition I first examined the *arundo phragmites*, or common reed. It produced no luminous appearance on collision with flint. Twenty-seven grains of the epidermis, exposed to a strong heat, gave 13 grains of white earthy matter, insoluble in

* I am aware that the ingenious Mr. Macie discovered the tabasheer, found in the bamboo, to be chiefly composed of flint. He, however, did not absolutely discover it in the wood of that cane; and no one, to my knowledge, has ever suspected its existence in the epidermis of any vegetable.—*D.*

the mineral acids. Ten grains of this was fused with 34 grains of potash. The compound was soluble in water. The nitrous phosacid threw down from the aqueous solution a white flocculent matter, which was necessarily flint; this matter I did not weigh, but I conjecture that it was about seven or eight grains. One hundred and ten grains of the reed from whence the epidermis was removed, gave about six grains of flint.

D. 8. I now examined the culm of wheat: 200 grains, burnt, gave 31 grains of white ashes; of these 18 grains were soluble, with effervescence, in the muriatic acid. The remainder had all the properties of silex. The matter dissolved in the muriatic acid was potash. The ashes of oats and barley afforded silex in nearly the same quantities as those of wheat. The culms of the grasses, among which I examined *anthoxanthum*, *poa pratensis*, and some others, appeared to contain more silex in the epidermis, than even the corns with a much larger proportion of carbonate of potash.

D. 9. The silex in all these vegetables, as in the canes, appeared to be contained in the epidermis, or in the second bark. When the plants are carefully burnt, the figure of the epidermis is preserved. In the cane, when well burnt, it has a white glossy appearance, and is semi-transparent. In the reeds, corns, and grasses, it is white and opaque, and when viewed through a magnifier, appears to consist of longitudinal threads joined together by net work. In the microscope, even the smallest particles have a distinct reticular appearance.

D. 10. The quantity of carbonate of potash in the ashes of the corns and grasses, induced me to suppose that, in a strong heat, they might be fused into glass. The ashes of the *arundo phragmites* were exposed to the strongest heat of an air-furnace for some minutes; there was no appearance of fusion; the carbonate of potash was not sufficient to form glass with the siliceous earth. The ashes of straw, in a strong heat, gave a fine white transparent glass, perfectly insoluble in water, and indecomposable by acids. The ashes of hay gave a black glass, with a superabundance of potash. This conversion of corn and grass into glass, may be effected by the blow-pipe, and affords a pleasing experiment. A straw burnt with the blow-pipe, and urged with the strong heat of the blue flame, beginning at the top, is converted into a fine pellucid globule of glass almost fit for microscopic experiments. A culm of grass is fused under the blow-pipe into a globule of glass, black and opaque, probably from its containing iron.

D. 11. These facts will afford some curious inferences to the speculator on organized nature. The flint entering into the composition of these hollow vegetables, may be considered as analogous to the bones of animals; it gives to them stability and form, and by being situated in the epidermis, more effectually preserves their vessels from external injury. They will probably enable us to determine whether silex be a simple or a compound substance. Reed or wheat might be easily made to vegetate deprived of silex. Confined under mercury, in a soil composed of known quantities of the saline earths, and supplied with distilled water and factitious air, we might discover whether it would compose silex, or substitute for it another earth. This experiment, as well as some others on the same subject, we propose to make, if leisure and opportunity occur. The numerous complex attractions of organic beings, resulting from
their

their variety of composition, are continually producing changes which the art of the chemist is unable to imitate. Though the chemist, at present, can extend his power no further by the simple attractions and repulsive motions of inorganic matter, yet a path of science, displaying a boundless field for investigation, seems now open in the changes effected in dead matter by living beings. By discovering those changes, we might go far towards discovering the laws of their organization.

IV.

Analytical Table of the Results of the Course of Experiments on the lateral Communication of Motion in Fluids. By Citizen J. B. VENTURE, Professor of experimental Philosophy at Modena, &c. drawn up by the author himself.*

DESCRIPTION of the apparatus, page 172.—It is proper to mistrust all theories of Hydraulics, not excepting that which is exhibited in the present work, excepting so far as those theories may be supported by experiment, p. 173.

In any fluid, those parts which are in motion carry along with them the lateral parts which are at rest. Prop. I. exper. 1. *ibid.*—I call this phenomenon *the lateral communication of motion*; and I consider it as a principle of experiment, or elementary fact, without explaining its cause, 174.—If water be drawn out of a vessel by an horizontal cylindrical pipe, of which the part nearest the vessel is contracted according to the form of the contracted vein of water which flows through an equal orifice in a thin plate, the expenditure will be increased by this pipe, in the same manner as if there had been no contraction. Prop. II. exper. 3 and 4. *ibid.*—The velocity of the stream, within this tube, is greater than that of the jet through a thin plate, p. 175.—The increase of expenditure of water through an horizontal cylindrical pipe, whether it be of uniform diameter throughout or contracted at the end next the reservoir, is caused by the pressure of the atmosphere. Prop. III. exper. 5, 6, 7, *ibid.*—This increase of expenditure, through a pipe, does not take place in the vacuum of the air pump. Exper. 8. p. 176.

When water is drawn through a descending cylindrical tube, of which the upper part is of a divergent form, answerable to that of the contracted vein, the expenditure will be that which is answerable to the height of the charge above the lower orifice of the tube. Rectification of the theory asserted on this subject by Guilliellini, and adopted by various philosophers.—Experiments. prop. IV. exper. 9, 10, 11, 12. p. 178.—The lateral communication of motion in fluids is the cause which excites the pressure of the atmosphere to increase the expenditure and internal velocity in horizontal conical tubes of a certain form. Prop. V. p. 273.—Experiments relative to this augmentation—the result always falls short

* And subjoined to his treatise.—The paginal numbers refer to the second volume of this Journal, except where the Number III. is prefixed, which of course implies the present volume.

of theory—cause of this defect. Exper. 13, 14, 15. p. 275.—Limits of the augmentation of expenditure in the same horizontal conical tube. Exper. 16, 17, *ibid.*—In horizontal cylindrical tubes, the increase of expenditure does not approach the maximum so nearly as in conical tubes. Prop. VI. exper. 18, p. 423.—Case of the simple cylindrical tube more particularly examined, *ibid.*—The Velocity of the fluid stream issuing through a tube is less than that which flows through an orifice in a thin plate. Cause of this difference. Exper. 19, p. 424.—The same law, and the same causes, also determine the expenditure through ascending and descending tubes. Exper. 20, 21, *ibid.*—The effect of the lateral communication of motion is produced in a very short portion of the length of the inner cavity of the cylindrical tube. Exper. 22. p. 425.—The effect itself is greater than could have arisen from the mutual attraction of the particles of the fluid, *ibid.*—The expenditure which takes place through a cylindrical tube of given dimensions, and under the same charge, may be increased to nearly double by proper adjustages. Prop. VII. *ibid.*—Roman law relative to this object, p. 426.—Application of the same law to the flues of chimnies. *Ibid.*

How far elbows and sinuosities diminish the expenditure through tubes. Exper. 23. p. 487.—Loss of expenditure occasioned by enlargements or inflated parts in tubes. It is necessary to have regard to this in the construction of hydraulic machines. Exper. 24. p. 488.

In the machine for blowing by means of a fall of water, the air is afforded to the furnace by the accelerating force of gravity, and the lateral communication of motion combined together. Prop. VIII. *ibid.*—Wind produced by falls of water in the internal parts of mountains, p. 489.—The wind of the water-blowing machine is not produced by the decomposition of water. Exper. 25. p. 490.—Quantity of wind which one of these machines is capable of affording in a given time. *Ibid.*

It is possible, in certain circumstances, by means of a fall of water, to drain a piece of ground without the help of machines, even though the ground should be on a lower level than the established current below the fall. Prop. IX. p. 491.—Application of the same principle to the tail water of mills. *Ibid.*

The eddies of water in rivers are produced by motion communicated from the more rapid parts of the stream to the lateral parts which are less rapidly moved. Prop. X. p. 492.—Vertical Eddies at the surface and at the bottom of the stream of rivers. *Ibid.*—These circular motions constitute one of the principal causes of the loss of active force, and the retardation of the current of rivers, p. 493.—In a river of which the course is permanent, and the sections of its bed unequal, the water continues more elevated than it would have done if the whole river had been equally contracted to the dimensions of its smallest section. *Ibid.*

Whirling motions, or eddies, formed in a reservoir from which the water issues by an horizontal aperture. Theory deduced from the doctrine of central forces. Prop. XI. vol. III. p. 13. The cavity of these whirls is convex on the side next the axis. III. p. 14.—Phenomena relative to these whirls, and their explanation. Exper. 26, 27, 28, 29, 30. III. p. 15.

The lateral communication of motion takes place in the air as well as in water. Prop. XII.

III. p.

III. p. 16.—The excitation of sound in organ-pipes is effected by this communication. *Ibid.* The same cause augments the force of sound in conical divergent pipes. *Ibid.* p. 17.—Remarkable differences between the resonant vibrations of the air in a pipe, and the pulsations propagated in the atmosphere. *Ibid.*

The contraction of the fluid vein which issues through a thin plate, is not the Newtonian cataract. III. p. 18.—The velocity of the contracted stream through a thin plate is nearly the same as that of a heavy body which may have fallen through the height of the charge. III. p. 19.—Singular form of the stream which is emitted through a long hole or cleft. Exper. 31. III. p. 20.—In right-lined orifices the sides of the contracted stream answer to the angles of the orifice, and the contrary. Cause of this phenomenon. *Ibid.*—The contraction of the fluid vein is made at a greater distance under a strong charge than when the charge is weak. Exper. 32. III. p. 21.—Other varieties in the figure and velocity of the contracted stream. Exper. 33 and 34. *Ibid.*—Expenditure through a tube, the inner extremity of which is thrust into the cavity of the reservoir itself. Exper. 35. *Ibid.*

V.

A Memoir on the Typographic Art, by Alexis Rochon, of the French National Institute, and Director of the Marine Observatory at the Port of Brest.

TYPOGRAPHY is the art of multiplying copies. Amongst the processes to which the improvement of this art has given rise, there is one for ever memorable in the records of science and of art. This simple designation is sufficient to point out the invention of printing; and the reader will be ready to observe, that it is unnecessary to describe the utility of this happy expedient, the fruitful and inexhaustible source of mental illumination, the ever-durable register of all nations. Men of information well know, that the notions acquired by reading constitute the basis of all our science; and it is to the art of printing that we are most indebted for these acquisitions.

Henceforth the precious deposit of our sciences and arts is preserved, and nothing short of universal convulsion can destroy it. This wonderful art gives to man the faculty of transmitting the result of his enquiries to his fellow men, however dispersed over the surface of the globe, or even though removed to those periods of duration which shall be occupied by our remotest posterity. If this precious invention had been known to the ancients, we surely should not have to regret, at this moment, the numerous master-pieces of composition, and the many useful arts which time has for ever covered with obscurity.

Let those who shew an ignorant contempt for the mechanic arts, direct their attention to this, and repent their errors. Let them renounce those unhappy prejudices which tend to discourage the numerous and respectable class of industrious artists. Much sagacity has certainly been required to carry our arts to the degree of perfection they at present possess. I will say more, and bring the art of printing in support of my assertion.—One single happy discovery
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is sufficient to change the face of the universe. Let us leave those men who have more erudition than genius to seek the origin of printing in the ages of antiquity. They are ignorant that the great advantage of this art consists still more particularly in casting the characters, than in their mobility; for which reason we join in the opinion of those who affirm John of Gottenburgh, Furst, and Schoeffer, made the discovery in 1439. We must, however, observe, that Schoeffer, the clerk or principal workman of the goldsmith, Furst, was the inventor of moveable types, according to the learned Trithemius, author of the *Chronicle Herfanges*, who was particularly acquainted with Schoeffer. Trithemius was so well satisfied with this process, that he gave his daughter in marriage to Schoeffer. But whatever may be the decisions respecting this point of history, the immediate object of my memoir will not allow me to enter into any further discussion respecting the degrees of perfection which this art has successively acquired since that memorable epocha. I cannot, however, pass over in silence a method invented in Scotland, by William Ged, to make fixed plates with moveable characters. By means of these plates he printed an edition of Sallust, with the title "*G. Crispi Sallustii Catilinarum & Jugurthinum Historiarum. Edinburgi, Guill. Ged, Auri Faber edinensis non typis mobilibus ut vulgo fieri solet, sed tabellis seu laminis fusi excudebat, 1744,*" in 16mo.

The Sallust of Ged is well printed, and resembles, in every respect, a book printed with moveable characters. But Ged reduced his printing characters to a moderate price, and obtained, besides, a much greater saving on the paper, because he printed no greater number of copies, at a time, than he had reasonable ground to expect he should sell. His process is more particularly advantageous for classical books, which are of slow, though steady, sale. These Polytype plates, as they are now called, are likewise capable of affording a considerable profit to the founder, who may make them, for the purpose of exportation or sale, at great distances from the place of fabrication. Hoffman, an industrious artist, communicated, in 1784, to the French Academy, a process similar to that here described; but he applied his art to the composition of a journal, for which it was little calculated. If we could flatter ourselves with making any addition to the fame of the Didots, we should not fail to make honorable mention of their labours. But the object of the present communication is less the art of printing, or impression, than of typography. Typography embraces the generality of the art of multiplying copies, whether the plates present an engraving in relief or excavated, and whether the characters be fixed or moveable. It will not be contended, but that the art of typography was very anciently known in China. Duhalde, and every other writer who has treated of the industry of that nation, informs us, that they transfer to a block, of apple or pear tree, the work of which they are desirous of multiplying copies, by first pasting a written copy, upon thin paper, on the face of the block; through which they trace the marks of the writing with a point, and afterwards clear away the blank parts with the graving-tool. Duhalde observes, that this method of procuring copies has the great inconvenience of requiring an excessive multiplicity of blocks for such works as are voluminous. But how could it be possible for the Chinese to make use of moveable cast types, since they have near one hundred thousand characters. Nevertheless, according to the same author, this people are not ignorant of the
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use of moveable characters, because they make them in wood, and use them to correct the Account of the State of China, which is printed at Pekin every three months. Duhalde does not here seem to be aware of the inestimable advantage of casting types. The very celebrated Dr. Franklin, who was long a printer at Philadelphia, shewed me some essays which he had made for speedily multiplying the copies of his own hand-writing. I do not here speak of those English presses which answer the purpose of a copyist, but of a process which gave birth to that notion, a considerable time afterwards. This method consisted in writing upon smooth paper, with ink containing much gum, which was afterwards sanded with emery, or powder of cast-iron, and by means of a rolling-press, such as is used by the copper-plate-printers, the strokes of the writing were transferred to a plate of rose-copper, or pewter. This plate affords as many copies as the depth of the engraving can permit; but it must be confessed, these copies are very far from being beautiful, and the ground is spotted and soiled. Though Franklin did not immediately communicate his process to me, I shewed him, before the illustrious Turgot, that by writing with a steel point on a copper-plate, previously varnished, a more satisfactory result might be obtained by etching the strokes with nitric acid to a sufficient depth for the subsequent use of a liquid ink similar to that of the printers. In this case the plate may be wiped without precaution, and twelve or more copies may be pulled off upon coarse paper. These proofs are foul and reversed; whence, in order to have them neat and in the proper direction of the writing, it becomes necessary to place the same number of leaves of white paper, wetted and prepared, upon the twelve proofs, and, while the ink is still fresh, the whole being passed together through the rolling-press, the same number of counter-proofs are obtained as there were proofs: so that instead of twelve turns of the press, thirteen will be required to afford twelve counter-proofs, very black, neat, and legible, even when the plate has not been perfectly well wiped. This method is certainly not to be compared with fine engraving; but it may be useful in military operations, and all other cases in which a speedy multiplication of copies is required. No precaution is here necessary: whether the nitrous acid be more or less strong, or remain a longer or shorter time upon the plate, or whether the plate be somewhat heated to increase the strength of the solvent, the success of the operation will never fail; provided the steel point, made use of to trace the characters through the varnish, shall lay the copper perfectly bare. It is of advantage that the nitric acid should bite deep, because the counter-proofs are, by this means, much darker. The plate need not be well wiped, because it is of no consequence whether the proof which is used to afford a counter-proof should be very clean, provided it do not spot the copy intended to be procured. The most liquid kind of printers' ink may be made use of.

The art of multiplying copies by a copper-plate engraved by excavation, appears to have been an invention of the fifteenth century. The Italians and Germans contend for the honour of this discovery. This art, which is particularly distinguished by the name of engraving, is, nevertheless, more generally attributed to a goldsmith of Florence, named Thomas Sini-guerra. It is more especially used to transmit to posterity copies of paintings and designs of great masters. Engravers, or rather copper-plate-printers, have, in certain circumstances,
made

made use of counter-proofs; but they are little esteemed. These counter-proofs have put me in possession of a method of procuring, with celerity, a number of copies of a manuscript of no great extent; and I can affirm that this application of the art has not been without considerable utility. Interesting memoirs, of which the impression had been prohibited, were printed in this manner. The same process has also been used in the publication of flying gazettes. Without attaching any particular importance to a process of such easy invention after what was previously known, I shall only add, that it may be usefully applied in all cases where a printing-press cannot be had recourse to. The process here described may be also found in a work which I published in 1783, on mechanics and natural philosophy. They are contained in a memoir, entitled, "*Description d'une Machine à graver.*"

It was the illustrious Turgot who engaged me, after these first essays, to turn my attention to the mechanical method here described. The restraint under which the liberty of the press then laboured, caused this great man to wish that authors should be able to compose and print their works under their own inspection. The learned Morellet, who became strongly interested in this project, gave me some punches, which I used in my first attempts. If I have not entirely accomplished the wish of Turgot and Morellet, I have, nevertheless, produced a machine which has contributed to the perfection of paper-money, and will probably be useful, hereafter, in the process which God has pointed out for the composition of classical works. It would, no doubt, be difficult, without the assistance of a great number of drawings, to shew, in detail, the different parts of this machine, which is necessarily very complicated, though its action is infinitely simple: for when it is used, a detent is held by one hand, and the other, by means of a handle, brings the letter which is required under the pressing tool, which letter, as well as its place, is precisely indicated by a moveable piece, which passes along till it is properly disposed with regard to an index: the detent is then let go, and firmly retains a wheel which carries the letters of hardened steel: a small lever is then moved, of which the stop is nearer or further off, according to the width of the letter: the lever, while it strikes the stop, delivers the pressing-screw from a tooth which stopped its action; so that by these three operations, any letter whatever is spaced and struck with equal precision and facility.

Franklin was desirous of making a comparison between the time employed with this machine, to engrave a plate of 900 letters, and that required by a skilful compositor in performing the same work by moveable characters. He was very much surprized to see, that, without practice, the machine engraved the plate in nineteen minutes, without any fault, and his compositor employed twenty-three minutes to set up the same, which still required to be corrected*.

A report of the commissaries of the Academy of Sciences, of the 23d December, 1781, will

* Nine hundred characters are contained in about twelve lines of this journal. It seems probable, that the machine of the author was worked nearly at the same rate as the compositor placed his types in the stick, but that the latter being necessitated to take up his spaces, might lose time on this account. The speediest writer with a pen, who has come under my observation, would have written the same quantity, perfectly legible, in 48 minutes.—*N.*

serve to give a first notion of this machine. "The characters intended to be engraved are disposed in a circle, on a moveable wheel, successively brought under a pressing-screw, which engraves them upon a plate of pewter. The inventor has imagined different means, as well for speedily bringing the desired character beneath the screw, as to regulate the pressure in proportion to the extent of the letter itself, in order that all the different impressions may be made to the same depth. The plate itself has a double motion, one of which serves to space the letters and the words, and the other to regulate the distance between the lines. These motions are performed in such a manner, that any distance, at pleasure, may be allowed between the letters, the words, and the lines, and these distances possess the most perfect equality. The different widths of the letters present an obstacle here: in fact, it may easily be understood, that in order to render these spaces equal, it is necessary, not that the letters themselves should occupy equal portions of the line, but that a constant interval should be allowed between them. The beauty of printing, likewise, demands that these spaces should be, in some instances, varied in a small degree, in order that the justification should be perfect, that is to say, that the ends of the lines should present to the eye a regular and straight termination. The machine of our author affords the means of doing this.

"This machine appears to us to unite several advantages: 1. Engraved editions of books may be executed, by this means, superior to those which can be made by the hand of the engraver, however skilful; and these engraved originals will be made with much more speed, and much less expence. 2. As this machine is portable, and of no considerable bulk, it may become very useful in armies, fleets, and public-offices, for the impression of orders, instructions, &c. 3. It possesses the advantage which, in a variety of circumstances, is highly valuable, of being capable of being used by any man of intelligence and skill, without requiring the assistance of any professional workman. And, lastly, it affords the facility of waiting for the entire composition and engravings of a work before any of the copies are pulled off; the expence of plates, even for a work of considerable magnitude, being an object of little charge; and this liberty it affords to authors, may prove highly beneficial in works of which the chief merit consists in the order, method, and connection of ideas.

"So that even if it should prove, as experience only can shew, that it could not acquire the same speed as the common method of printing, nor an equal degree of facility for alterations and corrections, it would, nevertheless, be of very great utility.

"We think, therefore, that the idea of this machine being new, and the means adopted by the inventor, to give it the degree of perfection to which it is brought, being simple and ingenious, and its application being of great utility, the same is worthy of the approbation of the Academy; and that the description of the machine, executed with the machine itself, and presented by the author, is worthy of being published under the privilege of that Academy:

(Signed) "CONDORCET and BOSSUT.

"Given at the Louvre, 22nd December, 1781."

VOL. III.—MAY 1799.

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Description

Description of the Machine for Engraving.

This machine consists of two brass wheels, placed on the same axis above each other, and separated by a number of pillars, each two inches in length (Plate III). These two wheels, with the interval which separates them, are equivalent to a single wheel about three inches thick. In order, therefore, to simplify my description, I shall consider them as a single wheel which moves freely on its axis.

This wheel is perforated near its circumference with a number of square holes, which are the sheaths or sockets through which a like number of steel punches, of the same shape, are inserted, and are capable of moving up and down. They are very well fitted, and from this circumstance, as well as the thickness of the double wheel, they have no shake, or side motion, independent of the motion of the wheel itself. Every punch is urged upwards by a separate spring, in such a manner, that the wheel armed with its characters, or steel types (the lower faces of the punches being cut into the figures of the several letters), may turn freely on its axis; and if it be moved, the several punches will pass in succession beneath an upright screw, for pressure. The screw is fixed in a very firm and solid frame, attached to the supports of the machine; and by this arrangement, a copper-plate, disposed on the table, or bed of the apparatus, will receive the impression of all the punches in succession, as they may be brought beneath the vertical pressing-screw, and subjected to its action.

But as the press is fixed, it would necessarily follow that each successive impression would, in part, destroy, or mutilate, the previous impressions, unless the plate itself were moveable. It therefore becomes necessary that the plate should be moveable in two directions; the first to determine the interval between the letters and words, and form the lines; and the other motion, which is more simple, because its quantity may remain the same through the whole of a book, serves to give the interval between line and line, and to form the pages.

It will easily be conceived, that it would be a tedious operation to seek, upon the circumference of the wheel, each several character, as it might be required to come beneath the press, because it is necessary to repeat this operation as many times as there are characters in a work. I have considerably diminished the time and trouble of this operation, by fixing upon the axis of the great wheel, which carries the punches, another small wheel, about four inches in diameter, the teeth of which act upon a rack, which carries a rule moving between two sliders. This rule, or straight line, will, therefore, represent the developement, or unfolding of the circumference of the wheel which causes it to move, and will shew the position of the great wheel, which carries the punches. For these two wheels being concentric, the developement of the small toothed wheel, of about two inches radius, will exhibit, in a small space (for example, that of a foot), an accurate register of the relative positions of the punches, with regard to the pressing-screw. To obtain this effect, nothing more is necessary than to place a fixed index opposite the moveable rule, which last is divided in the following manner:

The punch on which the first letter of the alphabet is engraved, must be brought under the

centre

centre of the pressing-screw; and a line of division then drawn upon the moveable rule, to which the letter itself must be added to distinguish it. The index, already mentioned, being placed opposite, and upon this first division, will serve to place immediately beneath the pressing-screw, the punch, or rather the character, corresponding with the division upon the rule, without its being afterwards necessary to inspect the place either of the punch or the screw, with regard to each other. Consequently, as soon as the divisions which correspond with all the punches inserted in the wheel, are engraved upon the straight rule, the fixed index will immediately determine the position into which that wheel must be brought, in order to place the punches under the pressing-screw, in the order which the work may require.

This register (*tableau*), for by this name I shall hereafter distinguish the rule and its index, has no other function in the machine, than to guide the hand of the operator, and to shew when the punch is very near its proper position beneath the pressing-screw. When this is the case, the required position is accurately obtained by means of a detent, or catch.

The detent which I use for this operation, is a lever with two tails, one of which is urged toward the circumference of the wheel, by a spring. To this extremity of the lever is fixed a piece of hardened steel, which has the figure of a wedge, which, by means of a spring, is pressed towards the axis of the great wheel, but may be relieved, or drawn back, by pressure on the opposite tail of the lever, so as to permit the great wheel to revolve at liberty.

In the next place it must be explained how this detent takes hold of the wheel, so as to retain it precisely in the situation necessary to cause any one of the punches, at pleasure, to give its impression to the plate. For this purpose there are a number of notches cut in the circumference of the wheel, for the purpose of receiving the detent. These notches may be about half an inch deep, wider towards the circumference than elsewhere, and it will be of advantage that this outer width should be as great as the circumference of the wheel can conveniently allow. By this contrivance, the wedge will not fail to present itself opposite one of the notches into which it will fall, and draw the wheel exactly to its due situation, even though the index of the register should not be brought precisely to the line of division appropriated to any particular letter. For, if this last degree of precision were required in working the machine, it would be very prejudicial to the requisite speed which, above all things, is required in its use. When the wedge is, therefore, left at liberty, it not only enters immediately into its place, and moves the wheel till its two sides apply fairly to the interior surfaces of the notch, but retains the wheel in this state with the necessary degree of stability.

The method of giving the proper figure to these notches is very easy. For this purpose necessary, in the first place, to impress all the characters contained in the wheel, on a plate of copper or pewter. The support on which the plate is fixed must be moved in a right line, after each stroke of the punch, through such a space, that the characters may be arranged one after the other without touching. Now, as the perfect linear arrangement (supposing every other part to be true) must depend on the notches, it might seem sufficient to cut these according to the method used for the wheels of clock-work: but as it is very difficult to avoid some obliquity on the face of the punch, and, perhaps, in the hole through which it passes, it is in almost every

case necessary to retouch the notch itself. The requisite degree of precision may be easily obtained, when, upon examining with attention the print of the characters engraved upon the plate, the inequalities shall have been ascertained by a very fine line passing exactly under the base of two similar letters, assumed as objects of comparison: for the irregularity of linear position may, by this means, be determined, with great exactness, and remedied to the most extreme nicety. In this operation, the workman must file away part of that surface of the notch which is opposite to the direction of the motion the character requires. Great care must be taken to file only a small portion at a time, in order that the instant may be seized at which the wedge, by entering into the notch, brings the character to its due situation.

These details respecting the right-lined arrangement on the characters, must not divert our attention from the very great celerity with which any letter is brought to its place under the press, by means of the register and detent. This celerity is an object of so much importance in the engraving of a great work, that every means ought to be pursued which may tend to increase it. For this reason it is, that instead of following the alphabetic order in the arrangement of punches on the surface of the wheel, we ought to prefer that in which the sum of the different motions to be given to the wheel, for engraving an entire work, shall be the least possible. This tedious enquiry may well be dispensed with, by observing the order in which printers dispose their cases of characters, that the letters of the most frequent recurrence may be most immediately under the hand of the workman.

If all the characters afforded an equal resistance to impression in a plate of metal, a constant force would never fail to drive the punches to the same depth. But the faces of the letters are very unequal, and, consequently, it will be necessary to use a variable force. Most workmen use the hammer, and not a screw, as in this machine, for stamping. If the hammer had been used in this machine, it is evident that if we supposed it to have fallen from the same height upon every one of the punches, the force of the stroke could be rendered variable according to the nature of the characters, by placing a capital, or head, upon each, of an height properly adjusted to receive the hammer after passing through a greater or less space. But the heads of our punches are variable at pleasure, because they are screwed on; and thus it is that, by experimentally adjusting the heads of all the punches, a set of impressions are obtained, of equal depths from every one of them. When, for example, the letter *i* is placed under the hammer, the upper part of its head is at a small distance from the head of the hammer, in order that its fall, which begins always at the same place, may strike this letter weakly; but when the letter *M* is brought under the hammer, the upper part of its head being much less elevated than that of the letter *i*, will receive a much stronger blow. The impressions of the letters *M* and *i* will, therefore, always be equally deep, if the heads of the punches be once properly fixed by experiment.

Though I have already observed, that the pressure in this machine is given by means of a screw, I should certainly have used the hammer, if it were not for the inconvenience arising from

from the tremulous motion it gives, particularly to those parts which are hammer-hardened *. The pressure of a screw has not the same inconvenience. Its effect is gradually performed, without occasioning those sudden jars so inimical to precision and durability of a machine. It nevertheless happens, in some instances, that the impression on metals made by a screw, do sometimes partake of the circular motion of the screw; but this defect may be avoided, by giving its threads a great inclination. The screw I make use of has eight threads, which are so inclined that it runs through its female socket, and would fall out merely by its own weight. This construction affords the double advantage of preserving the impressions from the effects of the circular motion, and of affording a fall in the screw of nearly nine lines for each revolution. The head of this screw is solidly fixed in the centre of a brass wheel, of which the position is horizontal. The diameter of this wheel must be sufficiently large, that its motion may not be perceptibly affected by the irregularities of friction in the screw. This considerable diameter is also requisite, because the pressure of the screw depends not only upon the force which is applied, but the distance of the place of application from the centre of movement.

It is essential that this wheel should have very little shake; for which reason it is adviseable that the axis of the screw should be prolonged above the wheel itself, that it may slide in a socket firmly fixed to the frame of the machine. In this situation, the wheel, which is fixed on the prolongation of the screw, will have its plane constantly preserved in a situation parallel to itself, without any libration, notwithstanding the rise and fall of near nine lines, or three quarters of an inch, which it undergoes for each revolution on its axis.

It has been stated, as a requisite condition, that the screw should constantly fall from the same fixed point, or elevation, upon the heads of every one of the punches. To accomplish this essential purpose, a lever is firmly fixed to the support of the screw, which lever resembles the beam of a balance, having one of its extremities armed with a claw, and the other serving to give it motion through a small vertical space. The claw falls into a notch in the upper surface of the wheel attached to the screw, as soon as that wheel has risen to the desired elevation, and the lever itself is so far limited in its motion, that it cannot take hold of the wheel, excepting when it has reached that height. The wheel, therefore, remains confined and immovable, by means of this detent, and cannot descend until it is delivered by pressure upon the opposite tail of the lever. In my machine, the wheel which has the pressing screw for its axis, does not perform an entire revolution. It was with a view that there might never be any fall capable of shaking and disturbing the machine, that I determined to use only two-thirds of a revolution to strike these punches, which afford the strongest resistance. The screw consequently falls only through six lines, upon those heads which

* Which alters their figure, and loosens the fittings. The effect of agitation or stroke, in restoring hammer-hardened metals to their original state, is well known to workmen. Filing and turning of hammer-hardened steel, in particular, causes it to regain its figure. Thus, for example, when a circular saw is ground, and flattened by the hammer of the saw maker, it cannot afterwards be rendered thinner and truer in the lathe, because the point of the graver produces a tremulous motion which removes the effect of the hammer.—N.

are least elevated, and about two lines upon those which stand highest. Whence the difference between the extreme heights does not exceed four lines.

It is obvious, that so small a difference is not sufficient to strike all the characters from M to the letter i, when the wheel which governs the screw is put in motion by a constant weight, of which the impulse, like that of the hammer, is increased only by the acceleration of its fall. It is evident that this requisite variation of force might be had by changing the weight; but it is equally clear, that the numberless and incessant changes which the engraving of an entire work would demand, would be incompatible with that degree of speed which forms one of our first requisites. I was, therefore, obliged to render the force of the weight, which turns the screw, variable, by causing it to act upon levers of greater or less lengths, according to the different quantities of impulse required by the several punches. For this purpose, I adopted the following construction. I connected by a steel chain to the wheel, which moves the screw, another wheel, having its axis horizontal, so that the two wheels respectively command each other. They are of equal diameter, and the chain is no longer than to make an entire turn round each wheel. This second wheel or leading pulley, is intended to afford the requisite variations of force, which it does by means of a snail fixed upon its axis. The snail is acted upon by a cord passing over its spiral circumference, or groove, and bearing a weight which is only to be changed when a new set of punches for characters of a different size are put into the great wheel. The spiral is so formed, that when the weight descends only through a small space, the part of the cord, which is unwound, acts at a very short distance from the center of the pulley; but when the fall is greater, the part of the snail upon which it acts is so far enlarged as to afford a much longer lever, and, consequently, to give a proportionally greater effect to the stroke. This construction, therefore, by giving the advantage of a longer lever to a greater fall of the screw, affords all the power which the nature of the work and the different spaces of the letters demand.

The support on which the plate is fixed, must, as has before been remarked, move so as to form straight lines. This motion, which serves to space the different characters with precision, is obtained by means of a screw, the axis of which remains fixed, and carries a female screw or nut. The nut itself is attached to the support of the metallic plate, which receives the letters and carries it in the right lined direction without any deviation, because it is confined in a groove formed between two pieces of metal. The screw is moved by a lever which can turn it in one direction only, because it acts by a click upon a ratchet-wheel, which is fixed to the head of the screw. The action of this lever always begins from a fixed stop; but the space through which it moves is variable, according to the respective breadths of the letters. This new consideration induced me to fix upon the rule or plate of the register, a number of pins, corresponding with the different divisions which answer to each punch: these pins determine the distance to which the lever can move. It, therefore, becomes a condition that its position in the machine should be opposite the fixed index which determines the character at any time beneath the pressing-screw. The lever and its pin are, therefore,
the

the sole agents employed to space the characters. If the plate were not moved by the lever, the impressions would fall upon each other, and thus, for example, the letter *i* would be totally obliterated by the impression of the letter *l*.

Whenever, therefore, it is required to dispose the letters *i* and *l* beside each other, the plate must be moved after striking the letter *i* through a space equal to the quantity of the desired operation. Suppose this to be one fourth of a line, and that the lever should run through an arc of ten degrees to move the plate through this quantity: as soon as the pin of the letter *l* shall be adjusted to the necessary length to enable the lever to describe an arc of ten degrees, the operation of spacing the two letters *i* and *l* will be reduced to that of placing the last letter beneath the fixed index, and moving the plate till the lever shall be stopped by the pin belonging to the letter *l*. All the other letters will be equally spaced, if the disposition of the punches in the wheel be such, that the last stroke of any letter shall confound itself with any letter of a single stroke, supposing them to be impressed one after the other, without moving the lever between stroke and stroke. This arrangement deserves to be very seriously attended to, because the process could not be performed without it.

Many well informed persons are of opinion, that the perfect equality which this machine for engraving affords in the formation of letters and signs the most difficult to be imitated, may afford a means of remedying the dangers of forgery. It is certain that the performance exhibits a simple and striking character of precision, which is such, that the least experienced eyes might flatter themselves in certain cases to distinguish counterfeits from originals. My unfortunate colleague, Lavoisier, whom the friends of science and the arts will not cease to regret, made some experiments of this kind for the *caisse d'escompte* which were attended with perfect success. Artists appointed for that purpose endeavoured, in vain, to imitate a vignette formed by the successive and equal motion of a character of ornament*. My first machine for engraving was executed by Carrochez, with whom I had very much trouble, from the assiduity with which it was necessary to attend to his operations. I was more fortunate in the construction of the second; a skilful artist, Richer, executed this for a lover of the arts, with a degree of intelligence which was altogether surprising, and he even rendered it more perfect by the addition of a snail. The third machine of this kind, which was intended for the fabrication of assignats, is not finished. It is by the same artist.

While I was employed in the construction and improvement of my machine for engraving, Condorcet engaged me to make use of, in a short memoir of the celebrated Dupati, a method I had imagined to compose a large work with a very small number of types. I complied with his demand, and presented to the academy a polytype plate, entitled *Essai d'imprimerie, présenté à l'académie des sciences le 8 Février, 1786.*

The number of moveable cast types which I possessed, was contained in a box of the form of a book, and was sufficient to compose only four lines. When these four lines were

* This argument seems to apply only upon the supposition of the machine itself, being generally unknown to the public.—N.

composed, I took their impression in fine plaister, mixed with charcoal-dust* ; this mould enabled me to cast a number of copies of the four lines which I had composed. I soon perceived that the first and last letter of each line lost its position by sinking further back than the face of the other letters. It was easy for me to obviate this defect, by placing a small metallic support at a certain distance from the beginning and end of each line, to prevent the sinking of the letters. It is scarcely necessary to remark, that the mould ought to be very dry, and that a slight pressure upon the metal while in the half fluid state, is useful to obtain a good plate : seven or eight of these are required to form a page in octavo. I can take upon me to assert, that this process is neither tedious nor embarrassing, and it has the convenience of including the power of making corrections and additions.

This, in short, is the account I have to give of my enquiries into the typographic art. Reth, formerly director of the fabrication of assignats, is better acquainted than any other person with what has since been done for the improvement of this important art. I have but a few words to add. In my essay on ancient and modern coins, I described a method of imitating ancient and modern coins in bell-metal, by the polytypic art. For this purpose I place in a cavity in an anvil the piece of bronze heated to a proper degree, to render it soft : the use of the cavity is to enable the metal to sustain the action of that pressure which it is to receive from the screw or ram at the instant the medallion is applied to it, which is intended to give its hollow impression. If the metal be heated to a proper degree of softness, the original will not be in the least altered, whether it be of gold, silver, or copper ; but it is requisite that the piece should not be suffered to become hard by cooling, nor the medallion itself to remain in contact with it a sufficient time to alter its own temperature. The success of this process depends, therefore, on the quickness of the operation, and the practical knowledge of the degree of heat which the object of the impression can bear to soften it without injuring it in other respects : it is necessary to fasten the medal to the face of the screw or ram, in order that it may not touch the heated metal but at the moment of its fall.

Two silver medals struck under the magistrature of Titus Cavifius, Triumvir, Monetrarius, which exhibit the instruments used at Rome for coining, suggested to me the notion of the process I have been describing. There are two dies, one superior and the other inferior ; the lower die is supported on an anvil, beside which lie a hammer and pair of tongs. The form of the tongs leave no doubt respecting the use to which they were applied ; they serve to place the blanks or pieces of metal of the legal weight and fineness between the two dies, and these blanks being sufficiently heated, could, by a single stroke of the hammer, receive both impressions. By this design the ancient art of polytypage is displayed. The two Roman medals which bear for their design, the Dies, the Pincers, the Anvil, and the Hammer, were struck at Rome three hundred years before the Christian era, at the epocha of the war with the Tarentines. Andrew Morell, in his work upon the medals of the Roman Families, expresses himself as follows :

* The material used by the celebrated Tassie, in a similar process of extreme delicacy, was a mixture of sulphate of lime (plaister of Paris) and tripoli. See *Philos. Journal*, II. 63.—N.

Caput Junonis monetæ salutaris : instrumenta monetalia, incus, malleus, forceps, cum vulcani pileo in laurea.

In bello, videlicet, contra Pyrrhum et Tarentinos, quum pecunia destituerent Romanis, eos scribunt Junoni ad comparandam eandem, vota fecisse, deam vero monuisse illos ut Justitiæ armis uterentur; sic enim pecuniam iis non defecturam. Quod monitum quum salutare Romanis fuerit cognomina Monetæ Salutaris inde Juno adopta est. Laurea ad victoriam illam olim deæ monitu relata respicere potest.

This Memorandum is translated from the J. de Phys. IV. N. S. p. 363.

VI.

On the Processes for Manufacturing Hats, the Use of Machines, and other Objects. By N. L.

TO MR. NICHOLSON.

Newcastle, 15th April, 1799.

SIR,

IN addition to the information contained in my former letters on the subject of hat-making by engines, I would add the house of Messrs. Wells and Chatterton of Brenchly, in Kent. From these instances, it would appear that the question regarding the possibility of making hats by engines, is clearly decided in the affirmative. The enquiry now apparently is, how far can those already in use be simplified; their principles accurately displayed to the public; or their numbers encreased?—For I believe the directions given already are all that are erected.

The knowledge of hat-making necessarily includes the practice of bowing, basoning, felting, pulling (these two last are included in the term working; that is, at the plank); so that to a hat-maker the remainder of the processes of felting he would have to investigate (whether of animal or vegetable filaments, amongst which might be included even the combing of wool, the dressing of flax, and the fabrication of soldiers' belts), would be trifling. For a person to have complete success in the formation of machines, such as those in question, the fabricator should have a comprehensive and determined mind, to bear with, and to overcome the impediments which may be thrown in the way by the journeymen; and an enlarged view of chemical, and especially of mechanical, operations, a sufficient portion of which, few indeed of the masters of the present day possess.

Your account of hat-making in your last number, suggested by a visit to the manufactory of my esteemed friend Mr. Collinson, will convince your readers how far from the truth any former accounts are as given to the public in the Encyclopedia, and also in the Universal Magazine for the year 1750, which are all the places, or nearly so, in which it has appeared. They will not wonder at the few improvements regarding the introduction of machines into a business which has, till very lately, been hidden by the *laboured obscurity* of prejudiced and self-interested manufacturers. Few, indeed, yet think, *that the best way of improving a manufacture is to give it publicity.* The progress of our cotton and woollen ma-

manufactories to their present state, has not probably been impeded by a knowledge of the respective businesses being developed. It is not improbable but that the intervention of machines has sufficiently repaid to the interested manufacturer, any loss attending that which he might suppose to be an impolitic disclosure. We have found, in the course of our enquiry, that machines are invented and at work; from their small number, and the strong prejudice existing against them, even in the minds of the *majority of the masters*, the undertaking must be acknowledged to be only beginning to expand. Is it *impossible* to accomplish it? A few years ago the masters in Lancashire generally smiled at the attempt, and the house at Lea Wood, in Cromford, was ridiculed by the majority, and pitied by a few. But shall ingenuity, Sir, fabricate engines for other businesses, and must the mere attempt at machines for this appear as an absurdity? Surely the condemnation of such attempts is, if not futile, at least premature; indeed it appears to me a step gained, and that no inconsiderable one, to be able *publicly* to produce the address of houses who use engines. The contemplation of such facts must overthrow the opinion of the impossibility of their erection, that many masters wish to spread abroad.

The queries you suggest with regard to the dregs, are answered by my men in the following manner: The operation of planking depending, in a great degree, upon the acidity of the liquor, they prefer old dregs to new, because the older they are the sourer they become.—The same reason, namely, its acidity, is given by the felt-makers for preferring old urine, as mentioned in a former letter, in the boiling of felt-hats; as to those no * vitriol is used in the working. The dregs ought to be thick, or the top (the thin) poured off. If thin or new they spoil the body of the hats by hindering their working. I may mention here, that in consequence of the stuff-hat-makers using vitriol, they are obliged to have a leaden kettle, whilst the felt or woolmakers have only a cast-metal one; though wool hats feel much softer when worked in a lead kettle than a cast-metal one, if worked in each with clean water, (the reasons for this difference would oblige the author of this). The kettle or bath heated to a less degree than that you mention in your memoir, has no power, even though with an additional quantity of lees or vitriol; the hats are generally observed to work (shrink in) as the kettle “comes too,” that is, heats.

Before the operation of planking is begun, the hat is dipped into the boiling kettle, and allowed to lie upon the plank until cold again; this is called soaking, that is, being perfectly saturated with the hot liquor; if they are put in too hastily in this state, for they are then only bowed and basoned, they would burst from the edges, each hat not being sufficiently felted into the other. Vitriol alone would harden the hat too much; the dregs keep it mellow and thicken the body, for the vitriol alone eats or purges the stuff too much; the hats feed as it were upon the dregs †. The journeymen tell me that the dregs are to hold

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* The term *vitriol* appears to designate the sulphuric acid (*oil of vitriol*) in manufactories; and not the sulphate of iron or copper.—N.

† There is a degree of obscurity in the description, as well as the rationale of what happens here. Might it be conjectured, that the sulphuric acid by dissolving or disengaging a natural mucilage from the face of the hair,

or fill the body, whilst a little vitriol cleanses it of the dirt, &c. that may be on the rabbit or other wools; too much vitriol would make the whole that was weighed out to the journeymen work into the hats, but by the mutual action of the vitriol and the dregs, the quantity of the first being small, about a small wine glass full, the dirt and the strong hairs* get purged out, whilst at the same time the dregs keep the hat plump. After the body is got up to a certain size, the workmen put on gloves made of the sole of a shoe, to shield their hands from the vitriol, to enable them to work the hat tighter, and to bear hotter water. The kettle is usually weakened before blocking the hats, lest the vitriol should eat out the dregs. There is only one cloth used at the hurdle, which with paper is generally thought sufficient, and the cloth from its being unbleached and soiled in the wear, might naturally enough convey the idea of being dyed brown. The bow is best made of ash, that it may be heavy and steady in working: it is composed of the stang † or handle made of the above wood: the bridge at the smaller end, or that nearest the window, when held in the hand in the act of bowing, is called *the cock*, and that at the other end, which from its being hung to balance, and being thicker, is the nearer to the workman's hand, is called the breech. After bowing, and previous to the basoning, a *hardening skin*, that is, a large piece of skin, about four feet long and three feet broad, of leather alumed or half tanned, is pressed upon the bat, to bring it by an easier gradation to a compact appearance, after which it is basoned, being still kept upon the hurdle. This operation, the basoning, derives its name from the process or *mode of working*, being the same as that practised upon a wool hat after bowing, the last being done upon a piece of cast metal, four feet across, of a circular shape, called a *bason*: the joining of each bat is made good here by shuffling the hand, that is, by rubbing the edges of each bat folded over the other to excite the progressive motion of each of the filaments in felting, and to join the two together. Many journeymen, to hurry this work, use a quantity of vitriol, and then, to make the nap rise and flow, they kill the vitriol, and open the body again by throwing in a handful or two of oatmeal; by this means, they get a great many made, though, at the same time, they leave them quite grainy ‡ from the want of labour. This, in handling the dry grey hat when made, may be in part discovered, but in part only.

Another advantage attending the use of dregs, whether of beer, porter, or wine, is, that as the boiling in the dying does not draw out much of the mucilage from each hat, when they

hair, may render the action of felting too speedy, close, and irregular, so as not to allow time either for purging or repairing; and that the mucilage of the dregs prevents this, and has the advantage of being afterwards washed out to any required degree towards the end of the process?—N.

* Because they are straiter:—Philos. Journal, I. 401. note.—N.

† An old north-country word, signifying a pole or staff.—N.

‡ The cause, as I presume, of hats wearing into shining or greasy spots. This was a principal defect of the cloth made under Booth's patent, in which the staple of the raw material, namely, cotton, flax, or wool, was wrought together, by an engine, with little or no dependance on the felting property of the fibre itself.—N.

come to be stiffened, the dregs form a body within the hat, sufficiently strong or retentive to keep the glue from coming through amongst the nap: vitriol alone purges or weakens the goods too much, consequently half of the quantity does better with the addition of dregs, and they also allow the body to be closer from its getting mere work.

Let us now examine the opportunities that journeymen have to steal from their masters; and probably this review will suggest still stronger arguments, for the more general introduction of machines, than has been advanced in a former number. For we shall find that the goodness of the bodies, as well as the quantity of beaver weighed out for the napping or covering, which you call the facing, may both be greatly impoverished, and the master, from the dishonesty of the men, nearly ruined before he is aware: from the master who weighs to the maker, and from him after he has got his hat ready for the dressing, or raising the nap, the beaver may be easily stolen, and yet remain unknown to the master, by the hats coming in damped, though imperceptible to the hand, and of course weighing heavier. Or it may be said, by the journeymen, that the hats were wider laid in the basoning, and had purged more at plank, and of course lost weight. Nay, the very dregs in which it is seen the hats are worked, may increase the weight, and even beating them when dry, does not free them *entirely* from the dried mucilage. The colour of the covering, or beaver, is no infallible guide, as the journeyman who cuts it, often puts inferior beaver into the middle of a hood, lock, or ball of beaver, and secretes a part of the best, which they, as well as the journeymen hat-makers, sell to the inferior masters at a low price; detection in either case being almost impossible. The journeyman hatter knows this, and can say the beaver was light coloured; though that article might be very good, and the covering appear light from the hare wool or inferior beaver put in by the journeyman hat-maker, after having stolen an equal weight of good beaver (the value of best hare's wool being about 16s. per lb. and beaver from 30s. up to 60s. per lb.), especially if backs of hare's wool be weighed into the body. It is from these depredations that the use of machines appears so urgent: the same inconveniencies attend the master furrier or skinner; he does not examine each lock or ball of beaver minutely, for his time will not allow him, and any mixture of inferior beaver (which is very easily obtained and effected), worth, as above, about 30s. with the best at about 60s. and a part of that best stolen, will within a very little time, arrive to a large amount. These, and many such, will demand, I had almost said, imperiously, the attention of the ingenious mechanic, whilst the certain advantage from the above statement, will not be lost sight of by the fabricator, in examining the arguments for and against the introduction of machines into each business. To the moralist, who views the duplicity, the crimes and their parent ignorance of his fellows, the recollection of the depravity so deeply impressed upon each circle of journeymen—from the ease with which these thefts may be conducted, from the great wages they receive, many in London more than 30s. per week, and from their confirmed habits of debauchery, consequent upon both these—motives sufficiently strong will present themselves, to accelerate an enquiry, *at least*, into the impediments, or the advantages, attending the introduction of engines, if they were only for the bowing, hardening, and basoning, for then the journeymen would be totally

hindered

hindered from stealing; or if those few already in use were more general, and probably more simplified, the purpose might be answered. Let it then be the practice of the well-wishers to virtue, to diminish, or remove as far as possible, the impediments, whether arising from moral or other causes, and vice will present to them too striking a front, not to induce them to hold out the advantages in an unornamented, though decisive manner. With regard to flax, the object of the journeyman in that business, and in that of the wool-combers, being the same, that is, to lay each filament in a straight line in certain proportions, an engine which would accomplish the one, must with a trifling alteration effect the other. I find in the General Evening Post for the 14th March, 1799, that there is a manufactory establishing in the neighbourhood of Prescot, in Lancashire, for the purpose of *combing wool and spinning flax*—How far might a hint be taken from this to accomplish the former?

Your's &c.

N. L.

VII.

*Account of a Substance found in a Clay-pit, and of the Effect of the Mere of Difs upon various Substances immersed in it. By Mr. BENJAMIN WISEMAN, of Difs in Norfolk. With an Analysis of the Water of the said Mere. By CHARLES HATCHETT, Esq. F.R.S. In a Letter to the Right Honourable Sir JOSEPH BANKS, Bart. K. B. P.R.S. &c.**

THE substance I have inclosed was found near Difs, in a body of clay, from five to eight feet below the surface of the soil. All the pieces I observed laid nearly in a horizontal direction; and varied in size, from two or three ounces, to as many pounds. The colour of the substance, when taken fresh from the clay-pit, was like that of chocolate; it cuts easily, and has the striated appearance of rotten wood. The pieces were of no particular form; in general, they were broad and flat, but I do not recollect to have met with a piece that was more than two inches in thickness: it breaks into laminæ, between which are the remains of various kinds of shells. The specific gravity of this substance, dried in the shade, is 1.588; it burns freely, giving out a great quantity of smoke, with a strong sulphureous smell.

By a chemical analysis, which I cannot consider as very accurate, one hundred grains appear to contain,

	grains.
Of inflammable matter, including the small quantity of water contained in the substance	41.3
Of mild calcareous earth	20.0
Of iron	2.0
Of earth that appears to be silex	36.7
	<hr/> 100

* Philof. Transf. 1798. page 567.

On the Effect of the Mere of Difs upon various Substances.

Observing several years ago, that flint stones taken out of the Mere of Difs were incrustated with a metallic stain, I was induced to make some experiments, in order to discover the nature or composition of this metallic substance. Nitrous acid readily removes it, dissolving a part, and leaving a yellowish powder, which, washed and filtered, was found to be sulphur. Vegetable fixed alkali precipitated from the nitrous acid a ferruginous coloured powder, which was iron.

With a view to determine what length of time was necessary for the formation of this metallic stain upon flint stones, or other substances, I inclosed in a brass-wire net the following articles: flint stones, calcareous spar, common writing slate, a piece of common white stone ware, and likewise a piece of black Wedgwood-pottery. After remaining in the water from the summer of 1792, to August, 1795, the flints and Wedgwood-ware had acquired the metallic stain in a slight degree, and the slate had assumed a rust colour; the other substances appeared not to be at all altered. I was greatly surprised to find the copper wire that held the net, surrounded with a metallic coating of a considerable thickness; it was of a deep lead colour, and of a granulated texture. When taken from the wire, and ground in a mortar, it had a black appearance, interspersed with very hard shining particles. The wire was evidently eroded, and this substance deposited in the place of the copper that was decomposed, somewhat similar to the decomposition of iron in cupreous waters. By repeated chemical analysis of this substance, one hundred grains contain, of copper, 70.3 of sulphur, 16.6; of iron, 13.3 grains.

I have never met with an account of the decomposition of copper, in waters impregnated with iron, in any chemical work; and, as iron appears to have a greater affinity to the vitriolic acid than copper has (as is constantly evinced in the neighbourhood of copper mines), it appears an anomaly in chemistry, that I am not adept enough in the science to account for.

[The President and Council of the Royal Society thinking the effects of the water of Difs Mere deserving of further inquiry, desired Mr. Wiseman would send some of the said water, for the purpose of examination. Mr. Wiseman accordingly sent a quantity of the water, accompanied by the other substances described in the following letter to the President.]

S I R,

Difs, May 29, 1798.

As the Society have expressed a wish, through Mr. Frere, to have some of the water in which the copper wire was deposited, which Mr. Frere, at my request, laid before the Society, I have sent two gallons of the water of Difs Mere (No. 1), with a small quantity of copper cuttings (No. 2), which laid in the same water, a few feet from the side, and six feet in depth, from the 7th of February, 1797, to the 20th of the present month, May, 1798. The pieces of copper, when laid in, weighed 3051 grains; when they were taken out, and washed from the mud that lightly adhered to them, preserving and weighing the scaly matter that came off, they weighed 2944 grains, indicating a loss of 107 grains. Examining the pieces
of

of copper, the same evening they were taken out of the water, I observed a number of small crystals formed upon some of them, in the form of pyramids joined at their bases; these crystals lost their shining appearance, by the evaporation of the water of crystallization, in the warmth of the succeeding day. Whether they will be preserved in a journey of nearly 100 miles, is perhaps doubtful. No. 3. contains two pieces of copper, on which the crystals were most abundant. No. 4. contains a small quantity of the substance formed upon the copper, that came off in washing and in weighing it.

The town of Dids is principally situated on the NNE. and E. sides of this piece of water. The land runs pretty steep on the W. and N. of it, to the height of 40 to 50 feet: on the SE. the ground comes within a few feet of the level of it. The soil of the upper part of the town is a stiff blue clay; that of the lower part, to the SE. a black sand, beneath which it is a moor. The water in the higher parts of the town is good; in the lower parts, it is a chalybeate, of which a specimen is sent (No. 5.)

No. 6. contains a quantity of flint stones, taken from the SE. side of the Mere, where the water is shallow; many of which are strongly marked with the metallic stain, which they acquire by lying in this water a few years.

The Mere contains about eight acres, and is of various depths, to twenty-four feet: from its situation with respect to the town, it may naturally be supposed to contain a vast quantity of mud, as it has received the silt of the streets for ages. In summer the water turns green; and the vegetable matter that swims on its surface, when exposed to the rays of the sun, affords vast quantities of oxygen gas. I cannot help considering this process as having a considerable agency in the corrosion, and in the formation of the metallic crust upon the copper deposited in this water. Some of this vegetable matter will be found in the water sent to the Society.

I intend to make some further experiments with different metallic substances, at different parts, and at various depths; but, as the process is slow, if in the mean time you, Sir, or any of the members of the Society, will have the goodness to point out any experiment you or they may wish to have made, I shall be very glad to contribute all in my power towards the illustration of the subject.

I have the honour to be, &c.

BENJ. WISEMAN.

The Right Hon. Sir JOSEPH BANKS, Bart.

K.B. P.R.S.

[The water, and other substances described in the foregoing letter, were delivered to Mr. Hatchett, who had been previously requested, by the President and Council, to examine them. The result of his examination is related in the following letter to the President.]

Analysis of the Water of the Mere of Difs, by CHARLES HATCHETT, Esq.

DEAR SIR,

HammerSmith, September 14th, 1798.

In consequence of the request which you and the Council of the Royal Society have done me the honour to make, that I would examine the water of Difs Mere, and the other substances sent by Mr. Wiseman, I now hasten to acquaint you with the result of my experiments.

The substances sent by Mr. Wiseman are as follows: Some copper-wire, with a blackish-grey incrustation. Water from Difs Mere (marked No. 1.) Copper-cuttings, covered with a blackish crust, similar to that on the copper-wire (marked No. 2.) Some cuttings similar to those above mentioned (marked No. 3.) The paper, No. 4, contained some of the black crust, detached from the cuttings. No. 5, a quart bottle, containing some water from the lower part of the town of Difs, and called, by Mr. Wiseman, a chalybeate water. No. 6, some flints, taken from the SE. side of the Mere, where the water is shallow, and having, as Mr. Wiseman terms it, a metallic stain.

My first experiments were made on the incrustation of the copper-wire, mentioned in Mr. Wiseman's first letter. This incrustation was easily detached from the wire, and being reduced to powder, was digested with nitro-muriatic acid, in a gentle heat: a green solution was formed, and there remained a residuum, of a pale yellow, which proved to be sulphur.

The solution being diluted with two parts of distilled water, was supersaturated with pure ammoniac, by which a few brown flocculi of iron were precipitated. The supernatant liquor was blue; and, being evaporated, and re-dissolved by sulphuric acid, the whole was precipitated by a plate of polished iron, in the state of metallic copper. The component parts of this coating were, therefore, copper, and a very small portion of iron combined with sulphur. I could not extend these experiments, as the whole quantity of the coating that I was able to collect, amounted only to three grains and a half*.

The next experiments were made on the black crust of Nos. 2, 3, and 4. This I found to be exactly the same as that formed on the copper-wire, viz, it consisted of copper combined with sulphur, and a very small portion of iron.

* The copper-wire, when the coating was removed, was perfectly flexible, and the surface did not appear unequal or corroded: this is commonly the case under such circumstances; for, when sulphur has combined superficially with a metal, the compound is observed to separate easily, so as to leave the metal underneath not injured in quality, and very little, if at all, affected in appearance. Those who diminish silver coin, make use of the following method: They expose the coin to the fumes of burning sulphur, by which a black crust of sulphurated silver is soon formed, which, by a slight but quick blow, comes off like a scale, leaving the coin so little affected, that the operation may sometimes be repeated twice or thrice, without much hazard of detection, if the coin has a bold impression.

I next examined the water of Dis Mere (No. 1.), and I was at length led on, step by step, to make a regular analysis of the fixed ingredients. Before I made the analysis, I examined this water with certain re-agents, and remarked the following properties. 1. The water of Dis Mere has a yellowish tinge, and the flavour is rather saline; but it has not any perceptible odour. 2. Prussiate of potash did not produce any effect. 3. Acetite of lead produced a slight white precipitate. 4. Nitrate of silver formed one very copious. 5. Tincture of galls had not any effect. 6. Muriate of barytes caused a slight precipitate. 7. Ammoniac, potash, and oxalic acid, severally produced precipitates, when added to different portions of this water.

Analysis.

A. Three hundred cubic inches of the water, by a gentle evaporation, left a pale brown scaly substance, which weighed 58 grains. B. These 58 grains were digested in alcohol, without heat, during 24 hours, and afforded a solution, which, by evaporation, yielded muriate of lime, slightly tinged by marshy extract, 18 grains. C. Six ounces of distilled water were then poured on the residuum, and, with repeated stirring, remained during 24 hours. By evaporation, this afforded muriate of soda, with a very small portion of sulphate of soda; in all, 10 grains. D. What remained was boiled in 800 parts of distilled water, and the solution being evaporated, left of selenite 1.70 grains. E. The undissolved portion now weighed 25 grains, and was digested with diluted muriatic acid: a great part was dissolved, with much effervescence, and, being filtrated, afforded, by ammoniac, of alumine 1.50 gr. From this I afterwards separated a very minute quantity of iron, by means of prussiate of potash. F. Carbonate of soda was then added to the liquor, and precipitated carbonate of lime 20 grains. G. The insoluble residuum weighed 3.50 gr.; and proved to be principally carbon (produced by decomposed vegetable matter), with a very small quantity of siliceous earth. The result of this analysis was, therefore,

B. Muriate of lime	-	-	-	-	-	18
C. Muriate of soda, with a very small portion of sulphate of soda.						10
D. Selenite	-	-	-	-	-	1 70
E. Alumine, with a portion of iron too small to be estimated	-					1 50
F. Carbonate of lime	-	-	-	-	-	20
G. Carbon, with a little siliceous earth.	-	-	-	-	-	3 50
						<hr/> 54 70
					Loss	3 30
						<hr/> 58 0

It is worthy of notice, that the iron present was in so very small a quantity as not to be detected by any test, till it had been separated in conjunction with the alumine.

The water, No. 5, from Mr. Wiseman's account, does not appear to have been concerned

in producing the effects which he has observed, and the quantity was too small to be subjected to a regular analysis; I noted, however, what follows :

1. It has a very strong hepatic flavour and smell.
2. A plate of polished silver, put into it, became black in a few hours.
3. It became faintly bluish with prussiate of potash, after standing five or six hours.
4. Tincture of galls produced a faint purple cloud.
5. Solution of acetite of lead afforded a brown precipitate.
6. Nitrate of silver produced the same.
7. Potash, and ammoniac caused a precipitate; but that of the former was the most copious.
8. Oxalic acid produced a precipitate.
9. Muriate of barytes had also a slight effect.

The water, No. 5, cannot, therefore, be considered as a chalybeate (the quantity of iron contained in it being scarcely perceptible); but it appears to be a water containing some hepatic gas, together with substances similar to those contained in No. 1. From the above experiments it is evident, that the water, No. 1, does not contain any of the component parts of the crust formed on the copper-wire and cuttings, although it is certain that the incrustation took place during the immersion of those bodies; but, before I mention my ideas on this subject, I shall give an account of some experiments made on the flints, No. 6. These were coated with yellowish shining substance, which appeared to me to be pyrites; and, as the flints could not have contributed any metallic substance to form this coating, I was enabled by their means to ascertain, whether the copper of the crust, formed on the wire and cuttings, had been furnished by the pieces of copper, or by any thing in the vicinity of the water.

1. I poured nitro-muriatic acid on some of the flints, in a matrafs, so as completely to cover them. The coating was rapidly dissolved, with much effervescence; and, when the flints appeared perfectly uncoated, and in their usual state, I decanted the liquor.

2. A yellow matter subsided, which proved to be sulphur.

3. Prussiate of potash produced Prussian blue; and the remaining part of the solution, being superaturated with ammoniac, afforded an ochraceous precipitate of iron. The supernatant liquor did not become blue, as when copper is present, nor was the smallest trace of it afforded by evaporation.

Martial pyrites is, therefore, the only substance deposited on bodies immersed in the water of Difs Mere; and the copper of the crust, formed on the wire and cuttings, was furnished by those bodies.

It is proved by the analysis, that the water of Difs Mere does not hold, in solution, any sulphur, and scarcely any iron; it has not, therefore, been concerned in forming the pyrites; but it appears to me, that the pyritical matter is formed in the mud and filth of the Mere; for Mr. Wiseman says in his letter, that "the Mere has received the filth of the street for ages." Now it is a well-known fact, that sulphur is continually formed, or rather liberated, from putrefying animal and vegetable matter, in common-sewers, public ditches, houses of office, &c. &c.; and this most probably has been the case at Difs. Moreover, if sulphur thus formed should meet with silver, copper, or iron, it will combine with them, unless the latter should be previously oxydated. The sulphur has, therefore, in the present case, met with iron, in, or approaching, the metallic state, and has formed pyrites; which (whilst in a minutely

a minutely divided state, or progressively during formation) has been deposited on bodies, such as the flints when in contact with the mud. But an excess of sulphur appears to be present; for, when copper is put into the Mere, the sulphur readily combines with it; and, at the same time, a small portion of iron appears to unite with the compound of copper and sulphur, possibly by the mere mechanical act of precipitation.

The incrustation on the copper wire and cuttings is, in every property, similar to that rare species of copper ore, called by the Germans *kupfer schwärze* (*cuprum ochraceum nigrum*); and I consider it as absolutely the same. In respect to the martial pyrites on the flints, there can be no hesitation; and, as in these two instances, there were evident proofs of the recent formation of ores in the humid way, I was desirous to ascertain the effect on silver. I therefore wrote to Mr. Wiseman, to request that he would take the trouble to make the experiment; and received from him the following answer, accompanied by the specimens.

“ SIR,

“ Difs, 8th of September, 1798.

“ Immediately upon the receipt of your letter (27th July), I laid some silver plate, and silver wire, into the Mere; the whole weighed 235.6 gr. I took it out on Thursday last (September 6th), and, after cleaning it carefully from mud and weeds, I find it weighs 242.8 gr.; an increase of 7.2 gr. The silver plate you will find much tarnished, in some parts almost black; the wire is in many places fairly incrustated, which crust, upon the pressure of the fingers, comes off in thin scales. The whole appearance of the silver strongly indicates the presence of sulphur, which I have no doubt abounds in every part of the Mere. The peculiar smell of the mud gives me reason to suppose, that a great deal of hepatic air is produced; which, probably, uniting with the iron held in solution in the water of the Mere, may account for the martial pyrites found on the flints. By what affinity the copper wire, laid in this water, is attacked, I am not chemist enough to determine.

“ I have begun a set of experiments, with the view of producing the same effects upon copper wire by artificial means; but whether I shall succeed, I am not at able present to say.

“ I am, &c.

“ BENJ. WISEMAN.”

P. S. By experiments I have lately made, I find hepatic gas precipitates carbonate of iron in the form of a black flocculent matter; 71 parts of which are iron, and 29 sulphur.

The silver plate I found (as Mr. Wiseman has mentioned) much tarnished, and in many places almost black, but I could not detach any part of it. I succeeded better with the wire, and collected a small portion of a black scaly substance, which, as far as the smallness of the quantity would allow it to be ascertained, was sulphuret of silver; and was similar, in every respect, to the sulphurated or vitreous ore of silver, called by the Germans *glasertz*. This effect on the silver was to be expected; and I recollect to have read, not many months ago, in one of the foreign journals, that Mr. Proust had examined an incrustation, of a

dark grey colour, formed in the course of a very long time, on some silver images, in a church at (I believe) Seville. This incrustation he found to be a compound of silver with sulphur, or, in other words, vitreous silver ore. The same principle is the cause of the tarnish which silver plate contracts with so much ease, particularly in great cities; for this tarnish is principally a commencement of mineralization on the surface, produced by the sulphureous and hepatic vapours dispersed throughout the atmosphere, in such places.

To Mr. Wiseman's observations we are much indebted, as they make known the recent and daily formation of martial pyrites, and other ores, under certain circumstances. It is not to be supposed that such effects are local, or peculiar to Difs Mere; on the contrary, there is reason to believe that similar effects, on a larger scale, have been, and are now, daily produced in many places. The pyrites in coal mines have, probably, in great measure thus originated. The pyritical wood also may thus have been produced; and, by the subsequent loss of sulphur, and oxydation of the iron, this pyritical wood appears to have formed the wood-like iron ore which is found in many parts, and particularly in the mines on the river Jenisei, in Siberia. In short, when the extensive influence of pyrites in the mineral kingdom, caused by the numerous modifications of it, in the way of composition and decomposition, is considered, every thing which reflects light on its formation becomes interesting; and I cannot but regard as such, the effects which Mr. Wiseman has observed in the Mere of Difs.

With great respect, I remain, &c.

CHARLES HATCHETT.

The Right Hon. Sir JOSEPH BANKS, Bart.

K.B. P.R.S. &c.

VIII.

*Abstract of Experiments and Observations on the internal Use of Phosphorus. By ALPHON-
SUS LEROI, Professor at the School of Medicine in Paris*.*

1. **T**HE internal administration of phosphorus in such diseases as exhaust the vital powers, appears to give a certain degree of activity to the energy of life, and afford spirits to the patient without raising the pulse in the same proportion. The author relates several cases in his own practice. Among others is the following. He was called to a woman at the point of death, who was exhausted with weakness after three years' sickness. He yielded to the pressing intreaties of her husband, who earnestly desired that he would prescribe something. He composed a draught consisting of syrup diluted with water, which had remained upon the sticks of phosphorus. She was much better the following day, and continued to recover for several days afterwards. She died about sixteen days subsequent to this prescription.

* From the bulletin of the Société Philomathique, copied into the Journal de Physique, IV. N. S. 402.

2. He himself had, as he expresses it, the imprudence to take two or three grains of solid phosphorus, simply united with theriaca. The consequences were dreadfully alarming. His first sensation was that of a burning heat in the region of the stomach, which organ appeared to him to be filled with gas, and even emitted elastic fluid by the mouth. In this shocking state of torment, he endeavoured but in vain, to cause himself to vomit, and found no relief but by drinking cold water from time to time. His pains, were, at length assuaged; but the following day an astonishing muscular force was developed through the whole habit, with an almost irresistible disposition to exert that force. The effect of the medicine at length terminated after a violent priapism.

3. In many circumstances the author has employed, or continues to prescribe, phosphorus internally, with the greatest success, to restore and establish the forces of young persons exhausted by too frequent sensual indulgence. He describes the process by which he divides the phosphorus into very small particles. He agitates the phosphorus in a bottle filled with boiling water, by which means it becomes divided into globules; and by afterwards continuing to agitate the bottle beneath cold water, he obtains a kind of precipitate or very fine powder of phosphorus, which he levigates gently with a small quantity of oil and sugar, and dilutes the whole in the yolk of an egg, to be used as a lochoch. By the help of this medicine he has performed cures, remarkable for the speedy restoration of strength obtained by his patients.

4. In malignant fevers, the internal use of phosphorus, to stop the progress of gangrene, succeeded beyond all hope. The author relates several instances.

5. Pelletier related to him, that having neglected a portion of phosphorus in a copper basin, the metal was oxyded, and remained suspended in the water; that having, by accident, thrown this water into a small court where ducks were kept, those birds drank of it, and every one died; but the male covered the females to the last instant of his life: an observation which agrees with the fact of the priapism which the author experienced.

6. The author relates a fact, which shews the astonishing divisibility of phosphorus. Having used, in the treatment of a patient, certain pills, into the composition of which a quantity of phosphorus, amounting at most to one-fourth part of a grain, entered; he found upon the occasion of opening the body, that all the internal parts were luminous, and even the hands of the operator, though washed and well dried, preserved the phosphoric light for a considerable time.

7. The phosphoric acid used as a lemonade, proved very advantageous in the cure of a great number of disorders.

8. Leroi affirms, that having oxyded iron with phosphorus, he obtained a white oxyd, scarcely reducible by the ordinary methods, which he thinks may be advantageously substituted instead of white lead, in the arts, particularly in oil and enamel paintings. This white oxyd of iron produced a very strong nausea in the author, who ventured to place a particle upon his tongue. He does not hesitate to consider it as a dreadful poison. He could not reduce it but by fixed alkali and phosphoric glass.

9. The

9. The author affirms, that by the assistance of phosphorus he has decomposed and separated the sulphuric, muriatic, and nitrous acids, from their bases; and that he has transmuted the earths by means of phosphoric acid, so as to obtain considerable quantities of magnesia from calcareous earth. And he declares, that by his operations upon phosphorus he has obtained processes, by means of which he has effected the frit of rubies*, the fusion of emeralds, and vitrification of mercury.

IX.

Letter of Enquiry respecting the proper Form of a Boiler for Steam Engines.

To MR. NICHOLSON.

SIR,

17th April, 1799.

I AM about to erect a large steam-engine, but am in doubt of the proper construction of boilers. Those which I use at present, appear to me very improper for raising the greatest quantity of steam with the least fuel. The form, however, has been in repute for 10 or 15 years past. My boilers are 8 or 9 feet in height, 4 to 5 in breadth, and 9 feet in length. Through the middle lengthways is a flue of iron plates, as is the rest of the boiler, and the water must always cover this flue, consequently stand 5 to 6 feet deep in the boiler. This flue, with the flues on the outside of the boiler running parallel to it, is said to accelerate greatly the production of steam, and to have the same effect as a great fire-place below a broad and shallow boiler. But I have often observed, that a flame passing through any flue in which there are no obstructions, to make the flame reverberate, communicates very little heat to the side in proportion to the fuel wasted. And, in my boilers, while the four sides of the internal flue have been perfectly entire, the end of the flue where it turns off has been burnt through. If this internal flue communicates therefore little heat to the surrounding water, the greatest part of the steam raised, must be from the lowest stratum of water in contact with the bottom of the boiler; but by the construction this stratum has to overcome a column of 5 or 6 feet of water. Hence I conclude, that the construction of boilers to raise the greatest quantity of steam with the least fuel should be shallow, and flues placed below, with obstructions to force the flame against the bottom. Engineers object to such a form on account of the danger of burning the boiler, by inattention in the engine-keeper, in not supplying water enough always to cover the parts acted on by the fire, and the difficulty of preserving a highly elastic steam. But surely it is easy to proportion the supply of water to the evaporation, and a contrivance similar to a ball-cock in water-cisterns would obviate the first objection. By forming the crown of the boiler with a flat arch not far removed from the surface of the water, I can discover no reason why steam as elastic as in the present form of boilers may not be always at command.

You will oblige many of your readers by giving your opinion on this subject, so interesting to the arts.

I am, &c.

A. MINER. W.

* *Il opère la frite des rubis.* An expression which I do not understand.—N.

This letter arrived too late in the month for me to give an abridgement of so much of Count Rumford's excellent *Essay VI. *On the Management of Fire and the Economy of Fuel*, as is applicable to the enquiry it contains. If I can have the Count's permission to copy some of the engravings (of which I have no doubt), this shall appear next month. In the mean time I must refer my correspondent to the essay of the same author, on the propagation of heat in fluids, of which accounts are given in this Journal, vol. I. 289. 341. 563. He will there observe that fluids communicate no perceptible quantity of their heat in any other way, than by the actual contact with solids, produced by the relative motions of their parts; and from this leading fact it will follow, that the quantity of heat communicated to the boiler from a given fire, will be governed by the arrangements for causing the greatest number of particles of heated air and flame to come into contact with it, and for preventing the heat being conducted off. It is very obvious, that the boiler he describes is not calculated to absorb all the heat, but that from the shortness and straitness of the channes, those heated fluids must pass off at a very high temperature, to the consequent diminution of effect and waste of fuel. I do not here minutely enquire what may be the best figure for a steam-boiler of iron, at the usual strength of 10lb. to the square foot, to sustain an internal pressure of between one-sixth and one-fourth of an atmosphere, because every one is aware of the advantages of a figure approaching to the spherical form, and because other figures of much less strength will hold very well under the circumstances above-mentioned.

The principal question exclusive of the saving of heat, hereafter to be considered, is to determine how far the depth or shallowness of the water, in the boiler, may influence the production of steam.

If the parts of a mass of water could preserve the same relative positions, during the application of heat, and this heat were applied at the bottom under a depth of six feet, it would depend upon the conducting power of the fluid, whether the upper or lower parts should give off the greatest quantity of steam in a given time. If the lower parts were to afford the greatest product, this would be done under a greater pressure and at a higher temperature; so that it would become an object of experimental research, whether such steam, by giving out heat to the superincumbent water, through which it must rise, and rendering a portion of it elastic, might not be as effectual in its ultimate operation, as if the heat had been employed upon a shallower mass. But the fact is, that the parts of fluids do not preserve their relative situation during the application of heat: the water in the boiler will expand and rise with rapidity as it acquires heat, and, in all probability (for in this case also we are in want of facts), will give off a much larger portion of steam while it circulates near the upper surface, and is losing its elevated temperature, than while it glides along the bottom in the act of receiving heat. From this view of the subject, though I am disposed on the whole to conclude that the shallower mass of water may, in many respects, deserve the preference, yet I doubt whether the difference arising from mere pressure be an object of any considerable importance. As the medium temperature of the whole mass of a fluid, heated from beneath,

* Published before the commencement of this Journal.

is higher when deep than when shallow, it is found to be of some consequence in the distillation of fermented liquors, that the charge should not be deep. But this appears to be less on account of any supposed rapidity of evaporation, than because the flavour of the product may be altered, by such increase of temperature.

To afford a regular supply of water to the boiler, without depending upon the engine-keeper, is certainly an object of no difficulty, and is actually done in many steam-engines, by means similar to that suggested by my correspondent. In Mr. Kier's engine, described in this Journal, I. 422. the water is admitted through a valve, kept shut by a float, which falls as the water subsides by evaporation. On which particular, I may here add, that the body immersed in the water is not a simple float, but a piece of stone that hangs from the valve, which is assisted in its tendency upwards by a balance-lever and counter weight. When the stone is immersed to a certain depth, the weight attached to the lever predominates, but when the water falls below that depth, the stone exerts more of its gravity, and keeps the valve down, till the requisite supply of hot water has flowed in.

21st April, 1799.

W. N.

X.

Description of the Furnace for converting Bar-iron into Steel, by Mr. JOSEPH COLLIER.*

THE furnaces for making steel are conical buildings, about the middle of which are two troughs of brick, or fire-stone, which will hold about four tons of iron in the bar. At the bottom is a long grate for fire. The steel furnace, however, is not well adapted with description. I shall, therefore, avail myself of an accurate account, which was communicated to me by a gentleman conversant with the manufacture.

A layer of charcoal-dust is put upon the bottom of the trough, and upon that a layer of bar-iron, and so on alternately, until the trough is full. It is then covered over with clay, to keep out the air, which, if admitted, would effectually prevent the cementation. When the fire is put into the grate, the heat passes round by means of flues, made at intervals, by the sides of the trough. The fire is continued until the conversion is complete, which generally happens in about eight or ten days. There is a hole in the side, by which the workmen draw out a bar, occasionally, to see how far the transmutation has proceeded. This they determine by the blisters upon the surface of the bars. If they be not sufficiently

* Extracted from his *Observations on Iron and Steel*, in the Manchester Memoirs, V. 109.—The paper itself contains a concise and clear, though in some respects imperfect, account of the usual processes of the smelting and purifying this useful metal. This author, who, inadvertently, blames M. Fourcroy and myself for this inevitable consequence of abridgment in our elementary writings, has himself shewn that the task of describing complex operations in a few words, is sufficiently difficult to afford a claim upon the public indulgence.—On Steel, see *Philos. Journal*, I. 210. 248. 381. 468. 575; II. 64. 102.

changed,

changed, the hole is again closed carefully to exclude the air; but if, on the contrary, the change be complete, the fire is extinguished, and the steel is left to cool, for about eight days more, when the process for making blistered-steel is finished.

Fig. 1, plate IV. is a plan of the furnace; and fig. 2 is a section of it, taken at the line A B. The plan is taken at the line C D. The same parts of the furnace are marked with the same letters, in the plan and in the section. E E are the pots, or troughs, into which the bars of iron are laid, to be converted. F is the fire-place. P, the fire-bars. And R, the ash-pit. G G, &c. are the flues. H H is an arch, the inside of the bottom of which corresponds with the line I I I I, fig. 1, and the top of it is made in the form of a dome, having a hole in the centre at K, fig. 2. L L, &c. are six chimnies. M M is a dome, similar to that of a glass-house, covering the whole. At N there is an arched opening, at which the materials are taken in and out of the furnace, and which is closely built up when the furnace is charged. At O O there are holes in each pot, through which the ends of three or four of the bars are made to project quite out of the furnace. These are called *tasting-bars*, one of them being drawn out occasionally to see if the iron be sufficiently converted.

The pots are made of fire-tiles, or fire-stone. The bottoms of them are made of two courses; each course being about the thickness of the single course which forms the outside of the pots. The insides of the pots are of one course, about double the thickness of the outside. The partitions of the flues are made of fire-brick, which are of different thicknesses, as represented in the plan, &c. by dotted lines in the bottom of the pots. These are for supporting the sides of the pots, and for directing the flame equally round them. The great object is to communicate to the whole an equal degree of heat in every part. The fuel is put in at each end of the furnace, and the fire is made the whole length of the pots, and kept up as equally as possible.

XI.

On the Origin of the Areometer, by Citizen EUSEBE SALVERTE.*

AT the present moment, while Hassenfrantz is publishing his interesting memoirs upon *Areometry* in your Journal*, it will, probably, be some gratification to your readers to receive some information concerning the origin of the instrument to which that science has given its name.

In the *Encyclopédie méthodique. Physique*, I. 257, is the following passage: "It is commonly thought, that the *Areometer* was invented about the end of the fourth century by Hypacia, the daughter of Theo†, as we learn in the fifth letter of Synesius Cyreneus, in his fifth letter."

* *Annales de Chimie*, XXVII. 113.

† Hypacia was a Platonic philosopher, equally celebrated for her virtue, science, and beauty. The people of Alexandria, excited against her by St. Cyril, slew her in the 415th year of the Christian era—S.

This notice is not accurate, if the poem *De Ponderibus et Mensuris*, printed at the end of the works of Priscian, and admitted by all the learned to have been written by Rhemnius Fannius Palaemon be truly ascribed to that grammarian: Rhemnius lived under Tiberius, Caligula, and Claudius Cæsar, and, consequently, was three centuries anterior to Hypacia. The following is the description which he gives of Areometry, which is equally valuable for its perspicuity and exactness:

Ducitur argento tenuive ex ære cylindrus,
 Quantum inter nodos fragilis producit arundo,
 Cui cono interius modico pars ima gravatur
 Ne totus sedeat, totusve supernatet undis,
 Lineaque à summo tenuis descendit ad ima,
 Ducta superficie, tot quæque in frustra secatur
 Quot scrupulis gravis est argenti ærisve cylindrus.
 Hoc, cujusque potes pondus spectare liquoris.
 Nam si tenuis erit, majori immergitur undâ;
 Sin gravior, plures modulus superesse notabis.
 Aut si tantumdem laticis sumatur utrinque
 Pondere præstabit gravior; si pondera secum
 Conveniunt, tunc major erit quæ tenuior unda est.
 Quod si ter septem numeros texisse cylindri,
 Hos videas latices, illos cepisse ter octo,
 His drachmâ gravius fatearis pondus ineffe.
 Sed refert æqui tantum conferre liquoris,
 Ut gravior superet drachmâ, quantum expulit undæ
 Illius aut hujus, teretis parsima cylindri*.

It cannot, therefore, be doubted but the Areometer was an instrument well known, and commonly used, three hundred years before the birth of Hypacia. It is difficult to conceive how Synesius, contemporary and friend of this celebrated lady, could attribute the invention

* In English, "A cylinder is made of silver, or thin brass, in length equal to the distance between the knots of a brittle reed. The lower part is loaded within, so that it shall neither totally sink, nor entirely float. A fine line is drawn from the upper to the lower extremity of its surface, and divided into as many portions as the scruples which express the weight of the cylinder. With this instrument the relative weight of any fluid may be ascertained: for, if it be light, the cylinder will sink deeper; or, if heavy, the number of divisions above the surface will be more considerable. If equal bulks of different fluids be taken, the densest will exceed the other in weight; or, if the weights be equal, the rarest fluid will occupy the greater space. For example, if the cylinder be found to sink through twenty-one divisions in one fluid, and in another through twenty-four, it may be concluded, that the heavier exceeds the other by three scruples, or one drachm. But, it is more accurate, instead of attending to the difference, to compare the two fluids, by attending to the quantities of each in magnitude, which are displaced by the immersed part of the cylinder."

to her*. But the following line immediately after this description in Rhemnius, is still more remarkable :

Nunc aliud partum ingenio trademus eodem.

“ Let us now describe another invention of the same genius.”—After which he proceeds to describe the method used by Archimedes, to ascertain the quantity of silver contained in the crown of Hiero.

It appears, therefore, to be certain, that we owe the invention of the Areometer to the same man who enriched the accurate sciences with so many discoveries, and who, to the glory of his talents, added, that of living for the welfare of his country, and dying in its defence.

The poem of Rhemnius, or, rather, the fragment which remains, deserves to be known. Independent of the two descriptions I have quoted, and a complete system of ancient measures, it contains other interesting details. Such is the following observation, which supposes experiments of some delicacy on the specific gravity of liquids.

† Namque nec errantes undis labentibus amnes
Nec merfi puteis latices, nec fonte perenni
Manantes, par pondus habent : nec denique vina,
Quæ campi aut colles, nuperve aut ante tulère.

Permit me to observe the elegance and accuracy of these expressions. They appear to me to belong to a writer of good latinity : and to obviate all the doubts which can be raised concerning the date of the poem, another citation will confirm this idea :

—————Pondus rebus natura locavit
Corporeis : elementa suum regit omnia pondus.
Pondere terra manet ; vacuus quoque ponderis Æther
Inde fessa rapit volventis fydera mundi.

An author of the sixth century, such as Priscian, the only one to whom the poem of Rhemnius could be attributed, could not easily have composed these verses.

* With all the advantages of the art of printing, we find, that valuable inventions are forgotten, and re-invented, during the lapse of periods much shorter than three centuries. Whoever will peruse the writings of Boyle, Kunkel, Hooke, Wren, and the registers of the learned societies and scientific correspondences during the last century, will have ample reason to be convinced of this.—Hypacia might be the second inventor.—N.

† In English, “ The weights of river, well, and spring-water, are different ; as are likewise the wines, according to the place of their growth, in hills or places, and the time of keeping.”

XII.

Chemical Considerations on the Use of the Oxydes of Iron in the Dying of Cotton.

By J. A. CHAPTAL *.

THE oxyde of iron has so strong an attraction to the fibre of cotton, that if the latter be plunged in a saturated solution of iron in any acid whatever, it assumes a chamois yellow colour, of greater or less intensity, according to the strength of the solution. It is no less curious than easy to make the experiment of passing a piece of cotton through a solution of sulphate of iron rendered turbid by the oxyde which remains suspended in the fluid. Nothing more is necessary than to pass the cotton through the bath, from which it seizes the last particle of the oxyde, and restores its transparency. The solution, which before was yellowish, immediately becomes more or less green, according to its strength.

The colour which the oxyde of iron gives to cotton, becomes deeper by simple exposure to the air; and this colour, which is soft and agreeable at first taking out of the bath, becomes hard and ochreous by the progressive oxydation of the metal.

The colour of the oxyde of iron is very solid. It resists the action not only of the air and water, but likewise of alkaline solutions. Soap gives it brightness without perceptibly diminishing its intensity.

From these properties it is, that the art of dying has availed itself of the oxyde of iron as a most valuable colouring principle. But I have succeeded in giving a new extension to the application of this oxyde. I shall confine myself in presenting to the Institute those results only which deserve to be adopted in the practice of manufacturers, and have been executed for several years in my dying-works.

In order that the oxyde of iron may be conveniently applied to the cotton thread, its solution must first be effected. Acids are used as the best solvents.

Most dyers make a mystery of the acid they use; but the universal practice is confined to the acetous, the sulphuric, the nitric, and the muriatic acids.

Some dyers attribute great differences to the acid they use in the solution of iron; but the preference is commonly given to the acetous acid.

This predilection appears to me to be grounded much less upon the difference of the colours, which one acid or the other may give, than upon the corrosive action which each of them exerts to different degrees on the stuff. This is so great, with respect to the sulphate and the muriate, that if the piece be not washed immediately as it comes out of the bath, it will certainly undergo an effect of the same nature as combustion; whereas the solutions in the acetous, or any other vegetable acid, do not produce this inconvenience.

The iron appears to exist at the same degree of oxydation † in the different acids, since it produces the same shade of colour when precipitated; and any acid salt may be indiscriminately used, provided the nature of the salt, and the degree of saturation in the acid, be sufficiently

* Read to the French National Institute, 21 Germinal, in the sixth Republican year, and inserted in the *Annales de Chimie*, XXVI. 266, whence this translation is made.

† On this subject see, however, the *Philos. Journal*, I. 453.

known; for the subsequent operations may then be properly conducted, and the inconvenience attending the use of some of these salts may be prevented. This no doubt is a leading advantage possessed by the man of information, beyond the simple operative artist, who is incapable of varying his processes according to the nature and state of the salts he may use.

I shall confine this paper to accounts of the colour obtained from the oxyde of iron:

1. When it is used alone upon goods which have received no previous preparation, and, 2. when it is employed jointly with the red of madder, on a piece prepared to receive the Adrianople red.

1. If sulphate of iron or any other martial salt be dissolved in water, and cotton be plunged therein, it will acquire a chamois tinge, more or less deep, accordingly as the solution may be charged with the salt. The affinity of cotton with the iron is such, that it attracts the metal, and takes a great part from the acid which held it in solution.

(To be concluded in our next.)

SCIENTIFIC NEWS, &c.

Respirability of the Gaseous Oxyd of Azote.

Extract of a Letter from Mr. H. DAVY.—Dated, Clifton, April 17, 1799.

“I HAVE this day made a discovery, which, if you please, you may announce in your Physical Journal, namely, that the nitrous phosoxyd or gaseous oxyd of azote, is respirable when perfectly freed from nitric phosoxyd (nitrous gas). It appears to support life longer than common air, and produces effects which I have no time to detail at present. Dr. Mitchell’s theory of contagion is of course completely overturned; the mistake of Priestley and the Dutch chemists, probably arose from their having never obtained it pure. I am now preparing a paper on this subject, for the next volume of the West-country Contributions.”

In a subsequent communication, Mr. Davy expresses his apprehension, lest a general notice of the respirability of gaseous oxyd of azote should induce any one to make injurious experiments on himself, and therefore wishes it should be added, that the circumstances of safety and of hazard will be speedily pointed out to the public. A train of experiments, by which he hopes to clear up this perplexed subject, are in progress; and he is the more solicitous that this general notice should be published, because some most remarkable phenomena witnessed by divers persons, in the pneumatic institution, and leading to useful practices, have been a good deal talked of at Bristol, and an erroneous anticipation may get into print, before the true account can be prepared.

I under-

I understand by a letter from the Rev. Mr. Pearson, of Lincoln, that he did not mean to imply, by the words in the first parenthesis on page 53, that the earth has any perceptible irregularities in its rotation, of which he observes that a suspicion was entertained by Kepler. The inaccuracy of expression arose from his attention being directed to the length of the apparent solar day, instead of mean solar time.

New Produce of European Sugar.

I have heard much, within the last fortnight, concerning the product of sugar from a species of the beet, or mangel wurzel, by Mr. Achard. The experiments of Margraaf and others many years ago, who obtained sugar from vegetables by treatment with alcohol, are well known to chemists. But it is stated, that the chemist of Berlin, has obtained it by a process sufficiently cheap, and in quantities so considerable, that if the reports which are circulated be true, the discovery will bid fair to change a large part of the commercial system of the world, with regard to this great article of modern consumption. The experiments are said to have been confirmed, on a large scale, by Klaproth. But I must defer any further report, until the subject can be exhibited in an unquestionable shape.

A translation of Dr. Gren's *Elementary Treatise on Chemistry*, in two volumes octavo, is ready for the press. This work is an abridgement by the author, of his own system of chemistry, of which the second edition, in four large octavo volumes, was published in 1796. A work highly esteemed throughout Germany, for systematical arrangement, and the extent of knowledge it displays. Though Dr. Gren was neither of the phlogistian nor antiphlogistian order of chemists, but maintained a system of his own, in which both theories were combined, he has nevertheless chosen to follow the antiphlogistian system in his abridged work. The translator has added the discoveries made since the year 1796.

Dr. Frederick Albert Charles Gren, public professor of medicine at Halle, died on the 26th of November last, of a nervous fever, in the 38th year of his age. The public proofs which he gave of ability and industry, have rendered his loss a severe affliction to the scientific world. He was author of an *Introduction to natural philosophy*, of which the third edition was published in 1797;—*Elements of Pharmacology*, in two volumes octavo, of which the second edition was published in 1798;—and also the *Journal of Natural Philosophy*, began in 1770, of which eleven volumes are extant.

April 27, 1799.

I have received some papers from Dr. Beddoes, relating to the subject animadverted upon at page 41 of the present volume, and also a letter from Dr. Gibbs. Dr. Beddoes does not consider his documents as sufficiently interesting to the public, to deserve a place in the
Journal;

Journal; but wishes them to be printed on a separate leaf. But as such a leaf would, no doubt, be considered and preserved by all my readers as a part of the Journal, and as the general cause of morality and science is involved in discussions of this nature, I have preferred that an abridged statement should appear in the Journal itself.

The papers I have received are: 1. Letter from Dr. Beddoes, dated April 24. 2. Letter from the Rev. Mr. Richardson, of Bath, to Dr. Beddoes, dated 17 (I suppose April). 3. Questions to Mr. Notcutt, by Dr. Beddoes. 4. Mr. Notcutt's answer, dated April 21. 5. Queries to Mr. Davy, with his replies. These papers were received yesterday, and by this morning's post I received a letter from Dr. Gibbes, dated April 26. In Mr. Richardson's letter it is stated to be a misrepresentation, that Mr. Notcutt pointed out some sulphate of strontian in his (Mr. R's) collection; but he admits, that Mr. N. said the specimen in question might probably be sulphate of strontian; but Mr. Richardson does not remember ever mentioning this conjecture to Dr. Gibbes.—Mr. Notcutt in his letter admits, that Dr. Beddoes' statement of Mr. N. having recognized the sulphate of strontian, is correct to the best of his recollection; that in a late conference with Mr. Richardson, this last gentleman had affirmed, that Mr. Notcutt had not spoken with certainty in giving his opinion of the specimen at the time alluded to: upon which Mr. Notcutt observes, that he cannot pretend to repeat exactly the words he may have used, but that it is evinced, that he felt the strongest conviction of its being that substance, by his informing Dr. Beddoes, Mr. Clayfield, and others, immediately upon his return to Bristol, that it had been found near Sodbury, as well as by his authorizing the former to notice it in the volume of papers just published; that he, Mr. Notcutt (from Dr. Gibbes' knowledge, that considerable quantities of the same substances had been discovered near Bristol by Mr. Clayfield, and that Mr. Clayfield had long been engaged in a course of experiments upon its analysis, and that no official account of it was before the public), cannot account for his omitting to notice what was so immediately connected with the subject of his paper, without supposing that he wished to take to himself more than the merit of having analyzed the specimen from Sodbury.—Mr. Davy in his answers to the queries proposed to him, states, that Mr. Notcutt, in a conversation previous to the publication of Dr. Gibbes' experiment in the Philosophical Journal, did inform him that sulphate of strontian was found in large quantities near Sodbury; and admits, that Mr. Notcutt had mentioned before that period, that he (Mr. Notcutt) had positively told Mr. Richardson, that the mineral in question was sulphate of strontian. And lastly, he partly admits and partly states, that Dr. Gibbes was present at conversations respecting Mr. Clayfield's analysis, who confidentially communicated to him (facts or incidents) respecting the same mineral in the same district. And that Dr. Gibbes must have known, that by publishing his analysis prior to Mr. Clayfield's, and without communicating it to him, he (Dr. G.) would be considered as the first discoverer of the sulphate of strontian, to the prejudice of Mr. Clayfield, whose name ought to have been mentioned.—The testimony of Mr. Clayfield himself could not be obtained in time for Dr. Beddoes to send it with the other papers, because Mr. C. was then absent from Bristol.

On the other hand, Dr. Gibbes himself states, that he knew of Mr. Clayfield's experiments before he wrote me the account of his own, and had noticed them, and Mr. Clayfield's name, prior to that time, in a work published last January *; that he never knew that the variety of which he sent so imperfect an analysis had been noticed by any one, and he understands that no one has to this day subjected it to the test of experiment but himself. He thought Mr. Clayfield's experiments had been publicly announced, and now finds by the note to p. 41 of this Journal, that this was in fact the case;—that the great difference in the specimens of this mineral found in Gloucestershire, have deceived the best judges;—that Mr. Richardson positively affirms, that Mr. Notcutt did not know what the mineral in question was, but that had he even affirmed it to be the sulphate of strontian, it would be of no consequence to the case, because Mr. Richardson never mentioned his name nor his opinion to Dr. Gibbes;—that in the papers transmitted to me (which have been communicated to Dr. Gibbes), he accuses Dr. Beddoes of omitting to mention a second letter of Mr. Richardson; which was strong in favour of Dr. Gibbes;—that the idea of losing Mr. Clayfield's good opinion, for whom he entertains the most profound respect, is the only circumstance which can give him pain in this business; and that the high gratification he has received from the Philosophical Journal, produced the wish to give a testimony of approbation by a communication, in consequence of which he instantly embraced the first favourable opportunity of so doing.

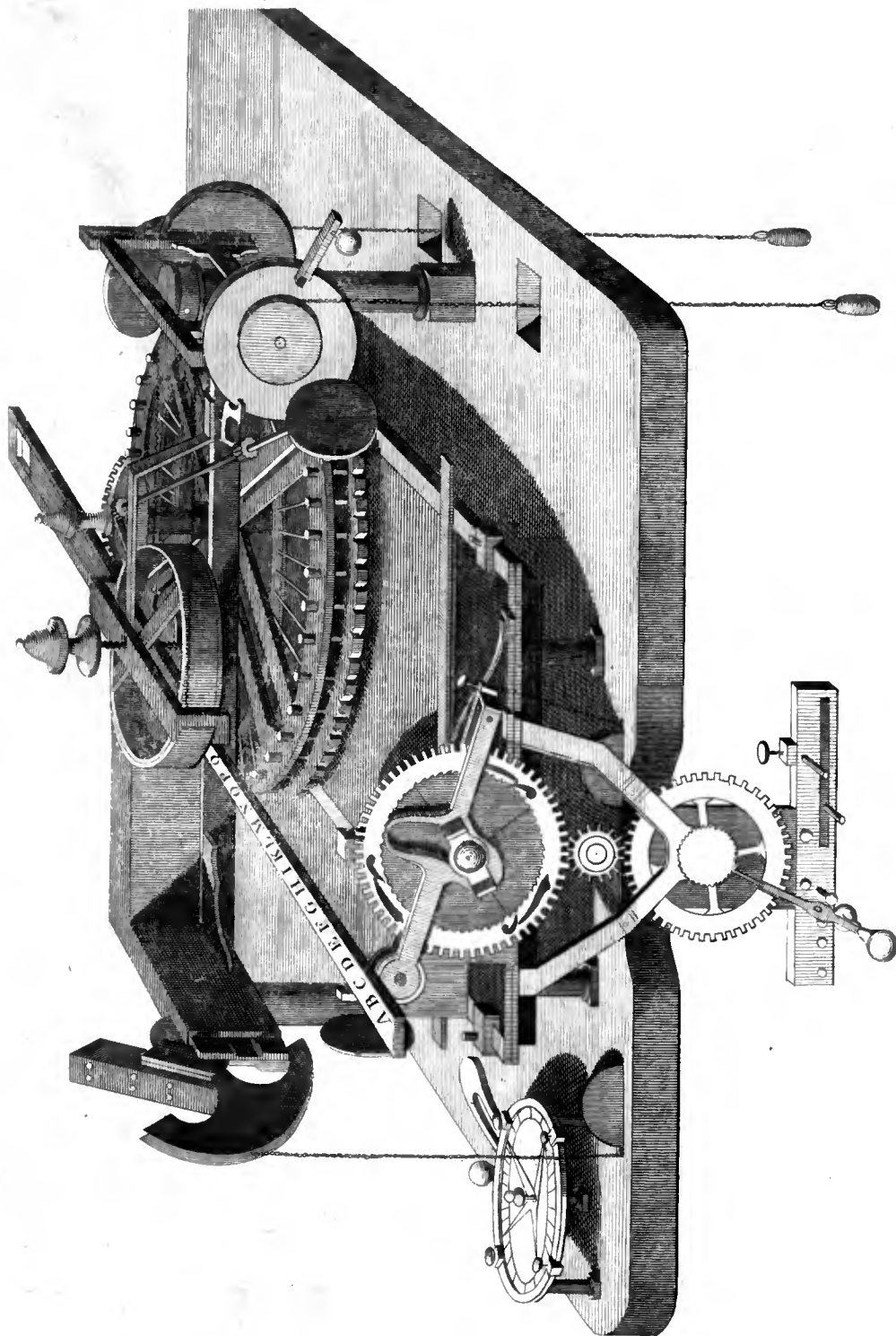
I hope that the length of this abridged account will be a sufficient apology for my not having inserted the original papers. As an individual, to whom the parties are no otherwise known than by their public labours, and respectable stations, I may venture to add the following observations.

It appears on the whole: 1. That Mr. Clayfield discovered the sulphate of strontian in England long ago, as related at p. 36 of our present volume. 2. That this discovery was imparted to hundreds of persons; to a large audience at Bristol; to a learned society at Manchester; and to a periodical work of large circulation (page 41). Whence, it seems hard to imagine, that the mere discovery could be any part of a confidential communication to Dr. Gibbes. 3. That the probability of the Rev. Mr. Richardson having mentioned the opinion of Mr. Notcutt to Dr. Gibbes, is removed by the negative of Mr. Richardson himself, as to that point. And 4. lastly, that though it might be wished, that Dr. Gibbes, while mentioning his own experiments, had noticed their connexion with those other observations of such public notoriety; yet, the chief and probably the only cause of offence in the publication, at page 535 of vol. 2. of this Journal, will be found to consist in the apparent ostentation of the title, which was not written by Dr. Gilles, but by the Editor.

* This work I presume to be his *Syllabus of a Course of chemical Lectures*, where I find the following head. "STRONTITES—Where found—Analogous to barytes—Its properties—Produces red flame when applied to a candle—In what differs from baryte.—Phosphure of strontian.—M. Pelletier, Mr. Hope, Mr. Schmeisser, Mr. Clayfield.—Sulphate of strontites found near Bristol."—N.

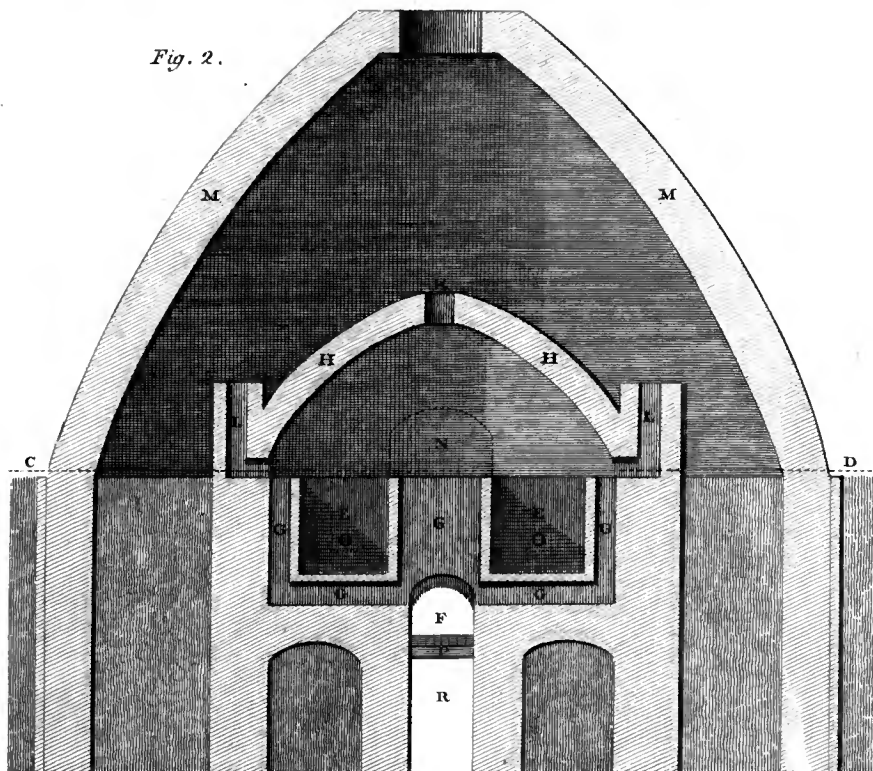
Robison's Machine for engraving Metallic Plates

Philos. Journal Vol. LIII. 19. 117. engraving p. 48.



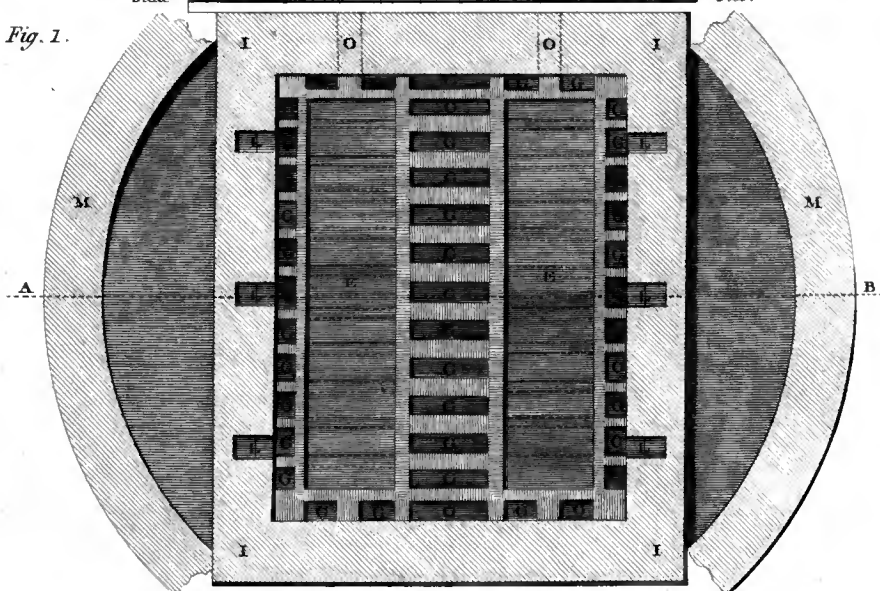
Section & plan of the Furnace for converting Bar-Iron into Steel.

Fig. 2.



Scale 0 1 2 3 4 5 6 7 8 9 10 11 12 Feet.

Fig. 1.





A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JUNE 1799.

ARTICLE I.

*An Account of some Endeavours to ascertain a Standard of Weight and Measure. By Sir
GEORGE SHUCKBURGH EVELYN, Bart. F.R.S. and A.S.**

HAVING for some years turned my thoughts to the consideration of an invariable and imperishable standard of weight and measure, as being a thing in a philosophical view highly desirable, and likely to become extremely beneficial to the public, I had, so early as the year 1780, taken up the idea of an universal measure, from whence all the rest might be derived, by means of a pendulum with a moveable centre of suspension, capable of such adjustments as to be made to vibrate any number of times in a given interval; and by comparison of the difference of the vibrations, with the difference of the lengths of the pendulum (which difference alone might be the standard measure), to determine its positive length, if that should be thought preferable, under any given circumstances; by which means all the difficulties arising in determining the actual centre of motion and of oscillation, which have hitherto so much embarrassed these experiments, would be gotten over.

§. 2. I made several computations of the probable accuracy that might be expected from such an experiment, and was satisfied with their result. But not seeing clearly how such a

pendulum could be connected to a piece of mechanism to number the vibrations without affecting them, I dropped the idea for that time. I learnt, however, some time afterwards, that Mr. John Whitehurst, a very ingenious person, had been in pursuit of the same object, with better success, and had contrived a machine fully corresponding to his expectations and my wishes. This he afterwards explained to the world in a pamphlet entitled "An Attempt to obtain Measures of Length, &c. from the Mensuration of Time, or the true Length of Pendulums," published in 1787. Mr. Whitehurst having therein done all that related to the standard measure of length, and suggested that of weight, it appeared to me that it remained only to verify and complete his experiments.

§. 3. For this purpose, by the kind assistance of my friend Dr. G. Fordyce, who at Mr. Whitehurst's death had purchased his apparatus, I was furnished with the very machine with which Mr. Whitehurst had made his observations. I also procured to be made by Mr. Troughton a very excellent beam-compass or divided scale, furnished with microscopes and micrometer, for the most exact observations of longitudinal measure; as also a very nice beam or hydrostatic balance, sensible with the $\frac{1}{1000}$ of a grain when loaded with 6lb. troy at each end. Mr. Arnold made me one of his admirable time-keepers, in order to carry time from my sidereal regulator in my observatory, with which it was adjusted, to the room wherein I had fixed Mr. Whitehurst's pendulum; and who having taken a journey into Warwickshire, was so good as to assist in the beginning of these experiments. Thus equipped, I went to work in the latter end of August, 1796, when the temperature was about 60°, first to examine the length of the pendulum; when, to my great mortification, I found that the thin wire of which the rod consisted was too weak to support the ball in a state of vibration; and that after 15 or 20 hours' action it repeatedly broke. The same misfortune attended my trials with three other different sorts of wires that I had obtained from London. Whether this accident happened from any rust in the old wire, or from want of due temper in the new, or from its being too much pinched between the cheeks*, I cannot tell. I can only observe, that all the wires that I used were considerably heavier, and, therefore, probably stronger than what Mr. Whitehurst mentions, viz. 3 grains in weight for 80. inches in length; nay, mine proceeded as far as from 5 to 6 grains for that length, and yet I could never get it to support the ball during the whole period of my experiment. This being the case, and being in the country, far removed from the manufactory of this fine wire, I was reluctantly compelled to relinquish this part of the operation to some more favourable opportunity. In the mean while, however, I thought it desirable to measure the difference of the lengths of Mr. Whitehurst's pendulum from his own observations; for very fortunately the marks that he had made on the brass vertical ruler of his machine were still visible, and this interval, which he calls "59,892 inches," I determined on my divided scale made by Troughton from Mr. Bird's standard to be = 59,89358 inches, from a mean of four different trials in the temperature of 64°; that mean differing from the extremes only = .0003 inch †.

* c c Fig. 1. of Plate II. in Mr. Whitehurst's pamphlet.

† See the remark in Philof. Journal, III. 32. note.—N.

§. 4. By this examination, if I have not verified I have at least preserved Mr. Whitehurst's standard; and, for the present, I shall consider this measure of the difference of the length of the two pendulums, vibrating 42 and 84 times in a minute of mean time, as correct. On this presumption I shall proceed to the examination of weight.

§. 5. From the opinion of different skilful persons with whom I have conferred, as well as from the result of my own considerations, I am inclined to believe, there is hardly any body in nature with which we are familiarly acquainted, that is of so simple and homogenous a quality as pure distilled water, or so fit for the purposes of this enquiry; and I have concluded, that if the weight of any quantity of water whose bulk had been previously measured by the abovementioned scale, could be attained under a known pressure* and temperature of the atmosphere, we should be in possession of a general standard of weight.

§. 6. With this view, I directed Mr. Troughton to make, in addition to the very sensible hydrostatic balance before mentioned, a solid cube of brass, whose sides were 5 inches; and also a cylinder of the same metal, 4 inches in diameter and 6 high. From St. Thomas's hospital, by favour of Dr. Fordyce, I procured 3 gallons of distilled water; with these I made the following observations: but before I relate the experiments, I will describe the apparatus.

Mr. Whitehurst's machine for measuring the pendulum, has been sufficiently explained in his pamphlet mentioned above; my divided scale, which was a new instrument, was as follows:

§. 7. *Description of the Beam-compass, or Divided-scale of equal parts.*

ab (Plate V. fig. 1.) is a block or beam of mahogany, 6 feet 3 inches long, 6 inches deep, and 5 wide, upon which are laid two brass rules, *cde* and *fg*, each divided into 60 inches and tenths. The former of these, called the scale, is for a time kept immoveable by the finger-screws *ce* *d*, and is furnished with fine hair-line divisions, intended to be viewed only by the microscopes, *bi*: the latter, called the beam, has no motion but by means of the screw *g*, and bears stronger divisions upon it, with which the sliding pieces or indexes at *k* and *m* may readily be compared by the naked eye, and is intended only to set the microscopes, or rather the wires in their focus, to the required distance nearly, viz. to within $\frac{1}{100}$ or $\frac{1}{200}$ of an inch. The microscopes are compound, and similar to those described by the late General Roy in his account of his large theodolite. (See Phil. Trans. vol. LXXX.) The one at *b* contains only cross-wires fixed in its focus; the other at *i* has a micrometer also, by means of which its cross-wires may be moved to the right or left, or over the image of the divisions of the scale, any given space not exceeding $\frac{1}{10}$ inch; and the quantity so moved may be measured by the divisions on the screw-head passing under the index at *o*. The divisions on these rules have been called inches and tenths; it was not necessary that they should be more than equal parts; but they were in fact laid down by Mr. Troughton from a scale of the late excellent artist Mr. J. Bird, who had divided into inches several scales of different lengths; one of which, 42 inches long, belonged to the late General Roy; a second, of 5 feet, was

* I do not here mean to infer any opinion respecting the compressibility of water, but only to say, that where water or any thing else is weighed in air, the density of that medium, as shewn by the barometer and thermometer, must be known in order to make allowances for it if necessary.

purchased by Alexander Aubert, esq.; and a third, of 90 inches, which is now the property of the Royal Society, is kept in their archives, and is said to have been used by Mr. Bird, in dividing his large mural quadrants*. Besides these, he made two standards of three feet, by order of the House of Commons, of which I shall speak more hereafter. The mode of using this instrument is as follows.

§. 8. Let the object to be measured be supposed to be about six inches, and let it be desired to compare it with the interval between the 20th and the 26th divisions in the scale $c d$: move by hand the microscope b , with its sliding plate, until the division of the index at k coincide with the division of 20 inches on the rule of $f g$; then move by hand also the microscope i , with its sliding plate, and appendage $l m n o$, until the index division near m coincides with 26 on $f g$: the axes of the microscopes, or centres of their cross-wires, will be at the approximate distance of 6 inches. To correct this, examine if the wires of p correspond with a division on $c d$; if not, move the rule $f g$ backward or forward by the screw g till they do; then will the microscope b be adjusted. Now examine if the wires in i cover exactly a division; if they do so, the true interval of 6 inches between the microscopes is obtained; if not, move the microscope i a little by means of the screw l till they do, and both the microscopes will be adjusted: then remove the rule $c e d$ from its place, by taking out the screws $c e d$, and place the object to be measured in its room, at the same time taking care that it be exactly in the focus of the object-glass of the microscope, in such a manner that one extremity may correspond with the wires in the microscope b ; that done, if the other extremity coincide with the wires in i , the dimension of the object is exactly 6 inches; if not, restore the coincidence by turning the micrometer screw n , and the divisions at o will give the difference in 1000ths and 10,000ths of an inch $+$ or $-$ 6 inches.

§. 9. *Description of the Hydrostatic Balance.*

$a b c d$ (Pl. V. fig. 4.) is a box, which contains the whole apparatus when not in use; and when used, serves as a foot to the hollow brass pillar $e f g h$, which is fixed into it by the four screws at the bottom e and f . This pillar contains another within it, and which is raised up and down about $\frac{1}{10}$ inch, by means of the screw x : $n o$ is the beam, 27 inches long, and 3,9 inches wide in its greatest diameter; each arm of which is made hollow and conical, for strength and lightness; through the centre at m , passes the axis of motion, the ends of which, when used, are suffered to fall gently upon two crystal planes, which are set horizontally, by means of the spirit levels $k l$, and the screws underneath the box, at c and b . The ends of this axis are of hardened steel, of a wedge-like shape, and reduced to a fine edge, viz. to an angle of about 40° , so as to move upon the planes with very little friction, and at the same time so hard, as (with due care in using) to be in no danger of being blunted: to prevent which, the inner pillar has a motion upwards, as has been said, by the screw x , and by means of a semi-circular arm at its upper extremity, lifts the beam of its bearings, when it is not used, or is greatly loaded. This axis is placed carefully at right-angles to the beam; and by means of two small brass springs

* A farther account of these scales is given in the appendix.

that prefs gently at the ends, is brought always to have the same bearing upon the crystal; so that no error need be feared from a small deviation from the right-angular position of the axis to the beam, should any such exist, and from its shape and quality, it may be considered as inflexible in ordinary experiments. At *p*, is a small adjusting screw, which raises or depresses a weight within; and with it, in consequence, the centre of gravity in the whole beam; by this means the motion in its centre may be brought to almost any required degree of sensibility. Should the centre of gravity be raised above the centre of motion, the beam would turn over; if it be in that centre, the beam would stand every where indifferently, without any vibration; if it be placed much below it, the vibration would be too quick, and its sensibility not sufficient; it is therefore brought by the screw *p* a very small quantity below the centre of motion, so as to describe one vibration in 40 or 50 seconds: the sensibility is then fully sufficient. At each end of the beam are circular boxes, *n* and *o*, through which pass the steel centres, from whence are suspended the scale pans *q* and *r*: these centres resemble, in some degree, those at *m*, but have their chamfered or angular edges upwards, and thereon hang the hooks *β*, to which are affixed the links *α*, and to them the three silken lines of the scale. Each of these centres has a motion in its respective box, by means of two small adjusting screws; that in *o* laterally, and that in *n* vertically; the former to make the two arms of the beam of an equal length, the latter to bring the three points of suspension of the beam and scales into a right line. At the extremity of the boxes are fixed two needle points, or indexes, which play against the ivory scale of division at *s* and *t*. These divisions, although they do not, indeed they cannot, shew any definite weight, are nevertheless very useful in making the adjustments, and even in weighing to the small fractions of a grain. *u v* are two steady plates that are raised or depressed by the wooden nut *w*, to check the vibrations of the scales *q* and *r*, and bring them speedily to an equilibrium; *y z* is a table, whereon the whole is placed, to raise it to an height convenient for experiments*.

To use with this beam I had three sets of weights made, viz.

The first set, or series, of fifteen weights, rising in a duplicate progression, from one to 16384 grains, viz:

No.					Grains.
1	=	-	-	-	1
2	=	-	-	-	2
3	=	-	-	-	4

* This beam is constructed after that made by Ramsden for the Right Honourable Sir Joseph Banks, Bart. P.R.S. &c. and known by the name of the Royal Society's Balance. It is to be regretted that we have no good account of that instrument. The chief differences between this, and the beam described in the text, are, 1, that certain pieces, or rings, similar to those called *stops*, in telescopes, are driven into the cones, and greatly prevent flexure, by preserving the circular section. For a tube can scarcely bend, unless its cavity be made to change its figure: and 2, the planes which bear the fulcrum are set and ground together, and are levelled by a spirit level placed on the face of the stones themselves.—N.

Experiments to ascertain

No.					Grains.	Fractions of a Grain.
4 =	-	-	-	-	8	
5 =	-	-	-	-	16	$\frac{1}{32}$
6 =	-	-	-	-	32	$\frac{1}{16}$
7 =	-	-	-	-	64	$\frac{1}{8}$
8 =	-	-	-	-	128	$\frac{1}{4}$
9 =	-	-	-	-	256	$\frac{1}{2}$
10 =	-	-	-	-	512	
11 =	-	-	-	-	1024	
12 =	-	-	-	-	2048	
13 =	-	-	-	-	4096	
14 =	-	-	-	-	8192	
15 =	-	-	-	-	16384	

The second series of weights in an arithmetical order, as follows, viz.

Grains.	Grains.	Grains.	Grains.	Decimal Fractions of a Grain, viz.	
				100th Grain.	Tenths.
1	10	100	1000	,01	,10
2	20	200	2000	,02	,20
3	30			,03	,30
4	40	400	4000	,04	,40
5	50			,05	,50
6	60	600	6000	,06	,60
7	70			,07	,70
8	80	800	8000	,08	,80
9	90			,09	,90
			10,000		
			20,000		

N.B. The fractions of a grain are made of fine wire flattened.

The third set consists of a weight of $\left\{ \begin{array}{l} 1 \text{ ounce} \\ 2 \text{ ounces} \\ 4 \text{ ounces} \\ 8 \text{ ounces} \\ 1 \text{ pound} \end{array} \right\}$ troy.

§ 10. *a b c d* and *a b c d* (Plate V. fig. 5.) is the brass cube of five inches, that has been mentioned, suspended in its own scales by means of four fine wires from the arm *o* of the beam, fig. 4. by taking away the common scale *a r*. The cube rests upon a cradle, or cross, three arms of which are seen at *g h i*, and by this means may be weighed either in air or water, by immersion into the large glass vessel *g h*, fig. 7.

At fig. 6 is seen the cylinder *a b c d* and *a b c d*, four inches in diameter and five high, slung

slung in another cradle, part of which is seen at *g b h i*, supported by four wires from the point *f*.

In fig. 7, is seen a sphere of brass, *d*, six inches in diameter, slung in a cradle, *a b c*, by three wires*, from the links *f*, suspended in a glass jar†, containing near four gallons of water, whose temperature is shewn by a thermometer at *e*.

§ 11. It was necessary to measure the exact size and correctness of figure of this sphere. For this purpose was made a wooden gauge or frame *a b c d e* (Plate V. fig. 2), in which the sphere was placed upon semicircular pieces, within-lined with green cloth to prevent bruising it; upon this frame was placed a brass square *k l m n*, whose sides were about $\frac{1}{100}$ inch in length more than the diameter of the sphere. This square, by raising or lowering the screws *o r s*, was easily made to coincide with a plane passing through the centre of the sphere. *p* is a micrometer screw, the interior extremity of which is brought just to touch the surface of the sphere, while the opposite side bears gently against the interior side of the frame at *o*; and by turning the sphere round so as to present different diameters to these points of contact, any variety in the diameter may be seen by the index *l* and plate *q* divided into 10,000ths inch. To render this operation more convenient, three great circles were drawn with a pencil upon the sphere, at 90° distance from each other (the two former were traced by the artist in the lathe, while the sphere was making, and the third was drawn from them), and each was divided into 8 equal parts. The immediate result of these experiments would only give the differences, and not the absolute quantity of the diameter; for this purpose, a brass ruler, *r*, fig. 3, was made of such a length as just to go within the brass frame *k l m n*; and being substituted in the place of the sphere, could easily be compared with any given diameter, and afterwards measured with the divided scale, fig. 1. With these instruments I made the following observations, August 31, 1796, the thermometer being at 61°.

§ 12. Examination of the dimensions of the brass cube, by means of the divided scale.

The microscope and micrometer being both adjusted, as well with respect to their focus‡

* These wires were of such a size that 91 inches weighed 20,71 grains, consequently 1 inch = 0,2276 grain, and the three wires = 0,6828 grain; and their specific gravity being 8,7, their loss of weight by sinking 1 inch in water, would be 0,0785 grain. This correction it may be necessary hereafter to attend to.

† The glass jar is made somewhat conical, being in

	Inches.
Diameter at top	12,0
Ditto at bottom	8,7
Mean ditto	10,35
Mean height within	11,8
Contents in cubic inches	992,78
Which is in ale-gallons	= 3,8 = 15½ quarts.

It may also be noted, that one inch in depth of the water near the top is = 113 cubic inches, which is equal to the exact bulk of the sphere, as will be seen hereafter.

	Inches.
‡ The focal length of the object lens is	= 0,75
The distance of the cross-wires from the object lens	= 2,00
The focal length of the combined eye-glass	= 1,50
Whence the magnifying power of the microscope becomes	= 14,2 times.

as to the value of the micrometer scale, the cross-wires in their focus were removed to a distance from each other of five inches *nearly* on the beam (the former being at 27, and the latter at 32 inches), and then *correctly* adjusted to this interval on the divided scale. I must observe, indeed, that the value of the micrometer scale was not exactly ten revolutions of the screw to $\frac{1}{10}$ inch, as Mr. Troughton designed; but this measure by the screw*, from 6 trials, was deficient by 0,0002 inch; viz. two ten-thousandths of an inch were to be added to each tenth of an inch measured by the micrometer, and so in proportion for a less quantity; but this correction is hardly worth notice.

	On the scale. Inches.	
The interval of the cross-wires in the microscope,	Inch	Inch
and micrometer	27	32 = 5,0000
Interval of ditto on another part of the scale,	26	31 = 5,0000
viz.		
Ditto ditto	25	30 = 5,0001 †

I therefore say, this interval was 5 inches correctly, to within less than the twenty-thousandth part of an inch on this scale.

Measurement of the cube, viz. of the side 1 (see fig. 5.)

Inches.		Mean.
From a to $b = 5,0114$	therefore = 4,9886	Inches. } = 4,98882.
a to $c = 5,0115$	_____ = 4,9885	
c to $d = 5,0105$	_____ = 4,9895	
b to $d = 5,0113$	_____ = 4,9887	

The side 2.

From a to $b = 5,0106$	_____ = 4,9894	} = 4,98955
a to $c = 5,0098$	_____ = 4,9902	
c to $d = 5,0102$	_____ = 4,9898	
d to $d = 5,0112$	_____ = 4,9888	

Height of the cube, from side 1 to side 2.

From a to $a = 5,0110$	_____ = 4,9890	} = 4,98925 †
b to $b = 5,0105$	_____ = 4,9895	
c to $c = 5,0107$	_____ = 4,9893	
d to $d = 5,0108$	_____ = 4,9892	

- * One revolution of the screw of the micrometer was $\frac{1}{100}$ inch.
 Each grand division, of which there were ten . . . $\frac{1}{1000}$ inch.
 These again subdivided into five each, became . . . $\frac{1}{5000}$ inch.
 And half a division, which is very visible, is . . . $\frac{1}{10000}$ inch.

† It cannot escape notice, that all these measures were something less than 5 inches, the quantity proposed; it arose from this, Mr. Troughton informs me, he was more solicitous to obtain a true figure, than the exact size; neither of which, however, were very important, as both were to be proved by the mode I have adopted. What was important was to have the sides true planes, and these were examined, as I am informed, by the reflected image of the moon seen through a large telescope, the focus of which would be altered, if the surface were either hollow or convex.

§ 13. Now

§ 13. Now the three foregoing mean measures of the side of the cube, multiplied into each other, will give $=124,18917$ cubic inches for the contents of the brass cube, which must be very near the truth; for if not, let us suppose the error in taking each of these measurements to be half a thousandth of an inch, which is much greater than is probable, viz. $=\frac{1}{10000}$ part of the side of the cube; and let us suppose each of these errors to lie the same way, which is also very improbable; in that case, the error in determining the solid content would be only $\frac{1}{10000}$ of the whole; in the above instance, about 0,03 cubic inch: but more probably the error does not amount to half this quantity.

§ 14. Examination of the cylinder.

The micrometer and microscope of the divided scale (Plate V. fig. 1.) being removed till their cross-wires were four inches distant, viz. from 54 inches to 58 inches, and the thermometer at 62°, I observed of the end or base of the cylinder, No. 1. fig. 6.

	Inches.	Inches.	Mean.
The diameter <i>a b</i>	$=4\text{---},0027=3,9973$	Inches. } $=3,99745$	
<i>c d</i>	$=4\text{---},0024=3,9976$		

End 2, of the cylinder.

	Inches.	Inches.	
The diameter <i>a b</i>	$=4\text{---},0014=3,9986$	Inches. } $=3,99785$	
<i>c d</i>	$=4\text{---},0029=3,9971$		

Height of the cylinder.

The microscope and the micrometer being placed respectively at 52,1 inches and 58,1 inches, viz. at the interval of exactly 6 inches on the scale, I found

	Inches.	Inches.	Mean.
The height from <i>a</i> to <i>a</i>	$=6\text{---},0049$	$=5,9951$	Inches. } $=5,99502$
<i>b</i> to <i>b</i>	$=6\text{---},0047$	$=5,9953$	
<i>c</i> to <i>c</i>	$=6\text{---},0047$	$=5,9953$	
<i>d</i> to <i>d</i>	$=6\text{---},0054$		
Repeated	$\left\{ \begin{smallmatrix} 58 \\ 56 \end{smallmatrix} \right\}$	$=5,9944$	

Now the mean diameter of the cylinder having being found

Inches.

At the end 1 $=3,99745$

At the end 2 $=3,99785$

The factor for the square of the diameter of a circle to find the area, being, as is well known. } $=0,7854$

And the height of the cylinder $=5,9950$.

The above four quantities multiplied into each other, give for the contents of this cylinder in inches $=74,94823$; and this result may be taken at least as correct as that of the cube, viz. to about the third place of decimals.

§ 15. Having adjusted the beam of the balance (fig. 4.) with respect to the length

of its arms, its centre of gravity, and the three points of suspension of the beam and scales, and having examined the weights, I proceeded to the remaining parts of this experiment.

September 2d, 1796. The balance-beam adjusted by the screw *p*, till the vibrations were so slow as to require more than 50 seconds of time for each, $\frac{1}{10}$ grain appeared to move the index through three divisions* of the scale *s* and *t*, $=\frac{1}{7}$ inch; when the beam was not loaded; but when the beam was loaded with 16384 grains, or near 3lb. troy, $\frac{1}{10}$ grain was equal only to $0\frac{1}{2}$ division † of the same scale.

§ 16. September 4th. The thermometer being at 63°, and the barometer at 29,36 inches,

	Oz.	Grains.	Grains.
The weight of the counterpoise to the pan or scale for weighing the cube in air, was	}	= 1 75,02	= 555,02
To which the weight of the common pan, with the silk lines on the left arm of the beam, and marked with <i>x</i> , the common right-hand pan having been removed			
And the whole weight of the pan or apparatus for weighing the cube in air, becomes			
§ 17. The counterpoise to the pan or scale for weighing the cylinder in air, was found	}	= 1 72,34	= 552,34
To which add the weight of the common pan on the left arm, as before			
And the whole weight of the pan or scale for weighing the cylinder in air becomes			

Note, in the preceding and such-like experiments, the common right-hand scale being removed, and the left-hand scale being always used and always the same weight, viz. 413,40 grains, when either the cube or cylinder, or any large body, is weighed, notice need only to

* Twenty divisions are $= 1,0$ inch.

† That is, the beam was sensible with $\frac{1}{160000}$ part of the whole weight. Mr. Harris's beam, with which he and Mr. Bird made their observations on the exchequer weights, turned with $\frac{1}{230000}$ part of the whole weight, and was consequently only one $\frac{1}{2}$ part so sensible as this. See "The Report of the Committee of the House of Commons in 1758, to inquire into the original standards of weights and measures in this kingdom, and "to consider the laws relating thereto." See also a second Report in 1759; both of which contain a vast deal of useful information on this subject, extending through fifty folio pages, and are to be found in the 2d vol. of Reports, from 1737 to 1767. A bill was brought in, in consequence, but afterwards dropped; and it is much to be lamented that this inquiry did not go to the full length of an act of Parliament. Note farther, the largest of the beams, of which there are some, of different sizes now made use of in the duchy court of Lancaster, for the actual sizing of the weights of the kingdom, is about three feet long, and is moveable with about 30 grains when 56lb. avoirdupois are in each scale, viz. about $\frac{1}{1800}$ part of the whole.—G. S. E.—An account of nine other balances may be seen in my *First Principles of Chemistry*, chap. VI.—N.

be taken of the counterpoise weight, viz. 555,02 grains, or 552,34 grains respectively; and these are to be deducted from the general amount of all the weights in the left-hand scale marked x ; but it certainly would have been more convenient to have had single weights ready adjusted for these counterpoises, both in air and water. These, though at first omitted, have since been supplied.

Grains.

§ 18. The counterpoise to the scale for the cube } in distilled water, with the heat of 61° -	= 442,75
To this add the weight of the common scale as before	= 413,40
And we have the whole weight of the scale for } the cube in water - - - - -	= 856,15
But the weight in air having already been found	= 968,40
The difference of the weights - - - - -	= 112,25
Gives for the specific gravity of this brads -	= 8,62
§ 19. The counterpoise to the scale for the cy- } linder, in the same water with the same heat	= 441,68
To this add the weight of the common scale as before	= 413,40
And the whole weight of the scale for the cylinder } in water becomes - - - - -	= 855,08
Its weight in air has already been found -	= 965,74
The difference of these weights - - - - -	= 110,66
Gives for the specific gravity of this brads -	= 8,78
The mean specific gravity of this brads, and brads- } wire, may therefore be put about -	= 8,7

N.B. The tables of specific gravity, give that of wrought brads from 8,00 to 8,20. It was necessary to ascertain the specific gravity of the brads wire, to make the correction mentioned in the note to § 10; for as it was highly possible that in experiments with this hydrostatic balance, the scales for the cube and cylinder would occasionally be immerfed to different depths in the water, and their weights would be altered as more or less of the wires, by which they were suspended, remained out of the water.

Grains.

I accordingly found that 80 inches in length of } this wire, used in the scales for the cube and } cylinder, weighed in air - - - - -	= 6,16
---	--------

And consequently 1 inch would be = 0,077 grains, and four wires of 1 inch = 3,08 grain, which divided by the specific gravity, viz. $\frac{308}{8,7}$, would give 0,0354 grain for the correction of every inch that the scale was sunk lower in the water; and so in proportion.

(To be continued.)

II.

Letter from Dr. BEDDOES respecting Cit. FOURCROY's Account of the Discoveries of MAYOW.
To MR. NICHOLSON.

SIR,

May 10, 1799.

IT would be easy to write a comment upon Mr. Fourcroy's exposition of the merits of Mayow, as long as the exposition itself (see *Annales de Chimie*, No. 85); I wish, however, to submit only two or three remarks to that author's candour. From unremitting occupations of a more important nature, Mr. F. was, I suppose, prevented from bestowing much time upon Mayow. Marks of haste and of want of information are very apparent. I will not insist upon such an error as translating *taedae ferali*—*torche cruelle*, or upon the wrong assertion that I republished Mayow in 1790. In general, the French author has bestowed ample praise upon our countryman; but in the estimate of his understanding he has failed most materially. He has neither entered into his views nor rightly conceived the spirit with which Mayow laboured. Mr. F. says, that *the thread which he found soon broke in his hands; that he did not suspect the extent of the career which he opened; and that he was not sufficiently struck with the singularity and importance of his first discoveries* (p. 89). It would be extraordinary indeed, if a person who outstripped his age in a degree of which there is no other example in the history of science, had not been endowed with superior comprehension of mind. And Mr. F. is directly contradicted by Mayow's dedication. Never was a sense of the importance of a man's writings more fervently expressed, "*Quæ autem de Nitro scripsimus, ea se per universam ferè naturam diffundunt! resque abstrusas explicant quarum plerasque e numerosâ scriptorum turbâ vix quisquam attiget*:" and so on. Had Mr. F. known that Mayow died at 27 or 28, he would not perhaps have talked of the thread breaking in his hands, and of his only opening the career. What Mr. F. advances concerning causes of the greater popularity of Boyle, appears to me ill founded. But I will not encroach upon your Journal, by any further observations, unless you or your readers desire it.

I am, Sir, your humble servant,

THOMAS BEDDOES.

III.

Observations and Experiments on various Saponaceous Compounds, particularly the Fish Soap of
Sir JOHN DALRYMPLE. By Mr. ROBERT JAMESON F.L.S. &c.

SIR,

I INCLOSE you a paper on the making of Soap from Fish, a scheme which has been for some time the object of very considerable attention in this part of the island. As many seem to view it in a more favourable light than it deserves, I conceive it to be doing justice to the public, to publish, through the means of your excellent Journal, a fair statement of its plausibility.

I am, with respect, Sir, your obedient servant,

ROBERT JAMESON.

Sheriff Bræ, Leith, May 13th, 1799.

To Mr. NICHOLSON.

On

On Soap, and particularly the Combination of Fish and Potash, called Fish Soap.

THE utility of this substance must have made it an object of early attention, for as mankind advanced in civilization, and associated in villages and towns, saponaceous matter became necessary, not only for the purposes of cleanliness, but for other cotemporary arts. Although this seems to be the probable origin of the manufacture, we have no written record of its use until the time of Pliny, who attributes its invention to the Gauls. Their soap seems to have been a compound of tallow and potash, for Pliny observes, that it was made of tallow and ashes, but the best of goats' tallow and the ashes of the beech-tree. Besides this soap, we have early accounts of the use of different deterfive substances, particularly, ashes of vegetables, natron, and urine; several of the ancient writers mention saponaceous vegetables, that appear to have been used for cleansing clothes. Beckman, professor of economy at Gottingen, is led to suppose, on the authority of Strabo, that bran was employed by the Romans, for the purposes of washing such cloth as would be injured by the hard rubbing, and strong deterfive power, of an alkali or common soap.

The Romans also used different natural combinations of the argillaceous earth, for the purpose of fulling. These were imported from different countries, as the Chia, Lemnia, Sarda, Samia, &c. They varied much in their deterfive power: the best and most valued was named cimolia. This has been considered by several mineralogists, as the same with our fullers-earth; but Mr. Hawkins has lately discovered the true cimolia of Pliny, which is very different from the fullers-earth, upon the island of Cimolo, or what is now denominated Argentiera*.

During the course of ages, several alterations have been made in the art of soap-making; but these, like many other improvements, have occurred accidentally to the manufacturer, who in general was but ill qualified to make the best use of them. Their prejudices, and the vague general knowledge of the man of science, have been the greatest bars to improvement. Happily, circumstances are now altering, the artist and the philosopher are combining their efforts to improve the manufactures, and to establish such a connection among them as will be for their mutual advantage. The great institution now forming in London, will contribute very much to this end, and afford us another proof of the superiority of the British nation, in every thing regarding the improvement of manufactures.

The accounts which have been published of the process for soap-making, are in general vague, and often contradictory. The report in the 19th vol. of the *Annales de Chimie*, is the most complete treatise that has ever appeared upon this subject. It is a model which deserves to be imitated, not only for the accuracy of the details, but the comprehensive manner in which the whole is treated.

Different Kinds of Soap.

I cannot pretend to enumerate in this paper all the kinds of soap which are now manufactured, but will only mention the most important.

* Plinius hist. nat. lib. 35, cap. 57.—Agricola de naturâ Foss. lib. 2.—Lenz. Mineralogistes Hand-buch. Emmerling, Lehrbuch, vol. 3.—Klaproth. Beitrage.

In France, both hard and soft soaps are manufactured in very considerable quantity, but the hard soap is the most generally used. The hard soap is principally made from the oil of olives and barilla, and it is found that twelve hundred weight of oil, with a sufficient quantity of barilla ley, forms a ton of soap*. Manufacturers, however, do not always rest contented with such a return; but by using many unwarrantable practices, increase the weight of the soap, without adding to its value as a cleanser. Thus we are told in the French reports, that Quesnot has published a method of increasing the weight of soap in a four-fold proportion. This he does by mixing with it alumen, muriate of soda, starch, chalk, soda in powder, oil, tallow, and water; and this stupid process he announces under the title "*De belle augmentation de savon.*"

In France the soft soap is made with potash and different vegetable oils, as oil of turnip, hempseed, lintseed, &c.

In Hungary, soap is made with tallow and natron, according to Townson, who has given us a vague account of their method of manufacture. In many parts of Germany and Russia, tallow soap is manufactured; and in Russia a good hard soap is made with butter: the butter which is used for soap making, is in general damaged salt butter, which does not make a profitable soap; this is owing to its rancidity, and the quantity of salt and cheesy matter diffused through it. Weigleb assures us that a soap is manufactured with yellow and white wax, which is hard and firm, with an agreeable smell like almonds.

As no considerable quantity of vegetable oil can be procured in Great Britain, our soap is always made with tallow, or fish-oil; sometimes also, the refuse of kitchens and rancid butter have been used. The soaps manufactured in the great way are white, mottled, yellow, and soft soaps.

1. *White Soap.* This is made from tallow, with a ley of barilla, or kelp; the tallow is reckoned good if 13cwt. with the proper proportion of ley, affords a ton of soap. It is the most expensive of the hard soaps, and is principally used for the finer kinds of work.

2. *Mottled Soap.* This is the next in value, and is manufactured from tallow, kitchen-stuff, and barilla. The mottled appearance is given, towards the end of the operations of boiling, by dispersing the ley through the soap. In France, they follow a method, which is more simple, and surely more successful. Their method is as follows:—After the soap has been separated from the waste ley, it is boiled again with a fresh portion of caustic ley, then a quantity of the solution of sulphate of iron is added, which by its decomposition, deposits its oxyd through the soap, giving it a bluish colour. The soap is now allowed to cool slowly, when it rises above the waste ley; this ley is then drawn off, and the soap melted by itself, before casting into frames.

3. *Yellow Hard Soap,* is made with tallow and rosin; and these are reckoned good, if 10cwt. of tallow, 3½cwt. of rosin, with the proper quantity of barilla, or kelp ley, afford a ton of soap. The rosin is used merely to cheapen the soap, for it certainly does not increase its power as a cleanser. Rosin and alkali boiled together, without tallow, form a viscid tenacious mass, of a very inferior nature as a cleanser.

* Annales de Chimie, vol. 19.

4. *Soft Soap*, is made with whale or fish oil; but I am not acquainted with the proportion used. The process for making it differs from that of hard soap, in this circumstance, that none of the ley is drawn off, but the whole is allowed to combine with the soapy compound. On this account, it is necessary to use a pretty pure alkali, particularly that it should be free from sea-salt, which is a substance of the greatest detriment, as it causes the soap to separate from the ley, and rise to the top of the vessel. The oil and alkali (which must be pot-ash) are boiled together, until a combination has taken place; then a quantity of tallow is added, which, by particular management, is dispersed through the soap, in the form of white spots, giving it a beautiful appearance*.

Having thus cursorily mentioned the principal kinds of soap manufactured in different countries, I will now, in as few words as possible, state the process followed in this country, for making common white soap. This will enable us to judge with more certainty of the plan lately proposed for making hard soap, by mixing the muscular fibre of fish with the tallow.

Process for making White Soap.

The tallow is first melted in the vessel with weak barilla ley, the mixture is then gradually brought to a boiling heat, and we continue adding alkali, until the whole of the tallow has formed a saponaceous compound. We know this has taken place, if the compound has a milky appearance, with a considerable degree of consistence, and seems to be separating from the liquor below. This separation is a very material part of the operation; and to effect it completely, a quantity of sea-salt, or weak kelp ley, is added, the materials are continued boiling for two or three hours, and then the fire is withdrawn. The soap will now be found united at the top of the liquor, or what is called the *waste ley*†, which is of no further use, and

* I should be happy to learn the proportions used in soft soap making, and also the particular management of the tallow in producing the white spots; and if this tallow be of any service.

† The ley which is found below the soap, and known under the name of waste ley, is of a very mixed nature, and the variety is much increased, according as potash, barilla, or kelp, are used in the manufacture. Several substances always occur, whatever be the nature of the saline compound employed. These are,

1. *Gelatine*. A considerable quantity of this substance (which had been previously combined with the tallow) is found in solution, or suspended in the ley: the caustic alkali has so strong an action upon it, that a portion is decomposed, the hydrogen and azote flying off in the form of ammonia, while the carbonaceous matter remains behind, giving a dark blue, or blackish colour, to the undecomposed gelatine.

2. As tallow contains sebatic acid, it unites with the alkali, and sometimes with the lime, forming sebars.

3. *Carbonat, or uncombined alkali*. However accurately the manufacturer works, still a quantity of the alkali escapes, and is found in the waste ley. Besides the substances we have already mentioned to be common, other combinations occur. Thus if kelp be used alone, in making the soap, besides the gelatine, &c. the ley contains muriate of soda, sulphate of soda, sulphurets of soda, and potash. As potash is never worked by itself in making hard soap, but generally with kelp (at least in Scotland), we obtain different saline combinations; these are muriate of potash, muriate of soda, and sulphate of soda; but this last is in small quantity: sulphuret of soda also occurs. If barilla is employed, we have muriate of soda, sulphate of soda, and sulphuret of soda, in the ley. The nature of the saline compounds found in barilla ley, are often considerably different from what has been now mentioned; particularly, when the soap is made with Catalonia barilla. This is owing

and is therefore to be drawn off by a pump or syphon. The soap is now melted for the last time, with a small quantity of water, or what is better, weak kelp ley; it is then allowed to cool for a short time, and afterwards cast into frames. The melting with water is of considerable importance, as it gives the soap a smooth and compact appearance, or what the workmen term a skin.

Having premised this much with regard to the different kinds of soap, which are already known, I will now proceed to consider the plausibility of a late proposal, for the manufacturing of soap with fish and tallow, or by using fish alone, dissolved in potash.

Attempts have been made at different times to procure a substitute for tallow; but in no instance has it been attended with success. Several years ago, when the Greenland fishery was more considerable than it is at present, soap manufacturers used a considerable quantity of oil, in making their hard soap; but the strong oily smell soon destroyed its sale, and several soap-makers were subjected to considerable loss. It is to be regretted that this attempt was not attended with success, particularly when we consider the great advantages which would result from the increase of our Greenland fishery, by the diminution of the importation of tallow from Russia*.

During the course of the last year, Sir John Dalrymple, Bart. one of the barons of exchequer, obtained a patent in the name of John Crooks, for a new method of making soap. He proposed to use the muscular fibre of fish in a certain proportion with tallow to make hard soap, and to substitute it for oil in the manufacture of soft soap†. As this scheme promised to be of the greatest national importance, it was taken into consideration by the House of Commons. Soon afterwards, Sir John Dalrymple was examined by a committee of the

owing to a method they employ of adulterating it, by burning along with the *Salsola soda* different species of fuci. In London, a barilla is made by mixing together Alicant barilla and kelp; and this is termed British barilla.

* The importation of tallow from Russia is stated to be about one million sterling yearly.

† The discovery of the conversion of the muscular fibre into a substance resembling spermaceti, has afforded room for considerable speculation, as to the probable advantages mankind may derive from it. The amazing number of animals killed in different countries, for the sake of their skin and tallow, has been viewed as likely to afford a means of procuring this substance at a cheap rate. A more plausible method of rendering this interesting discovery of use, may be found in employing fish in place of the muscular fibre of land animals. In Scotland particularly, this speculation deserves attention, as the quantity of fish which fills our sea at different times of the year is very great. Thus the herrings, which yearly arrive upon the coasts of Scotland, would afford a great source of animal matter for this purpose; also, the dog-fish, which are so obnoxious to fishers, might be taken in immense quantities; and after they had afforded their oil, might serve for the purpose of affording spermaceti. The coasts of Scotland are also happily constituted for this branch of industry, owing to the number of running waters, which we find almost in every quarter; these might be easily turned in different directions, and employed to run over a great quantity of animal matter. The advantages attending a successful cultivation of this discovery in the Highlands of Scotland, are numerous and important; but they cannot be detailed here. It is an object worthy the attention of the Highland Society of Scotland, which has already done much for the interests of that part of the island. If premiums were offered with this view, we should soon have the whole tried experimentally.

House

House, when he detailed the value of the supposed improvement in such strong terms, that an order was given by the Lords of Trade and Plantations to have the whole determined by a skilful manufacturer, in the neighbourhood of some fishing station; and, in compliance with this order, the Honourable the Board of Trustees, at Edinburgh, requested my father to make the necessary trials, and report for their satisfaction. These experiments were made as soon as circumstances would permit; and the result has been lately laid before the board. As this report contains a satisfactory series of experiments, I will now lay before the public a few of them, with the view of setting this supposed improvement in a proper point of view.

Previous to this, I shall mention, in as few words as possible, the processes followed by Sir John Dalrymple in making hard and soft soaps *.

To make Soft Soap. The fish are first to be well washed in water, to free them from blood, &c. and are then to be thrown into a boiling solution of caustic potash, in the proportion of from six to ten parts of fish to ten of alkali. The fish being dissolved, tallow is then to be added in the proportion of one part to eight of fish. These being boiled together for some time, turpentine is added in the proportion of one part to sixteen of soap; afterwards, a 36th part of palm-oil is added, to give the soap a better colour. The soap being finished, it is to be spread about two inches thick in a cool place, and kept there for a month before being used; during this time, it must be turned every two or three days with shovels or hoes.

To make Hard Soap. Having dissolved a certain quantity of fish in caustic vegetable alkali, an equal quantity of tallow is to be added, and to be continued boiling, until they are completely united. The vessel is then allowed to cool, and mineral alkali, or mineral and vegetable alkali, and rosin are to be added: the alkaline ley in the proportion of five parts to twenty of fish and tallow. The whole is then to be boiled, and in an hour after the strongest waste ley is to be added in the proportion of two parts in weight to 25 parts. The whole is then to be boiled until the alkali has united with the vegetable and animal matters. The ley is now to be pumped off, and caustic mineral alkali added, in the proportion of three parts in weight to 25 parts; and the whole is to be boiled, until a good soap is formed: lastly, the soap is to be melted in water, and cast into frames.

We need not stop to examine the processes here detailed, as they must appear, from what has been already stated with regard to soap-making, to be very incorrect.

As the report given to the Board of Trustees contains a long series of experiments, it would occupy too much room to insert them at full length; I shall, therefore, only abstract those which appear to be the most important.

Hard Soap from Fish, &c.

EXPERIMENT I.

					Cwt.	qrs.	lb.
Herrings	-	-	-	-	5	2	20
Tallow	-	-	-	-	5	3	8
Rosin	-	-	-	-	1	0	

* This is abridged from his patent.

These substances were boiled together so as to form a soap, and, after casting into frames, afforded 13cwt. 2qrs. of soap. According to the returns mentioned in a former part of the communication, the tallow and rosin alone should have afforded 11cwt. so that the fish appears only to add 2cwt. It might be supposed that this 2cwt. 2qrs. of fishy matter had combined with the tallow and alkali, forming part of the soap: it was otherwise, the greater part was of a black colour, and adhered to the under part of the soap, immediately above the waste leys.

EXPERIMENT II.

					Cwt.	qrs.	lb.
Herrings	-	-	-	-	8	0	0
Tallow	-	-	-	-	4	0	0
Rosin	-	-	-	-	1	0	0

These materials afforded 13cwt. 3qrs. 4lb. of soap: of this the tallow and rosin gives 9cwt. and the fish 4cwt. The greater part of the fishy matter was useless, being nearly in a gelatinous state.

Observation.

In the art of hard soap making, it is known, that the addition of sea-salt, or weak kelp ley, is necessary, to enable the soapy compound to separate and rise to the top of the waste ley. If we attempt to follow this practice with the combination of fish and tallow, instantly the fishy matter separates, and the tallow rises to the top in the form of light yellowish coloured soap. This fact shows us the impossibility of combining fish and tallow to make hard soap.

Soft Soap made from Fish, Tallow, &c.

EXPERIMENT I.

					lb.	oz.
Herrings	-	-	-	-	36	0
Tallow	-	-	-	-	4	11
Rosin	-	-	-	-	2	5½
Palm-oil	-	-	-	-	1	2¾

This gave 45lb. of soap of the consistence of treacle. According to calculation, the tallow, rosin, and palm-oil gives 18lb. 12oz. and the fish 27lb. The soap in this state is too soft for the market, so that it requires to be deprived of a portion of its moisture to reduce it to the state of common soft soap, when it weighs only 33lb. It was found a difficult operation to effect an union of the fish and tallow, as it required, not only a longer continuance of boiling, but also a greater proportion of alkali, than a simple combination of tallow and rosin. Even after careful boiling, if the alkali contained the smallest portion of sea-salt, the fish separated from the tallow: this is a circumstance of such a nature, as to render working in the great way very precarious.

Many other experiments of the same nature with the one now mentioned, are related in the report; but it will be sufficiently satisfactory, to give the following table of expences, which is drawn up upon an average of all the trials.

Expence

Expence of manufacturing a Ton of Sir John Dalrymple's Soft Soap.

		Cwt.	qrs.	lb.	oz.		£.	s.	d.
Fish	-	18	2	0	0	} Value	1	1	0
Tallow	-	2	0	10	0		5	17	0
Rosin	-	1	1	1	0		1	0	1 $\frac{3}{4}$
Palm-oil	-	0	2	0	3 $\frac{1}{2}$		1	11	1
Pot-ash	-	5	3	0	0		15	8	0
Lime	-	3	0	0	0		0	3	6
Expence of manufacturing	-	-	-	-	-		2	0	0
Firkins as packages	-	-	-	-	-		2	3	0
Duty	-	-	-	-	-		16	6	8
Profit	-	-	-	-	-		3	0	0
							<hr/>		
What the manufacturer can afford to sell a ton at	-	-	-	-	-		£.47	12	4 $\frac{1}{2}$
Common soft soap a ton	-	-	-	-	-		48	0	0

From this calculation, we find only a very trifling difference of expence between the fish soap, and common soft soap: and experience has shewn me, that 1cwt. of common soft soap is equally valuable with 1cwt. 2qrs. of fish soap. This article, therefore, can never be brought into the market.

Sir John Dalrymple having found that the methods proposed in the patent were not likely to be attended with success, has adopted an old practice of the Swedish peasantry, in forming a soft soap, without oil or tallow, by dissolving fish in caustic potash. In the report, we have a number of trials upon this combination, with a view to ascertain the expence of manufacturing, and the following list of expences is drawn up from these trials.

Expence of manufacturing a Ton of this Soap; and what the Manufacturer can afford to sell it at.

		T.	cwt.	qrs.	lb.		£.	s.	d.
Fish	-	1	10	0	0	} Value	4	0	0
Potash	-	0	6	0	0		16	16	0
Lime	-	0	3	2	0		0	3	6
Labour, fuel, &c.	-	-	-	-	-	-	1	0	0
Firkins as packages	-	-	-	-	-	-	2	3	0
Duty	-	-	-	-	-	-	16	16	8
Profit	-	-	-	-	-	-	2	0	0
							<hr/>		
							£.42	9	2

From the above statement it would appear that the fish soap would supersede the use of common soft soap. Experience however has shewn me that 1cwt. of common soft soap will clean nearly as much linen as 2cwt. of this fishy combination. We must therefore conclude, that none of the proposed methods will be in any economical point of view of much consequence.

Having thus stated the results of the experiments laid before the Board of Trustees by my father, I will conclude this paper, with some miscellaneous observations upon the combination of fish and potash; and the relation of a few experiments on other saponaceous compounds.

When caustic potash and fish are heated together, a decomposition of the animal matter soon commences, as is demonstrated by the separation of a quantity of ammonia. The quantity of ammonia separated or formed in this process is so great, that should the fishy compound prove of any use, it would be economical to collect the ammoniacal vapours in a chamber with muriatic acid gas, to form muriate of ammoniac. This ammonia is derived not only from the gelatine, but also from the pure fibrous matter, as I found by particular trials with it. This fish soap has a more or less brown colour, varies in its consistence, which depends on the quantity of water it contains; it has a most disagreeable smell, and by keeping in a damp place becomes softer, and acquires a thin whitish covering. The bony matter of the fish is not dissolved, but remains suspended in the soap; so that when the soap is dissolved in water, the bony matter falls down. When an acid is added, the fishy matter is separated of a white colour, and remains diffused through the water, giving it a milky appearance.

Sir John Dalrymple mentions in his printed letters, that from repeated experience, he has discovered this soap to be possessed of peculiar properties, which he says, independent of its cheapness, will render it an object of great national concern. The following experiments were made to try the truth of Sir John Dalrymple's statements, as also to observe if this compound had similar properties with common soap.

1. With distilled water it forms a brown-coloured solution, which being kept for seven or eight weeks, did not alter in its appearance or smell. It also lathered pretty well.

2. With soft water it forms a brown-coloured solution, and lathers well.

3. With hard water it is changed in the same manner as common soap, a part of the saponaceous compound being decomposed by the earthy salts, which are generally to be found in such kinds of water. Sir John Dalrymple, however, asserts, that it washes equally well in cold hard, as in cold soft water, and besides, that it washes with equal advantage although the water be not heated. The experiment which has been just mentioned, shews that it does not differ from common soap when used with hard water—that it washes equally well in cold as in warm water, does not require refutation.

4. If this soap be agitated in cold sea water, a part of it is decomposed by the earthy salts, contained in the water, and the water acquires a milky appearance. This decomposition is rendered the more evident, if the solution be kept for some time, for it then acquires a most disagreeable smell, owing to the putrefaction of the animal matter, which had been separated by means of the earthy salts. Sir John Dalrymple affirms, that this soap washes well in cold sea water, which he justly observes, is a discovery of great importance. The milky appearance, and imperfect lathering which this soap exhibits when agitated with cold sea water, has probably led Sir John Dalrymple to draw this conclusion. These appearances, however, are no proof of its possessing any greater cleansing power than common soap with salt water.

Accordingly

Accordingly my father found that this assertion of Sir John Dalrymple, was without foundation.

In the 6th volume of the transactions of the London Society of Arts, mention is made of a soap, manufactured in India, which is said to wash with sea water. It would be very interesting to have the truth of this ascertained.

5. Chaptal, in the 21st volume of the *Annales de Chimie*, has described a method of making soap from wool, and he remarks towards the conclusion of his paper "This soap has been employed in every manner, and under every form, in my manufactory for dyeing cottons; and I am at present convinced that it may be substituted instead of the saponaceous liquor we make from lixivium of soda and oil, to prepare the cottons. I have constantly observed, that by dissolving a sufficient quantity of this soap in cold water to render the fluid milky, and by working the cotton with the apparatus, which is well known, it is sufficient to pass the cotton three times through, drying it each time, in order that it may be as well disposed to receive the dye, as that which has been passed seven times through the ordinary solution of soap. This will not appear surprising, when it is considered that animal matters are very proper to dispose thread and cotton to receive the dye, and some of the operations of our dye works consist simply in impregnating them with these substances *." In several trials which I have made with this fish soap, I find that it gives a greyish or brownish tinge to cotton cloth, which remains after repeated washing with common soft soap. If this be found to hold true in other trials, it is not improbable that this fish soap may be used in place of wool soap, in the processes mentioned by Chaptal. Several preliminary trials must be made, to determine whether the Gallic acid decomposes this compound, and if the astringent principle will combine with the fishy matter; at least, this is necessary in the process for dyeing cotton red. (See Chaptal's paper, *Annales de Chimie*, vol. 26th.)

Wool Soap with Tallow.

Having observed from the preceding experiments, that the muscular fibre of fish does not unite with tallow in making hard soap, I imagined that other animal substances might possess the same property. Accordingly I made several experiments, with the view of uniting the woolly matter with tallow.

Experiment 1. A quantity of white woollen cloth was dissolved in caustic potash, then a considerable portion of tallow was added, and the whole boiled (with a fresh addition of ley) until a brownish-coloured soap was formed.

2. A quantity of wool soap which had been made about a year before, and still retained a disagreeable smell, was boiled with tallow, and formed a soap.

3. A quantity of wool soap was boiled with whale oil, and afforded a soap.

4. A quantity of soap made with wool and tallow was boiled with water; sea salt was then added, and the boiling continued for some time; the tallow soon separated, and rose to the

* *Philos. Journal*, I. 43.

top, in the form of white soap : below in the waste ley was the brown-coloured woolly matter. This experiment shews the impossibility of combining woolly matter and tallow to form hard soap. I next endeavoured to combine glue with tallow, and to form a hard soap.

Glue Soap.

I took a quantity of common joiner's glue, and boiled it with caustic potash, until it was dissolved ; during the boiling, a considerable quantity of ammonia was separated, which gave the combination a pretty dark colour. This compound was again boiled with a fresh portion of caustic potash, along with a quantity of tallow ; and the boiling was continued until, upon cooling, a fine yellow-coloured soft soap was formed. This soap had a very strong smell, somewhat like putrefying fish. When it was boiled with sea salt, to form a hard soap, a decomposition was immediately effected ; the tallow rose to the top, in the form of white soap, and below in the waste ley was the glue of a brown colour and curdled.

Peat Soap.

Having found from the preceding trials, that neither the muscular fibre, glue, or wool, would remain united with tallow in the common process of hard soap making ; it occurred to me, that different vegetable substances might be useful. Accordingly I endeavoured to combine peat and tallow together, so as to form a hard soap. I dissolved a quantity of peat in caustic potash, and continued the boiling until the compound had acquired the consistence of common soft soap. In this state it was soluble in water, and lathered with it. With tallow it formed a brown-coloured soap, which was not decomposed by boiling with sea salt. I was in hopes, from this circumstance, that it might enable us to introduce an useful manufactory into the Highlands of Scotland, where peat is to be got in great quantity. I found, however, that it gave a brown colour to cloth which was washed with it, so that my attempt at rendering it useful was frustrated.

Preservation of Animal Substances by Alkaline Solution.

Sir John Dalrymple, in his examination before the House of Commons, claimed, as a discovery of the greatest importance, the method of preserving fish by immersing them in a solution of alkali. This is somewhat surprising, for we find this fact mentioned long ago, and in different publications. Thus, in one of the early volumes of the Transactions of the Royal Society of London, several experiments are related, on the power of lime water in preserving animal substances, and it is remarked, that fish immersed in lime water, were preserved fresh for a considerable time. Sir John Pringle also made several experiments on the antiseptic power of caustic alkalies ; and more lately, in one of the volumes of the Transactions of the Royal Irish Academy, there are series of experiments on the antiseptic power of caustic alkalies.

*Sheriff Bræ, Leith,
May 11th, 1799.*

ROBERT JAMESON, F.L.S. &c.

IV.

Experiments and Observations to prove that Snow does not contain Oxygen, either in Solution or in Combination, and that its fertilizing Quality does not depend on this Cause. By DR. JOACHIN CARRADORI DE PRATO.*

THE generally-received opinion, that snow produces fertility, is true, because it is proved by experience ; but I do not think it can be admitted in the sense which is commonly received. I am well convinced that snow produces this effect, but I conceive, as many others have also done, that it acts in a negative manner only, by defending the plants in winter from a degree of cold below that of freezing, and not, as is commonly thought, from its imparting any principle of fertility. But Citizen Hassenfratz at present thinks he has confirmed this last opinion, by a discovery of the cause of the phenomenon, which he thinks must be ascribed to a quantity of oxygen in combination in the snow, which it afterwards communicates to the seeds which are developed when it is converted into water, and he undertakes to prove by several experiments, that snow water contains much oxygen in the state of combination †.

I shall prove by incontestable facts, that snow water does not contain pure oxygen in the state of solution or aggregate, nor in the state of fixity or combination ; and consequently that the snow cannot impart fertility by virtue of this principle.

In the month of August of the present year, I took the purest snow I could find, and after having washed it, I filled a small glass bottle with a long neck, and when it began to dissolve, I covered it with very pure oil olive, in order that it might not absorb any air from the atmosphere. About sixteen hours afterwards, I first removed the whole of the oil from its surface, then threw in a small fish, and immediately afterwards covered the water with fresh oil. When the fish entered the water it began to struggle, and died almost instantly. I threw in another with the same precautions, and it also died. Lastly, I took a portion of water of the same snow, which had been kept for the same time exposed to the air, in a receiver with a large mouth, and poured it into a small glass bottle equal to the former. Into this I threw a fish of the same size as before, and immediately covered the water with oil olive ; but in this water the fish gave no signs of uneasiness, and lived quietly for more than three quarters of an hour. The thermometer stood at nineteen degrees of Reaumur during these experiments, and the barometer at about 27 inches and a half.

As I have elsewhere proved ‡ that fish by their respiration in water, have the faculty of absorbing all the oxygen it contains, and that they immediately die in water which is entirely deprived of oxygen : I inferred from these experiments, that snow water does not contain oxygen in a state of solution.

* Journal de Physique, V. 226.

† Journal Politechnique, IV. Cahier ; of which some account is given in our Journal, I. 144.

‡ Annales de Chimie, et Hist. Nat. de Pavia, tome 5 et 14.

In order to confirm my deduction, I immediately poured the water, in which the fishes had died, into a receiver, which presented a large surface to the air, and, a few instants afterwards, I threw in a fish of the same species, which lived very well in it, and might have remained as long as I chose. It is, then, an incontestable truth, that the mere want of oxygen was the cause of the death of the two fish thrown into the snow water in the first experiment; for it is very evident, that when the snow water was placed in a situation to re-absorb the oxygen, of which it had been deprived by its congelation, it became as capable of maintaining the life of fishes as any other water.

But it seems certain, from several experiments, that snow water re-absorbs oxygen from the atmosphere more slowly than other waters which were deprived of it. I have before remarked, that snow water after having been exposed during sixteen hours to the air, contained so little oxygen, that it could scarcely support the respiration of a small fish for an hour; whereas I knew from other trials, that waters commonly contain a sufficient quantity of oxygen for the respiration of a small fish in the same circumstances for several hours. It seems proper, therefore, to conclude, that the water by its conversion into snow loses part of its disposition to absorb the oxygen of the atmosphere. But in order to determine this more accurately, I chose to make some trials.

I exhausted all the oxygen contained in two pounds of well water, by the respiration of a fish which was kept in the water included in a narrow-necked bottle until it died. When the fish was dead, I cleared the bottle very exactly of the whole of the oil, and poured the water into a vessel with a large aperture, in which I kept it exposed to the air for sixteen hours. I then returned the water into the same bottle, introduced a fish of the same size and kind, and immediately covered it with oil. At the end of four hours the fish was still alive, but, after half an hour longer, it died in convulsions, in the same manner as fishes ordinarily die for want of air. Well water, therefore, though totally deprived of oxygen like snow water, that is to say, to the point of being incapable of maintaining the respiration of fishes during the same time, did re-absorb more oxygen than snow water.

This cannot be attributed to the quantity of foreign matters usually contained in snow water, on the supposition that they either hinder the water from re-absorbing oxygen, or the fish from extracting it by respiration; for I have found that fishes live for several hours in turbid water exposed to the same circumstances as the snow water: and I have also proved that this water, after having been deprived of oxygen, re-absorbs it in less time than snow water.

Nevertheless, snow water, after a long interval of time, becomes again charged with all the oxygen it can contain, and becomes capable of maintaining the respiration of fish, like every other water. In the month of September, I kept clear snow water for five days in a bottle; and in order that the oxygen might insinuate itself more easily, I first filtered it through a paper, and afterwards agitated it every day in the receiver. This water so treated, absorbed as much oxygen as it was capable of holding; for when it was introduced into a small bottle with a narrow neck, it supported the life of a fish for nine hours, which was introduced and the water covered with oil.

But

But snow water does not even contain oxygen in a combined state, as Hassenfratz pretends. I have proved this by the most decided experiments. If melted snow be oxygenated water, containing oxygen in the state of combination, it will follow that when exposed to the sun it will emit the oxygen again in the gaseous form, by the combination of caloric and light, precisely in the same manner as it escapes and shews itself when the nitric or oxygenated muriatic acids are exposed to the solar light. But certainly since snow water does not contain oxygen in solution, but can absorb it successively as often as it is exposed to the atmosphere, it must at least, if this principle do exist in snow, contain it in the aggregate state. I therefore, in the month of September, in the present year, put very pure snow in small pieces into a small glass bottle with a narrow neck, and before it was entirely melted, I poured oil upon its surface, in order to prevent the communication with the air, and afterwards exposed it for three successive days to the sun; so that the whole time of exposure amounted to eighteen hours; and afterwards, having well cleared it of the oil, I introduced a small fish: it died instantly, as in the snow water immediately after liquefaction. And notwithstanding the long exposure to the sun, which rendered it very perceptibly warm, though I observed it with every possible attention, I never perceived the smallest bubble or indication of oxygen. Consequently it cannot be admitted that this water was charged with oxygen in combination, because it must have escaped in the form of gas; and if it had afterwards absorbed it, the fish, which is the surest indication of the smallest quantity of oxygen in water, would have availed itself of it, and lived for some time.

The experiments of Hassenfratz, which seem to prove by analysis the presence of combined oxygen in snow water, are not, in my apprehension, conclusive, because they are not confirmed by synthesis. It is not an exclusive property of water saturated with pure oxygen in a combined state to alter the tincture of turnsol to a red colour, and to precipitate the solution of sulphate of iron.

It has already been observed by Bergman, in his analysis of waters, that snow recently liquefied is absolutely without air; but his assertion is not well proved, because the method he made use of to ascertain the existence of vital air, or oxygen in water, namely, boiling, is absolutely imperfect. Water retains its oxygen too strongly, to allow its being extracted by boiling, as this experiment would require. I filled a small matras with well water, and boiled it for an hour and a half, and upon taking it from the fire, I poured it out beneath oil, in order that no air might enter. When it was cold, I took off the oil and put in a fish, immediately after which I covered it again with oil-olive. Notwithstanding the boiling, the fish found a sufficient quantity of oxygen in this water to enable him to live more than three hours.

Fresh water fish are true eudiometers for water, for when they respire, they have the faculty of absorbing all the oxygen it contains. From this property, natural philosophers may have recourse to them for measuring the quantity of oxygen contained in different waters, by taking fish of the same species and size, and observing the time they live in equal quantities of the water. In this way a series of experiments might be instituted, to determine the true proportions of oxygen contained in different waters; as has been done by Fontana, though by the uncertain method of boiling. But it will be necessary to take into considera-

tion the variations of the barometer and thermometer, because I have observed that water contains less oxygen in summer than in winter; and when the barometer is high (qu. low?) than when low, the reason of which is sufficiently evident.

Bergman affirms, that snow affords an indication of nitrous acid. If this be true, it accounts for its effect in burning leather, and other substances plunged therein; as Hassenfratz has remarked, who ascribes it to the combined oxygen, with which he thinks it is saturated. Is it probable that this acid should be formed at the moment of the congelation of the water, by the concentration or mechanical approach of the parts, caused by the cold in the oxygen gas, with which the water is charged, and the azotic gas which may be afforded by the atmosphere?

Let this be as it may, a question will present itself concerning this commonly supposed activity of snow, namely, how it can, if reduced according to the assertion of Hassenfratz, into water possessing the same qualities, prove beneficial for developing the tender embryos of vegetables?

It seems, therefore, that from the whole consideration of the subject, we are justified in concluding, that there is no reason to believe that snow communicates any positive fertility to the earth; its good effects must be attributed merely to the simple preservation of plants from intense cold, which, by altering their organization, would destroy the powers of life.

V.

On certain Properties of Strontian and Barytes. By Citizen VAUQUELIN.

SINCE the method of obtaining barytes and strontian in a state of perfect purity has been discovered by chemists, they have observed several properties in these earths, analogous to those of the alkalis, such as the acrid burning taste, solubility in water, and separation in crystals, together with the change which they produce in the blue colours of vegetables, which they convert into green.

I shall, on the present occasion, exhibit some other of their properties, which I apprehend may serve to shew their still more striking resemblance to the alkaline substances.

About a year and a half ago, upon analyzing a siliceous sulphate of barytes, I found, after having decomposed it with charcoal, that the greatest part of the filix had been dissolved by the acid I made use of to decompose the sulphate which had been formed.

I could not immediately determine the cause of this unexpected event, but merely supposed the barytes to have produced it.

I have since made some more direct experiments, to elucidate this question, the result of which shews that my supposition was not without foundation.

Experiments on Strontian.

Experiment 1. Two hundred parts of strontian in fine powder, mixed with sixty parts of filix also powdered, were subjected for an hour to a strong fire in a crucible of platina; the

the product was a grey sonorous mass, cracked in various places, the parts of which adhered together with considerable force. In this state, it had no very evident taste, but when pulverized it was slightly caustic; when put into water, whether in the lump or in powder, this compound neither produced heat nor swelled up, as is the case with the pure strontian. It only became somewhat whiter.

Experiment 2. The substance last spoken of was pulverized and boiled in water, which dissolved it much less abundantly than pure strontian, but the water acquired a slight alkaline taste, and soon became covered with a white pellicle: it did not afford crystals. When saturated with nitric acid, this solution afforded, by evaporation, a jelly in considerable abundance.

Experiment 3. Another portion of the same matter pulverized and moistened with a little water, was totally dissolved, by the muriatic acid, and the solution afforded by evaporation a very abundant jelly, which, after washing and drying, presented all the characters of silix. The nitric and acetous acids produced the same effect on this substance.

Experiment 4. Five parts of pure strontian in fine powder, and one part of alumine, recently prepared, and still humid, were heated together with water, and when the liquor had fully boiled, it was filtered, and contained much undissolved matter. The filtered liquor had a slight alkaline taste, but did not crystalize, though a much greater quantity of the earth had been used, than could be dissolved without heat.

This solution was then saturated with muriatic acid, and being afterwards mixed with ammoniac, afforded a small quantity of flocculent matter, which was alumine.

Strontian consequently possesses the property of favouring the solution of alumine in water; but what is more remarkable is, that the alumine, on the other hand, rendered a great quantity of the strontian insoluble; for the water did not, in this case, dissolve one-tenth part of what it would else have taken up.

The examination of what remained on the filter, proved that the strontian became insoluble, by an intimate combination between these two earths; and it is probable, that if a greater quantity of alumine had been present, there would not have been a particle of mere strontian dissolved.

The residue was, in fact, soluble in acids, with scarcely any effervescence; its solution afforded a flocculent precipitate of alumine, by means of ammoniac; and the supernatant liquor formed a very abundant deposition, when carbonate of potash was added.

It will not, therefore, be surprising, if a combination of these two earths should, hereafter, be found in nature.

Concerning Barytes.

Experiment 1. One hundred and fifty parts of caustic barytes were mixed, as accurately as possible, with fifty parts of silix, and the whole was afterwards strongly heated for an hour and a half in a crucible of platina. The mass was in one single piece, cracked in various places, but without the cohesion observed in the experiment with strontian. Its colour was a light apple green, its taste almost mild, and it did not heat with water, whether it was immersed in lumps, or in powder. It preserved its green colour in the water.

Experiment 2. The pure nitric, muriatic, and acetic acids totally dissolve this substance. Its solution, in any one of these acids, immediately affords a flocculent precipitate by ammoniac. These solutions become gelatinous by evaporation, and when completely dried, the silica is again developed in possession of all its properties. It cannot, therefore, be doubted, but that barytes, as well as strontian, possesses the property of combining with silica, and of rendering it soluble, even in the weakest acids.

Experiment 3. Nine parts of caustic barytes were mixed with one part of alumina, newly separated from its solvent, and still moist; and the whole was subjected for a quarter of an hour to ebullition, with a sufficient quantity of water. Much undissolved matter remained. The filtered liquor had a slightly caustic taste, rather stronger than that of strontian treated in the same manner. It soon became covered with a white crust by the absorption of carbonic acid from the atmosphere; but it did not crystallize, though the quantity of water was not sufficient to have prevented that effect, if the alumina had not been present.

A drop of muriatic acid poured into a glass of this solution, produced a flocculent cloud, which was redissolved by agitation. A second and third drop produced the same effect, until the greatest part of the barytes was saturated; but at length the precipitate being no longer dissolved by the motion impressed on the fluid, an excess of acid dissolved it. When the fluid was entirely saturated with acid, the flocculent matter was again reproduced by ammoniac; and when this last addition ceased to produce any effect, a very abundant precipitate was afforded by the carbonate of potash.

We see, therefore, that barytes, as well as strontian, dissolves alumina, and even more abundantly; nevertheless, there still remained in the residue a portion of alumina and barytes, which were not dissolved, and appeared to be in a state of intimate combination.

Suspecting that this more abundant solution of alumina by barytes, than by strontian, might be owing to a greater quantity of barytes having been used in this experiment than of strontian in the other; I made a second, in which I mixed equal parts of barytes and alumina, and boiled them as before. The liquor still gave signs of an abundant solution of alumina and barytes, and about half the matter remained in the form of a white powder insoluble in water, in which the acids demonstrated the presence of these two earths in combination.

The same effects take place between strontian and alumina, and it is not surprising that this property extends itself to the alkalis; for when alumina is precipitated by potash, and rather too much of this substance is added, the earth always retains some traces of the alkali, however carefully it may be washed. Besides which, we have examples enough of glass which becomes soluble, or insoluble, according to the quantity of alkali; and a still more striking instance is afforded of potash in hard stones, which cannot be separated by any mechanical means.

The truth of what is here advanced, may be very simply shewn, by pouring a saturated hot solution of barytes into a solution of the muriate of alumina. A precipitate in flakes will first be found, which will be redissolved by a new quantity of the solution of barytes; and if before this precipitate be entirely redissolved, it be separated from the fluid, it will be found to be composed of alumina and barytes.

Barytes and Oil.

Experiment 4. It is known that when a solution of barytes or strontian is poured into a solution of common soap, a very abundant deposition is formed, which is a combination of the earth with the oil of the soap, while the alkali remains pure and caustic in the fluid. In order to ascertain whether these earths could unite directly with oil, I boiled a solution of barytes, made with heat, together with oil olive; the oil soon became consistent, the water lost its alkaline taste, and the new combination exhibited the taste and smell of a true soap, from which it differed merely in its want of solubility in water.

Barytes and Animal Matter.

Experiment 5. I was desirous of knowing whether barytes would act upon animal matters, in the same manner as the alkalies, which last are known to decompose them, and form a kind of soap, while ammoniac is disengaged. I therefore mixed twenty drams of bullocks' liver pounded with ten drams of barytes diffused in water, and boiled the whole in a retort, to which a receiver was applied. In a very short time, I obtained a large quantity of ammoniac, and the animal substance was converted into a sort of clotted magma of a rose colour, insoluble in water, and which, by several experiments, I ascertained to be a combination of fatty matter and barytes. It is, therefore, proved that the alkaline earths exert the same action as the alkalies upon animal matters, though with a slight difference in the result.

Conclusion.

It is evident from the preceding facts; 1. That strontian and barytes possess nearly the same habitudes as the alkalies, with regard to silica and alumina, like which substances they combine with the earths, and divide them in such manner, as to render them afterwards soluble in the weakest acids. 2. That they may be used like the alkalies, in the analysis of hard stones, which are not attacked by acids; a property which may be advantageously applied in such cases as do not allow the exhibition of alkalies. 3. That it is necessary to be careful in decomposing the nitrates of barytes and strontian in crucibles of earth, in order to obtain those bases in a state of purity, not to apply too strong a heat, for fear they should combine with silica and alumina. 4. That it was, no doubt, owing to combination with the earth of the crucibles, that barytes was formerly obtained in the state of a frit insoluble in water, and that the powder of charcoal, at present used in expelling the carbonic acid, has no other effect than that of removing the earth from the contact of the crucible. 5. That barytes and strontian ought henceforth to be separated from the class of earths, and inserted in that of alkalies, with which they have many more properties in common. This has already been done by Citizen Fourcroy, in his new work. 6. That it is very probable, we shall hereafter find combinations of barytes and strontian with silica, and perhaps with alumina.

Concerning

VI.

Concerning those Perpetual Motions which are producible in Machines, by the Rise and Fall of the Barometer or the Thermometrical Variations in the Dimensions of Bodies.—(W. N.)

IN a former communication, I have given an account of some of the delusive projects for obtaining a perpetual motion, from an invariable power *. In that paper I remarked that the flow of rivers, the vicissitudes of tides, the variations of winds, the thermometrical expansions of solids and fluids, the rise and fall of the mercury in the barometer, the hygrometric changes in organized remains, and every other of those mutations, which never fail to take place around us, may be applied as first movers to mills, clocks, and other engines, and keep them going till worn out. Many instances of this kind of perpetual motion are seen in water-mills, and other common engines, which are necessarily confined to certain local situations.—The wind-mill, though less confined with respect to place, is the subject of a much more variable power; other instruments, still less confined with regard to situation and exposure, have been made, which are capable of continuing their motion without ceasing. Such was the clock, or perpetual motion, in Cox's museum, which was shewn about twenty years ago in London. My former paper was written to shew the value of the perpetual motion, strictly so called, which has for the most part been pursued by men of little information. In the present memoir, I shall endeavour to ascertain that of this second kind of motion, which, because more promising, and of nearly the same apparent practical value, has been followed at some expence by men of higher claims. For this purpose, I shall first describe a few schemes, and then investigate the quantity of power they are likely to afford.

Fig. 1, Plate 6, is a sketch of the first mover in a clock, which formed part of Cox's museum, which was sold by public lottery, about the year 1776, if my recollection be accurate. A B represents the surface of the mercury in a barometer, the glass vessel of which had the form of a bottle or chemical matrafs. The diameter of the upper surface of the mercury was, I think, about twelve inches. C D represents the basin or receptacle, into which the aperture of A B was plunged. I suppose, of course, that the lower surface of mercury, which was exposed to the pressure of the atmosphere, was nearly the same as the upper A B, as in fact it appeared to be. From the intervention of the case, and other parts of the apparatus, I could only conjecture the manner in which the effect was produced; but this was afterwards explained to me by Mr. Rehe †, who contrived and made it. The basin C D is suspended by two chains K L, which pass over the pulleys or wheels H I, and are attached to the frame E F; which last is fixed to the barometer A B. Let us now suppose the apparatus to be at liberty, and it will be clearly seen, that if the two masses attached to the opposite ends of the chains K L be not precisely equal, the heaviest will descend, and cause the lightest to rise.—The masses must, therefore, be brought nearly to this state of equality, by the adjustment of weight added to one or both of them. In this state, suppose the pressure of the atmosphere

* *Philos. Journal*, I. 375.

† This gentleman is at present one of the board of inspection of naval works at the Admiralty.

to increase, and the consequence will be, that a portion of the mercury being forced from the vessel C D into A B, will render this last heavier, and cause it to descend ; while C D at the same time rises. And on the other hand, when, by a diminished pressure of the external air, the mercury subsides in A B, the vessel C D will preponderate, and A B will rise. Now the frame E F, which is interposed between the barometer and the pulleys I K, is jointed at the corners and also at the places where it is attached to the chain and the barometer ; and the inner edges of the upright pieces E, F are formed into teeth like those of a saw, the slopes of which lie in opposite directions, as is shewn in the figure. The wheel G, which is placed between these bars, is also toothed in the same manner ; and its diameter is such, that when the teeth on one side, as for example E, are engaged, those on the other side, F, may be free ; but it is too large to admit of both sides being disengaged at once. The wheel G is prevented by a click from moving in the direction opposite to that which may be produced by the action of the bars E and F. Hence the play of the machine is evident. When the pressure of the atmosphere diminishes and the barometer rises from its cistern, the side E of the frame will move the wheel G through a greater or less space, according to the variation ; and when, on the contrary, it falls, the teeth E will be drawn out of their bearing, and those of F will be thrown into the wheel, and still produce a motion of the same kind ; the joints of the frame E F allowing it to change its figure enough for this purpose. It is hardly necessary to remark, that this wheel G being connected with the clock, serves to wind it up, and that the clock is constructed to go for a much greater number of days than the barometer has ever been known to remain stationary.

The ingenious mechanic will readily form a notion of many other methods, of applying the variations of the barometer to similar objects. The wheel-barometer of Robert Hook, as well as another contrivance, in which the barometer and its cistern are placed at the different extremities of an inclined lever, may likewise be used for this purpose.

Several artists have exerted their industry, in attempts to apply the variations produced by change of temperature in bodies as a first mover. If a thermometer be suspended by its centre of gravity in such a manner, that the tube may lie nearly horizontal ; the daily variations in the bulk of the mercury will cause a preponderance on the one side or the other, accordingly as the temperature is higher or lower, than it was at the original fixing of the centre of suspension. The thermometer may contain mercury or any other fluid, or it may consist of air confined by mercury, as in the manometer. In this contrivance, the great and frequent ranges of variation affords much promise of utility. The limits of convenient or practicable power from change of equilibrium in a fluid thermometer, will hereafter be examined. A much greater force seems to offer itself, in the power by which the expansion is produced ; but the difficulty of forming a piston or other apparatus for confining fluids, will probably constitute an insurmountable impediment to this method.

The solid thermometer does not present the same difficulty. Fig. 2, represents a series of expansion-bars, each consisting of a plate of brass, soldered to another of steel, and possessing the property of bending by change of temperature, according to the laws already explained in this
work.

work *. If the steel face of C A be uppermost, and the end C be fixed to C B, the extremity A will rise from B, when the temperature is elevated; and if the succeeding bars be similarly fixed above each other, as in the figure, the whole system will occupy a greater length, or elevation, above C B, when heated, than when cold. Another more convenient method of disposing the bars is shewn in fig. 3; in this, the bars are fixed together at the middle, with the brass faces turned towards each other. Each bar has a slight curvature (much less than is here shewn), which will be increased by heat, and by that means cause the distance between the middle of two extreme bars to be greater than it would be at a lower temperature.

These causes of action may be applied to machinery by various contrivances, some of which serve to increase the length of range, but add nothing to the power. This last, no doubt, is an object of convenience, according to the effect intended to be produced. The only method of adding to the power will consist in increasing the number of the bars. Fig. 4, represents a system for this purpose, which is the simplest and most convenient that has occurred to me. A C represents the circumference of a barrel, resembling those in which the main springs of clocks are put; the length and diameter of which may be varied, according to the power intended to be gained. To this external part is fixed a ratchet-wheel to receive the click C, which confines its motion to one direction. At A is fixed a plate to receive the action of the expansion-pieces. B D is an internal cylinder of the same kind, which is also confined by a ratchet-wheel and click to move only in the same direction as the outer part A C. It is not necessary to describe the operative arrangements, by which these two cylinders are disposed, so as to move on the same axis, and the ends duly applied, so as to form one box; while the interior and exterior parts allowed to move independent of each other. At B is fixed a plate, by which the action of the expansion-pieces is communicated to the inner cylinder. A series of bars, similar to those delineated in fig. 3, are disposed in the space between the two cylinders, the greatest part of which they occupy, leaving only such an interval between A and B, as may be sufficient to allow for the motion of the bars. In this interval is placed a spring, tending to cause A and B to recede from each other: and lastly, there are side-pins proceeding from the places of junction of every pair of bars, which respectively pass through circular grooves in the caps, and prevent the motion of the bars from being interrupted or impeded by their touching either the inner or the outer cylinders. E represents a wheel, which is supposed to be connected by tooth-work, or otherwise, with the face of the external cylinder, and may be considered as the machinery intended to be moved. Or otherwise, if the clicks C and D, with the teeth they act upon, be reversed, and the interior cylinder be fixed to the axis itself, that axis may be used as the first mover.

* Philof. Journal, I. 62. 576.

(To be concluded in our next.)

VII.

Chemical Considerations on the Use of the Oxydes of Iron in the dying of Cotton. By J. A. CHAPTAL.

(Concluded from page 93.)

2. **I**F the iron be precipitated from a solution rather strong by alkaline liquor, marking between five and six degrees of the areometer of Baumé, the product will be a bluish green magma. Cotton macerated in this precipitate acquires at first a dirty irregular green tinge; but simple exposure to the air converts it to a yellow, in a very short time, of a very deep shade:

It is by a process nearly similar to this, that the ochre or rust colour of the shops is formed.

But these colours are attended with various inconveniences. 1. They either corrode the cloth or injure its durability. 2. This colour is harsh, disagreeable, and cannot easily be combined with the soft colours afforded by vegetables.

I was desirous of remedying these inconveniences, and succeeded by the following treatment:

I work the cotton in a cold solution, marking three degrees (specific gravity 1.02), then carefully wring it with the pin; and afterwards plunge it in a solution of potash at two degrees (1.015), into which sulphate of alumine has been poured to saturation: By this means the colour is brightened, and becomes infinitely finer, softer, and more agreeable;—the sulphate does not attack the body of the stuff;—and after having left the cotton in the (second) bath for four or five hours, it is taken out to be pressed, washed, and dried:

By this process, every shade which can be desired may be obtained by graduating the strength of the solutions. The samples which I present to the institute are prepared according to this method. This simple process, of which the theory will present itself to the mind of every chemist, possesses the advantage of affording a very agreeable, very solid, and particularly cheap, colour. I have used it with advantage for nankeens, of which the colour is infinitely more fixed than that of the English nankeens. It has the advantage over them in resisting alkalies, and the only fault I know in it is that of acquiring a brown colour by the action of astringents.

I was for some time of opinion, that it might be possible to combine this yellow with the blue of indigo, to obtain a solid colour. But hitherto my expectations have not been realized, and the results of the trials I have made are, that there is not a sufficient affinity between the blue of indigo and the oxydes of iron. I obtained only a dirty, earthy, very deep, and unequal green colour.

The oxyde of iron, on the contrary, combines very easily with the red of madder, and affords a light violet prune colour of very extensive and advantageous use in cotton manufactories.

But if these two colours be applied to cotton without employing a mordant capable of fixing the latter, the colour will not only remain dull and disagreeable, from the impossibility of brightening it, but it will likewise have the very great inconvenience of not resisting alkalies. It is proper, therefore, to begin the operation in the same manner as when cottons are prepared to receive the Adrianople red; and when the goods are brought as far as to the operation of galling, they are to be passed into a solution of iron, more or less charged, accord-

ing to the nature of the violet which may be desired. The cotton must be carefully washed and maddered twice in succession, after which it is brightened in a bath of soap.

When a true violet of a soft and firm colour is required, the solution of iron is not to be used till after the galling. The iron is then precipitated in a bluish oxyde, which combining with the red of the madder affords a superb violet, more or less deep according to the strength of the galling and of the martial solution.

It is very difficult to obtain an uniform colour by this process; and a very even violet is considered in the manufactories as a master-piece of the art. It is generally imagined, that this important problem of the art of dying can only be solved by well-conducted manipulations. But I am well convinced that the great cause of the unevenness of this dye is, that the iron deposited upon the cotton receives an oxydation by mere exposure to the air, which varies in the different parts of the cotton. The threads which are at the outside of the mass become strongly oxyded, while the interior part, being defended from the action of the air, undergoes no change. Whence it follows, that the inner part acquires a faint shade, while the outer surface presents a violet almost black. The only means of remedying this inconvenience, is to wash the cotton on taking it out of the solution of iron, and to madder it while wet. The cotton is thus rendered more even and velvety.

The solvents for iron are nearly the same for this colour, as for the yellow of which we have already treated.

I suppress all observation on the manipulations, in order to confine myself to mere chemical report; and on this consideration, I shall add an observation to direct the artist in the brightening of violet upon cotton.

The red of madder, and the oxyde of iron, being deposited on the stuff, determine the violet colour. This colour inclines to red or blue, accordingly as the one or the other of the two principles predominates. The dyer knows by experience, how difficult it is to obtain a combination which shall produce any desired tone or colour; particularly, when it is required to be uniform, bright, and solid. It may, nevertheless, be effected, not only by varying the proportions of the two principles, but also by varying the process of brightening. It is only necessary to know the following facts: namely, that soda destroys the iron, while soap by strong boiling attacks the red of the madder in preference. Hence the dye may be made to incline to red or blue, accordingly as the brightening is effected with one or the other of these mordants. Thus cotton taken out of the madder, washed, and boiled with 30,00 parts of soap, will afford a superb violet; though, if soda had been used, it would have turned out merely a prune colour.

The oxyde of iron precipitated upon piece-goods, unites still more advantageously with the yellow afforded by astringents; and an infinite number of shades may be obtained by varying the force of the mordants. In this case, it is less a combination, or solution, of principles, than a simple mixture, or juxtaposition of the colouring matters upon the stuff. The oxyde of iron may be more intimately combined with the astringent principle by a boiling heat, which brings it to the state of black oxyde, as our colleague Berthollet has observed.

It is also possible to render these same colours brown, and give them a variety of tinges,
from

from light grey to deep black ; simply by passing the cottons, impregnated with the astringent principle, into a solution of iron. The oxyde is then precipitated by the principle which is fixed in the stuff.

It is an observation which may become of high value in the art of dying, that the astringent vegetables most commonly used afford a colour which is not very bright, but of high value for its durability. This yellow colour is brightened in the series of vegetables, in proportion as the astringent principle is less in quantity, and the liveliness of the colour is also increased in the same proportion. It is, therefore, difficult to obtain yellow colours which shall be at the same time solid and bright. These two valuable qualities are inversely proportioned to each other. But it is possible to join these colouring principles in such a manner as to add brightness to solidity. Green oak-bark joins perfectly with woad, and fumach with quercitron. By this mixture, we may succeed in combining with the oxyde of iron such vegetable colours as unite durability with brightness.

I shall conclude these observations with a remark concerning the use of astringents in the dying of cotton.

It has been pretended, that by increasing the proportions of fumach, or the bark of alder, or oak, it may be practicable to supply the place of galls in the red dye for cotton. I should be so much the more gratified if this were true, as the galls considerably add to the expence of our colours, and fumach may be had at a low price, because it grows almost every where in the dry situations of our southern climates ; but I can affirm that the substitution is impossible, however large the dose of this astringent may be ; for the colour is much paler, with less body and durability. I know that this is not the case with regard to wool and silk, in which this article is used with success ; and in giving an account of this difference, I think it may be ascribed to the nature of the nut-gall itself. 1. The acid, which it contains exclusively of the other astringents, as Berthollet has proved, facilitates the decomposition of the soap, with which the colours have been impregnated ; and then the oil remains fixed in their texture in a much larger quantity, and more intimately combined. 2. The nut-gall, which owes its development to animal bodies, retains a character of animalization, which it transmits to the vegetable piece ; and by that means, increases its affinities with the colouring principle of the madder ; for it is known how useful animal substances are to facilitate this combination. This animalization becomes useless in operations upon wool or silk.

VIII.

Account of the Experiments of Citizen CLOUTER, on the different States of Iron, and its Conversion into Cast-steel.*

THE learned reporter begins his account, by giving an historical sketch of the scientific and correct information we possess, respecting the art of steel-making. He states, that from

* From the report of Citizen Guyton, made to the National Institute of France, on 16th Messidor in the year VI. (July 4, 1798), inserted in the *Annales de Chimie*, XXVIII. 19.

the time when the labours of Reaumur had enlightened the practice of making natural steel, and steel by cementation, the theory remained stationary, notwithstanding the numerous and valuable experiments of Bergman, Rinmann, Priestley, &c. until the appearance of the excellent memoir of Vandermonde, Berthollet, and Monge, in the *Memoirs of the Academy of Sciences* for 1786. That the English, who had long supplied the European market with steel of cementation, remained also in the exclusive possession of the article known by the name of cast-steel, which, though confined to certain fine works *, is, nevertheless, a very valuable branch of national industry; that various experiments have been made with success, on a confined scale, in France to imitate this product, since the time when Jars published an account of the method used at Sheffield; but that from a want of precision in the narrations of these processes; and the difference which is, with justice, considered to subsist between the experiments of the laboratory, and those of the manufacturer in his extensive operations, the art of making cast-steel was considered, by the most eminent French chemists, as very far from being publicly known: and Vandermonde, Monge, and Berthollet, notwithstanding their acquaintance with these facts, thought fit to declare, in the public instructions drawn up by order of the committees of safety, that they could offer nothing but conjectures on the subject †. In this situation was the knowledge of the chemists and manufacturers of France, when Citizen Clouet resumed his experiments on a larger scale at the house of the Conservatory of Arts, and the mineralogical school at Paris, on the fusion of various kinds of steel, and the immediate conversion of iron into cast-steel.

On this subject, the author delivered a memoir to the Institute of France, which forms the subject of Guyton's report. He first treats of the combinations of iron and charcoal. One thirty-second part of charcoal is sufficient, as he affirms, to convert the iron into steel; one-sixth part of the weight of the iron affords a steel which is more fusible, but still malleable; and, after this term, it becomes nearer to the state of cast-iron, and no longer possesses enough of tenacity. By augmenting the dose of charcoal, the fusibility is increased; and, at last, it acquires the state of grey cast-iron.

The particular cast-iron, which results from the combination of iron and glass, forms the second object upon which the attention of Citizen Clouet was fixed. The glass enters but in a small quantity into this compound, notwithstanding which the properties of the mass are changed. This iron, though very soft to the file, if heated merely to cherry red, flies in pieces under the hammer: the cast ingot contracts greatly in cooling; and when, by careful management, it has been made into bars, the operation of hardening gives them the grain of steel, and renders them brittle, without adding to their hardness.

Charcoal in powder, added to the glass, changes the result, and increases the fusibility; but the nature of the product is greatly influenced by the dose of these ingredients. From one-30th to one-20th part of the iron affords steel capable of a high degree of hardness, which may be forged at a low red heat, and has all the properties of cast-steel. If more charcoal be employed, the products resemble those of the smelting-furnace.

* It is used in a great variety of common tools and works in this country.—N.

† *Phil. Journal* II. 102.

The attraction of iron for carbone, continues Citizen Clouet, is such that, at a very high temperature, it will take it even from oxygen. He proves this by the following experiments : Let iron, in small pieces, be put into a crucible with a mixture of equal parts of carbonate of lime and clay ; let the heat be urged to the degree necessary to weld iron, and kept at that elevation for an hour, or more, according to the size of the crucible : the metal being then poured into an ingot mould, will prove to be steel of the same quality as cast-steel.

The oxydes of iron are equally susceptible of passing through the states of soft iron, steel, and fusible or cast-iron, according to the proportions of coal made use of. The black oxyde of iron, of which the state appears to be the most constant, becomes iron when heated in the crucible with an equal bulk of charcoal powder : a double quantity affords steel. A progressive augmentation imparts the characters of the white and the grey cast-iron.

Lastly, Citizen Clouet observed the same transitions dependant on the respective quantities, by heating cast-iron, and the oxyde of iron ; cast-iron and forged-iron ; the oxyde of iron and iron ; the oxyde of iron and steel. No more then one-fifth of cast-iron is necessary to convert bar-iron into steel.

Iron and its oxyde do not intimately unite together. The black oxyde, mixed with half the quantity of charcoal which would be necessary for its reduction, affords iron, which is soft, possessing little tenacity, of a black colour, and indistinct fracture.

One-sixth of oxyde restores common steel to the state of iron, by heating them together in the forge, or in the way of cementation.

At the end of his memoir, Citizen Clouet has given observations on the manner of producing cast-steel, and the furnances proper for this effect.

He determines the nature of the fluxes, the degree of heat, the quality of the crucibles, the precautions for casting the ingot, the method of forging this kind of steel, the processes to be followed in experiments at the forge upon two kilogrammes of the materials, and the proportions to be given to a reverberatory furnace capable of heating four crucibles, each containing 12 or 13 kilogrammes of steel (about 28 pounds avoirdupois each crucible).

He remarks, that the mere ingredients of saline glass cannot be directly used in this process ; that glasses, which are too fusible, render the steel difficult to forge ; that steel, kept for a long time in fusion, takes up more glass than is proper ; and, lastly, that the melted matter must be stirred, and the glass carefully taken off before casting, in order to prevent its mixing with the steel.

The Commissaries of the Institute proceeded to repeat and verify the experiments of Citizen Clouet. These operations, which are related at length, were as follows. 1. Six hectogrammes (about 21 oz. avoirdupois) of filings of farrier's nails, and four of a mixture of equal parts of white marble, or carbonate of lime, and baked clay of an Hessian crucible, both reduced to powder, were well blended together, and exposed to the heat of a forge-furnace urged by three bellows-pipes for an hour and a half. The crucible failed at the first experiment ; but, on repetition, a bar of steel was afforded. 2. Upon making the experiment with Macquer's furnace, the fusion was not complete, though the fire had been urged to 151° of Wedgwood. 3. In another excellent wind-furnace, 367 grammes (about 13 oz. avoirdupois) of small-drawn iron

iron nails, and 245 grammes (about $8\frac{1}{2}$ oz. avoirdupois) of a mixture of carbonate of lime and baked clay, were exposed to strong heat for an hour, when the fusion was judged to be complete; and after removing the vitreous matter the ingot was poured out. From the effect produced on two pyrometric pieces, it was judged that the steel had undergone an heat of 150 degrees. This steel had all the properties of cast-steel, and was made into razors by Citizen Lepetitwalle, who found it of a good quality, easy to be worked, and capable of bearing a comparison with the cast-steel marked *Marshall* and *B. Huntsman*.

Upon these facts, the reporter observes, that since iron does not become steel but by taking up about 0.2013 of its weight of carbone*, and in the present process it exists only in the form of carbonic acid, this acid must consequently be decomposed; which happens, as the reporter observes, by means of a combination between its principles respectively and certain adequate portions of the iron, that is to say, the oxygen of the acid combines with part of the iron, and forms an oxyde with which the vitreous flux becomes charged, and the carbone combines with the rest of the iron, and forms steel. Hence it may be inferred, that this new process must be attended with a loss of so much more consequence, as it is necessary to use iron of the best quality for making steel. But, on this head, the reporter takes notice that the loss in the experiment with the wind-furnace was not quite one-twelfth part; and, in another experiment, by Vauquelin, the loss was less than one twenty-second part; a loss, which he observes, will be well repaid by the increased value of the product, and may reasonably be expected to be still less in operations on a large scale. He thinks, moreover, that this new method may probably turn out of high value for producing steel of uniform quality with regard to the dose of carbone. For he thinks that this quantity, or proportion, is likely to be determined by the equilibrium of the forces of affinity which cause the decomposition of the carbonic acid. Or, in other words, if we suppose an indefinite quantity of carbonic acid to be presented at an elevated temperature to a mass of iron not greater than could be converted by the dose of carbone contained in that acid, the iron will form two combinations, the oxyde and the steel; and it is conceived that the equilibrium between the attraction which tends to preserve the union of principles in carbonic acid, and those which are exerted between the iron and those principles will prove to be such that the carburet of iron will be formed precisely in those proportions which constitute good cast-steel. This subject, which certainly shews the acuteness of Citizen Guyton with regard to the doctrines of chemical attraction, must be decided by the test of experiment.

The report is concluded with a summary of the facts and observations it contains, together with an inference, that the immediate conversion of iron into steel, without using charcoal, is a great and valuable discovery with regard to the increase of national industry; that there is no doubt but the process will succeed in the large way, and that Citizen Clouet is entitled to a public recompence for his liberal, and unreserved communications.

That all the facts are of high value to science, and that the observation respecting the combi-

* This quantity (upwards of one-fifth) so much exceeds any addition which iron is stated to gain by conversion into steel, that I suppose it to be 0.2013 in the centenary or hundred parts of iron. Iron is reckoned to gain about a little more than half a pound in the hundred weight by cementation.—N.

nation of vitreous matter with iron, as well as that which shews that carbonic acid can produce the steel conversion, are new and important, cannot be questioned; but whether the use of carbonate of lime and clay, which is attended with some loss, may be preferable to vitreous flux and coal, which afford some small additional weight, is not yet, as it should appear, decided by the actual operations.

SCIENTIFIC NEWS.

Accounts of Books, &c.

Letters to a Merchant on the Improvement of the Port of London, demonstrating its Practicability without Wet Docks, or any additional Burdens being laid on Shipping, and at a less Expence of Time and Money than any other Plan proposed. By C. Dodd, Engineer, 1799.

THIS octavo pamphlet, of nineteen pages, has been given away by the publisher of the author's former Reports (Taylor in Holborn). The plan consists in building a new bridge, instead of, or upon, the present London-bridge, which shall consist of one principal centre arch, formed of iron, one hundred feet from low water to the crown of the arch, describing a span of 300 feet, with two large shore arches of 80 feet span, as near to the butment on each shore as advisable, for keeping deep water alongside the present below-bridge quays. Among the advantages of the structure, the principal is, that it will admit shipping to pass through, and arrive at the space between London and Blackfriars-bridge, which, as the engineer observes, would contain above double the number of ships proposed to be held in the new London docks, which have been long in agitation; at the same time that it would be near the centre of commerce, more than twice as cheap in time and money, and require no houses to be pulled down, nor established course of industry to be disturbed.

The slope on each side, which must necessarily be added to this structure, is proposed to have an inclination answering to three inches in the yard, and will then, as Mr. Dodd asserts, terminate, on the city side, at Monument-yard, and on the Borough side, near St. Thomas's Hospital. And this may be proportionally diminished, either in inclination, or in length, by making the main arch, only 60 or 70 feet high, which, it is observed, will afford the same advantages, provided the shipping strike their yards and topmasts. The evident advantages of this plan are detailed at length, with considerable animation, in these letters; in the second of which, the subject is considered, as to, 1. Practicability with regard to the erection. 2. Deepening the river above-bridge. 3. The erection of quays, wharfs, &c. 4. Facility of accommodation to the commerce of the port of London. 5. Advantages of commercial transactions in rivers, beyond those in docks, and, in particular, of this plan, beyond the plan of the London docks; and, 6. The possibility of completing this undertaking, without any expence being laid upon the shipping.

In consequence of this display and elucidation of the subject, a petition has lately been presented

sented to the House of Commons, in favour of the plan, and the City of London directed George Dance, esq. architect to the corporation, to examine and verify the local circumstances relating to the same. By the help of some notes I have taken from a section made by this architect, which I have seen at the Right Honourable Sir Joseph Banks's, I shall be enabled to make a few observations.

From some experiments made by the Society for the Encouragement of Arts, under the direction of their able secretary, Mr. Moore, with an appropriate instrument, for measuring the reaction, I understand that a horse, moving at the rate of three miles an hour, can only exert a force equal to 70 or 80*lb.* and from the general estimate of the work of horses, deduced at page 466 of the second vol. of our Journal, the reaction against a common horse, moving at the same rate, will prove somewhat less than 60*lb.** and the half of this, namely 30*lb.* will turn out to be the steady force exerted by a horse in a post-chaise, or carriage, which in London is seldom driven quicker than at the rate of about six miles and a half an hour; though, on the roads near London, they go at the rate of eight, and sometimes near nine miles an hour. I think it likely that there might be somewhat of temporary effort in the deduction of Mr. Moore, and that 70*lb.* may be a good estimate for the stout cart-horses in London; and, from various circumstances, particularly the over-rated deductions of several respectable authors, we may infer that the animal can double his efforts for a short time, such as ten minutes, without receiving any injury from the exertion. These speculations are not only of general utility, but are particularly applicable to the subject before us. If the slope of Mr. Dodd's bridge should be so considerable as to require additional horses to surmount it,—or if the length of the inclined plane, required to afford an easy ascent, should be such as to exceed every reasonable computation, with regard to the expences of construction, and the purchase of lands,—either of these results would afford the most serious objections to the plan itself. Mr. Dance's survey appears to have these particulars in view.

In his drawing, the new bridge is traced 100 feet high in the clear, *above high water*, and a thickness of rather more than ten feet is allowed between the interior curve of the crown of the arch, and the pavement. Hence the inclination of a line drawn from Monument-yard to the highest part of the bridge pavement gives 3,9 inches rise per yard, or very nearly one ninth. If we suppose, therefore, an horse to draw half a ton up this slope, the weight will re-act with about 120 pounds, and from the length of the line of inclination, it appears that the horse would have this pull to make for about three minutes. But I must not overlook that Mr. Dance's arch is taken from the top of high water, whereas, that of the projector is stated, in two places of his pamphlet, to be taken from low-water line; which makes a difference (as I gather from the two lines in Mr. Dance's section) of 22 feet, or more than one-fifth in the whole reaction, and reduces it to 85 pounds, or very little more than Mr. Moore's horses at plough did steadily overcome. If this reasoning be well founded, it will consequently follow, that the sudden ascent of Mr. Dodd's slope will not occasion any inconvenience, or

* That is to say, the horse raises (or moves against a re-action of) $2\frac{1}{2}$ hogsheads of water, or 1375*lb.* (at the rate of or) through ten feet in a minute. But three miles per hour, give the rate of 264 feet per minute; whence $264 : 10 :: 1375 : 60$.

render it necessary to take his smaller arch, which would diminish it by one-third more, and bring the resistance within the lowest estimate of re-action given in the foregoing paragraph.

Mr. Dance has shewn in his survey, that the mean inclination of Holborn-hill, from Field-lane to Bartlett's-buildings, gives a rise of 2,06 per yard, and that a slope of this kind would reach about half way between Lombard-street and Eastcheap; the latter of which streets would be nearly half shut in, while Thames-street would be entirely surmounted, or covered up;—that the inclination of Ludgate-hill from the obelisk in Bridge-street to the rails in St. Paul's Church-yard gives a rise of 0,96 inches per yard, and that a slope of this kind would reach beyond Thread-needle-street on the city side, passing at double the height of the houses on the south-side of Thames-street; completely covering Eastcheap; intercepting the second-floor windows at Lombard-street; crossing Cornhill at the height of twelve feet, or above the tops of the shop-windows; and nearly four feet high at the end of Thread-needle-street. The same slope on the Borough side would reach completely through that town, and meet the ground on the road beyond the stones' end: the whole length of both slopes being above one mile and a quarter. It seems unnecessary to enter into the consequences deducible from these facts, or to observe, how much the length of these inclined roads would be diminished by rectifying the drawing with regard to the water-line, or by adopting the lowest of Mr. Dodd's arches; the latter of which would make a difference of nearly two to one. For if the extreme declivity, or shortest inclined plane, in Mr. Dance's plan can be easily surmounted by a loaded horse, for the time required to pass over its length, it would follow, of course, that speculations respecting other planes will be scarcely applicable to the subject *.

I have received a letter from the Rev. Mr. Richardson, of Bath, inclosing the second letter stated by Dr. Gibbes (see p. 96 of our last number) to be strongly in his favour; and also a letter from Dr. Beddoes, on the same subject; which letters I hope will terminate the discussion. Mr. Richardson (April 20) quotes an assertion of Mr. Notcutt, made in conversation since the dispute, that he did not know what the specimen was, but conjectured it might be sulphate of strontian; at the same time that Mr. N. informed Mr. R. that he did not yet know its composition, as neither he, nor Mr. Clayfield had analysed it. The subsequent part of Mr. R.'s letter, contains general inference and observation of that kind, which, in selecting the mere facts, I have avoided relating; and a request that Dr. Beddoes would transmit to me the facts contained in his letter. From Dr. B.'s letter, I find that he did not think it incumbent on him to publish Mr. R.'s report of Mr. Notcutt's testimony, because he was in possession of the testimony itself; and he therefore returned this second letter in time, with an invitation to Mr. R. to send it to me, if he, or Dr. G. thought it to the purpose. With regard to the general subject, Dr. B. asks me, whether I ought not to distinguish between the

* Since the above was composed, I have been informed that in Mr. Dodd's plan, first published in a single sheet, the main arch was proposed to be elevated one hundred feet above high water, and that this was the plan submitted to Mr. Dance.

notoriety of a discovery, and of the discoverer? Whether the name of the latter could be said to be before the public? Whether I, or one in many hundreds, even of my English readers, knew it? And whether Dr. G.'s paper will not make him pass for the discoverer abroad, till the succeeding numbers of the Journal shall correct this error? And he adds, that there can be no doubt of the similarity of the Sodbury mineral, to some varieties of that nearer Bristol. Mr. Clayfield suspects the Red-land, Ham-green, and Sodbury strontian to come from the same vein.

With regard to the questions, there is but one which seems to call on me for an answer; and to this I reply, that I knew nothing of Mr. Clayfield, and should probably have mentioned him in a note, or modification of the title at page 535, of the second volume of this Journal, if I had.

Extract of a Letter from Mr. H. Davy.

"When I mentioned to Mr. Clayfield the existence of silex in the epidermis of vegetables, he requested me to examine the equisetum hyemale, or Dutch rush, used in manufactories for polishing brass, &c. A short time after, Mr. Notcutt informed me that he had succeeded in obtaining a globule of glass from it by the blow-pipe. I have examined this plant, and find that the epidermis of it is almost wholly composed of silex, which is reticularly disposed like that of the bonnet cane, or calamus rotang.

"Since my last letter to you, I have seen a paper of the ingenious Vauquelin, in the 88 No. of Annales de Chimie, *On the Excrement of Fowls compared with their Nourishment; with some Observations on the Formation of the Egg*. He found in the ashes of the oat-grain, silex and phosphate of lime. I have not yet had time to examine any grains or seeds, but it is more than probable that the silex exists in the epidermis of the oat-grain, and not in the farina. The husks of wheat and barley, as well as the seeds of grasses, and hard shells and capsules of fruit, will, I have no doubt, be found to contain flint."

The following printed address was transmitted to me by the penny-post, under cover of a letter from Mr. Pepys, jun. to acquaint me that the Society had ordered it to be sent to the Scientific Journals. In return, I sent a letter requesting information concerning the constitution and funds of the Society, particularly with a view to inform the public of the regulations under which they might contribute to the desirable object held forth, as well as of the degree of confidence this body might be entitled to. To these inquiries I received only a verbal answer, that the Society is not yet organized, and that a prospectus will be sent to me as soon as published.

British Mineralogical Society.

THE benefits which have resulted to our manufactures, domestic economy, and convenience, from the great progress made in other branches of science, have, for a considerable time, called for the establishment of a Society whose attention should be directed to the analysis and reduction of our native ores and minerals.

The principal obstacle that hindered our mineralogical treasures from being explored has, in some measure, been happily removed by the advances that have been made in chemical knowledge; and there is now every reason to hope that, at no very distant period, by the gradual removal of other impediments, an accurate knowledge will be obtained of the composition, nature and properties, not only of such minerals as are generally found in cabinets, but of the various hitherto neglected strata of the different parts of this kingdom.

There are, no doubt, in many of our mines, other valuable metals and minerals (at present imported from abroad) besides those they are worked for; which want of chemical knowledge respecting the compound state of such productions have caused to be hitherto entirely neglected. Nor indeed could it be expected that the miner, whose business is to produce a given quantity of a known metal in a short time, should so far wander from his track as to attempt to examine what he has always been taught to consider as waste. When he meets with any other substance than the one he is in quest of, it excites no attention, and in many instances is turned out of the mine, and scattered like rubbish to repair the roads.

To assist in preventing a waste of the bounties afforded by Providence to this country, THE BRITISH MINERALOGICAL SOCIETY has been instituted; but the landholders and proprietors of mines can alone overcome the obstacles which arise from the prejudices of the miners, by causing them to produce samples of the various strata they pass through, and by giving them premiums for every new substance of which they may be able to produce specimens.—The country will be benefited exactly in the proportion that such a practice shall become general.

The British Mineralogical Society will analyse, *free of expence*, package and carriage excepted, for the proprietors of mines or landed estates, whatever substance they may meet with in sufficient quantity to render a knowledge of the component parts a desirable object. In return, the Society expects that singular or curious specimens of the mineral productions, which may be met with in working the British mines, will be sent for the improvement of the Society's Cabinet, which will thus in time become a national ornament.

It must be obvious, that, in pursuits of this nature, a Society possesses many advantages over an individual; for, in every case where the component parts of a mineral are very intricate, the knowledge and experience of all the members can be concentrated for the analysis.

When the analysis of any mineral sent for examination is completed, the result, with an account of the methods employed, will be sent to the proprietor of the specimen; and if it shall contain any new substance, the probable uses to which it may be applied will be suggested.

Such is the outline of the Plan of the Society, which, having nothing mercenary in view, encourages its members to expect that their labours for the mineralogical improvement of the British nation will be seconded, not only by those who have an immediate interest in obtaining an accurate knowledge of the nature and properties of the various mineral productions of their estates, but by every individual who has the prosperity of his country at heart.

Specimens sent for analysis are requested to be packed up in such a manner as to prevent, as much as possible, their being rubbed or fretted by the carriage; as the external structure and character

character of minerals often assist in leading to a knowledge of their composition. It is also requested that an account be sent of the circumstances connected with the specimen, as the depth below the surface of the earth, and the nature and thickness of the stratum in which it is found, and also of the incumbent and subcumbent strata, &c. and, where necessary, specimens of these with their provincial technical names.—Address the package (carriage paid) to the Society's Secretary, Mr. W. H. Pepys, junior, No. 24, Poultry, London.

By Order of the Society.

W. H. PEPYS, jun. *Secretary.*

A Practical Introduction to Spherics and Nautical Astronomy, being an attempt to simplify those useful Sciences,—containing, among other original Matter, the Discovery of a Projection for clearing the Lunar Distances in order to find the Longitude at Sea; with a new Method of calculating that important Problem. By P. Kelly, Master of Finsbury-square Academy, London. Johnson and Robinsons. Price 6s. p. 210. pl. 17.

This work is divided into two parts; the first contains the principles of stereographic projection, exemplified by numerous figures drawn upon a large scale; these are followed by right-angled and oblique spheric trigonometry, in which all the problems are solved both by projection and calculation: this part concludes with improved solutions of certain cases of spheric triangles.

The second part treats of astronomy, particularly those parts which are useful at sea; such as finding azimuths, amplitudes, time, latitude, and longitude. These problems are solved by the globes, and likewise by projection and calculation.

The method of finding the longitude by lunar observation is here explained at some length, in a clear and elementary manner. The principles of this problem are explained by stereographic projections, whence rules are deduced for estimating the true distance beforehand.

A new projection here given for clearing the distance from parallax and refraction, is performed by drawing four right lines from the plan scale. The author does not insist upon this method as affording a perfect solution of the problem, but he observes that it will be found sufficiently exact for the general purposes of navigation, and where perfect correctness is required, it will even be found useful as a guide or check to calculation.

The new method given for working the lunar observation, consists in the regular solution of two spherical triangles, and taking the zenith distances instead of the altitudes, by which the operation is performed by sines only: this is certainly an advantage, especially to learners, who are apt to confound sines with cosines, &c. but however simple and accurate this method may be, it cannot be conveniently applied except by such as are in possession of Taylor's Logarithms, or some other tables which readily afford portions of the quadrant to seconds.

The figures throughout this work are on a large scale, and appear to be very correct.

Fig. 2.

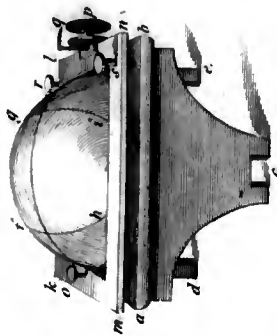


Fig. 1.

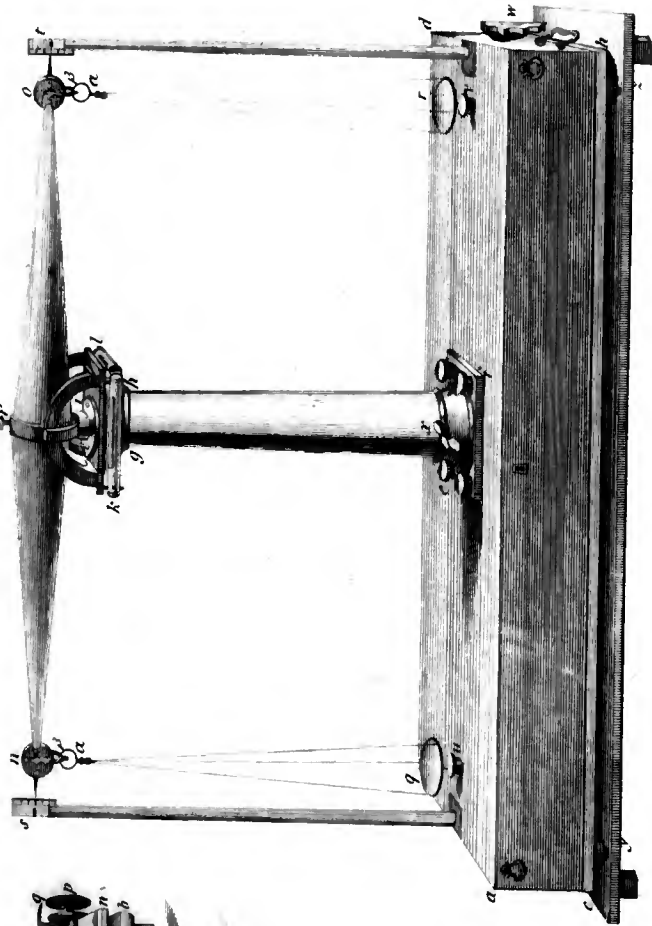


Fig. 7.

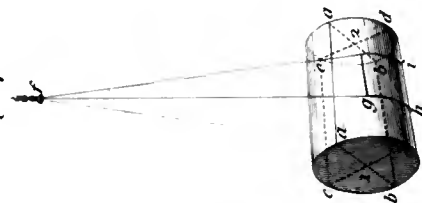


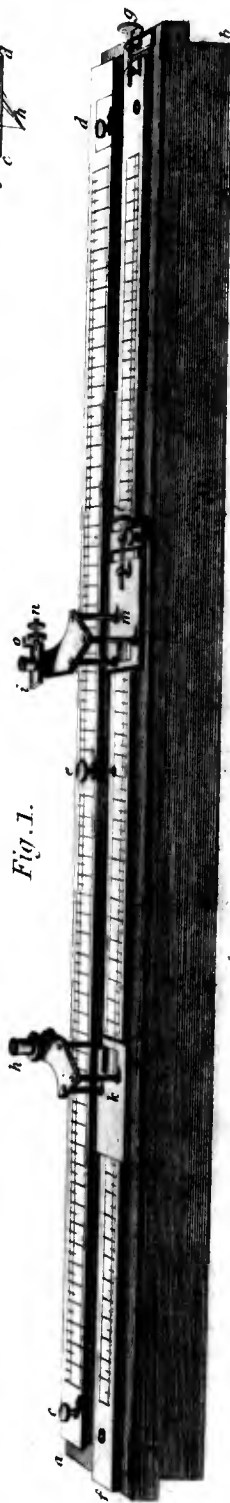
Fig. 5.



Fig. 3.



Fig. 1.



Barlow's copy.

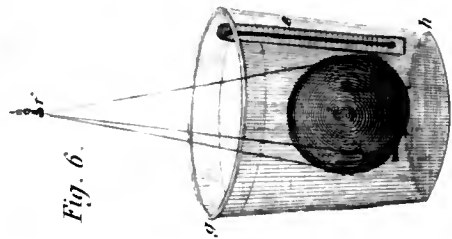


Fig. 6.



Barometric & thermometric first movers.

Fig. 1.

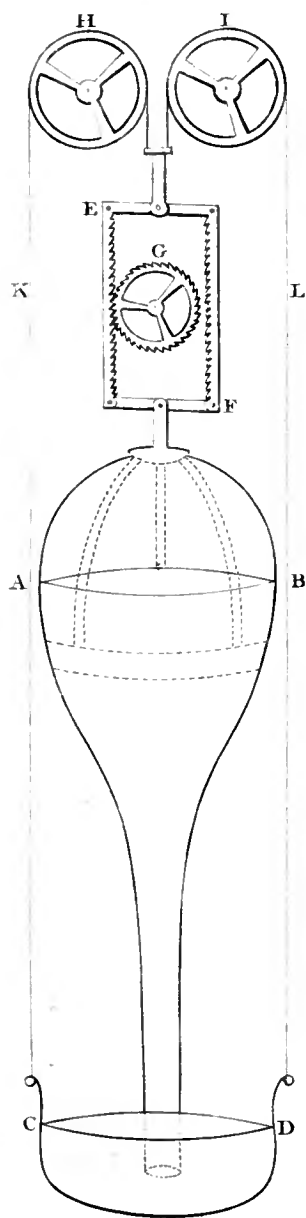


Fig. 2.



Fig. 3.

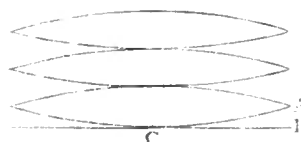
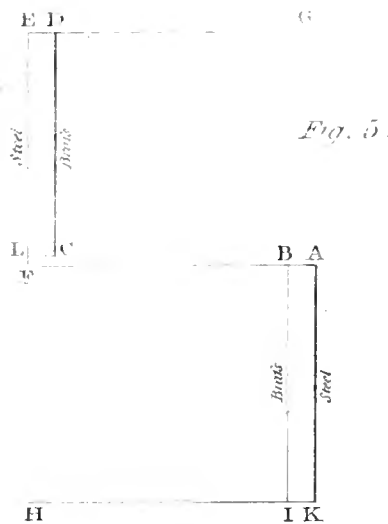
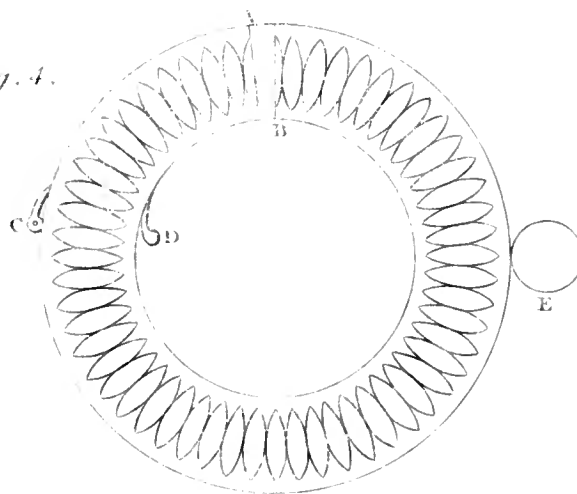


Fig. 4.





A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JULY 1799.

ARTICLE I.

Observations upon an unusual Horizontal Refraction of the Air, with Remarks on the Variations to which the lower Parts of the Atmosphere are sometimes subject. By the Rev. S. VINCE, A.M. F.R.S. and Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge.*

THE uncertainty of the refraction of the air, near the horizon, has long been known to astronomers, the mean refraction varying by quantities which cannot be accounted for, from the variations of the barometer and thermometer; on which account, altitudes of the heavenly bodies, which are not more than 5° or 6° , ought never to be made use of, when any consequences are to be deduced from them. The cause of this uncertainty is probably the great quantities of gross vapours and exhalations of various kinds which are suspended in the air, near to the earth's surface, and the variations to which they are subject; causes of which we have no instruments to measure the effects which they produce in refracting the rays of light. In general, the course of a ray passing through the atmosphere, is that of a curve, which is concave towards the earth: the effect of which is, to give an apparent elevation to the object, and thus the heavenly bodies appear above the horizon, when they are actually below it; but it

* Philof. Transf. 1799, p. 13.

will not alter the position of their parts, in respect to the horizon; that is, the image of the highest part of the object will be uppermost, and the image of the lowest part will be undermost. The figures, however, of the sun and moon, when near the horizon, will suffer a change in consequence of the refraction of the under limb being greater than that of the upper, from which they assume an elliptical form; the minor axis of which is perpendicular to the horizon, and the major axis parallel to it. But a perpendicular object, situated upon the surface of the earth, will not have its length altered by refraction; the refraction of the bottom being the same as that of the top*. These are the effects which are produced upon bodies at, or near, the horizon, in the common state of the atmosphere, by what I shall call the *usual* refraction.

But besides the usual refraction which affects the rays of light, the atmosphere over the sea is sometimes found to be in a state which refracts the rays in such a manner, as to produce other images of the object, which we will call an effect from an *unusual* refraction. In the Phil. Trans. for 1797, Mr. Huddart has described some effects of this kind, which he has accounted for, by supposing, that from the evaporation of the water, the refractive power of the air is not greatest at the surface of the sea, but at some distance above it; and this will solve in a very satisfactory manner, all the phenomena which he has observed: but effects very different from those described by Mr. Huddart, are sometimes found to take place. These I had an opportunity of observing at Ramsgate, last Summer, on August the first, from about half an hour after four o'clock in the afternoon, till between seven and eight. The day had been extremely hot, and the evening was very sultry; the sky was clear, with a very few flying clouds. I shall describe the phenomena as I observed them with a terrestrial telescope, which magnified between thirty and forty times; they were visible, however, to the naked eye. The height of the eye above the surface of the water, at which most of the observations were made, was about twenty-five feet; some of them, however, were made at about eighty feet from the surface; and it did not appear that any of the phenomena were altered, from varying the height of the eye, the general effect remaining the same.

The first unusual appearance which I observed, was that which is represented in Plate VII. fig. 1. Directing my telescope at random, to examine any objects which might happen to be in view, I saw the top of the masts of a ship, A, above the horizon, $x y$, of the sea, as shewn in the figure. At the same time also, I discovered in the field of view two complete images, B, C, of the ship in the air, vertical to the ship itself. B being inverted, and C erect, having their hulks joined. The phenomenon was so strange, that I requested a person present to look into the telescope, and examine what was to be seen in it, who immediately described the two images as observed by myself; indeed they were so perfect, that it was impossible we could differ in our description. Upon this, I immediately took a drawing of the relative magnitudes, and distances of the ship, and its images, which at that time were as represented in the figure, as near as it was possible for the eye to judge; and it was very easy to estimate them to a very considerable degree of accuracy. As the ship was receding from the shore, less and less of its

* See my Complete System of Astronomy, art. 194.

mafts became vifible; and continuing my obfervations, in order to difcover whether any or what variations might take place, I found, that as the fhip defcended, the images, B, C, afcended; but as the fhip did not fink below the horizon, I had not an opportunity of obferving at what time, and in what order the image would have vanifhed, if the fhip had fo difappeared.

Being defirous of feeing whether the fame effect was produced upon the other fhips which were vifible, I directed my telescope to another fhip, A (fig. 2), whole hulk was juft in the horizon, $x y$; when I obferved a complete inverted image, B, the main-maft of which juft touched that of the fhip itfelf. In this cafe, there was no fecond image as before. The fhip, A, moving upon the horizon, B continued to move with it without any variation in its appearance.

The next fhip which I directed my telescope to, was fo far on the other fide of the horizon, $x y$, as juft to prevent its hulk from being feen, as is reprefented by A (fig. 3). And here I obferved only an inverted image of part of the fhip; the image, y , of the topfail, with the maft joining that of the fhip, the image x of the top, a of the other maft, and the image z of the end c of the bowsprit only appearing at that time. Thefe images would fuddenly appear and difappear very quickly after each other; firft appearing below, and running up very rapidly, fhewing more and lefs of the mafts at different times as they broke out; refembling in the fwiftnefs of their breaking out, the fhooting out of a beam of the aurora borealis. As the fhip was defcending on the other fide the horizon, I continued my obfervations upon it, in order to difcover what changes might take place; when I found, that as it continued to defcend more of the image gradually appeared, till at laft the image of the whole fhip was completed, with their main-mafts touching each other; and upon the fhip defcending lower, the image and the fhip feparated; but I obferved no fecond image as in the firft cafe: a fecond image, however, might probably have appeared if the fhip had continued to defcend.

Upon moving my telescope along the horizon, in order to examine any other fhips which might be in fight, I obferved, juft at the horizon, $x y$ (fig. 4), the top, a , of the maft of a fhip; and here an effect was obferved which had not been difcovered; for there was an inverted image, B, vertical to, a , an erect image, C, both of them very perfect and well defined; and an image, $v w$, of the fea between them, the water appearing very diftinctly. As the fhip was coming up towards the horizon, I continued to obferve it, in order to difcover the variations which might follow, and found that, as the fhip approached the horizon, the image C gradually difappeared, and at laft it vanifhed; and after that, the image $v w$ of the fea difappeared; and during this time the image B defcended; but the fhip did not rife fo near to the horizon as to bring the main-mafts together. Had I directed my telescope to the fame point of the horizon a little fooner, I fhould have feen the two images before the fhip itfelf was vifible; in fact, the images were vifible when the whole fhip was actually below the horizon; for from the very fmall part of the maft which was at firft vifible, that part muft then have been below the horizon, and appeared above it by the ufual refraction; the altitude of a above the horizon having then been much lefs than the increafe of altitude which arifes from the common horizontal refractions. The difcovery of fhips in this manner, might in fome cafes be of great

importance; and on such occasions, it might be worth while to appoint proper persons to make observations for that purpose.

The cliffs of Calais being very visible, I directed my telescope towards them, in order to examine whether there were any thing unusual in their appearance; when I observed an image of the cliffs above the cliffs themselves, together with an image of the sea separating them, as is represented in fig. 5, in which xy represents the horizon of the sea, AB the cliffs, ab their image, and vw the image of the sea between them; the depth of ab was much less than that of AB . It is probable, however, that vw might not be the image of the sea immediately adjoining to the cliffs, but a partial elevation of the sea at some distance from them; and that the image vw might intercept some part of the image ab , which would otherwise have been visible; we must not, therefore, conclude that the image ab , so far as it appeared, was less than the corresponding part of the object. From the memorandums which I made at the time of observation, I do not find that I examined the appearance of the cliff, AB , and its images ab , which had there, at that time, been any striking marks in them, would have determined whether the object and its image were of the same magnitude. The image ab was, however, erect; the boundaries on the top of AB and ab agreeing together. Having examined this for some time, and taken a drawing of the appearance, during which I could discover no variation, I directed my telescope to other objects; and upon turning it again to the same cliffs, after the space of about six or seven minutes, the images ab and vw were vanished: but examining them again soon after, the images were again visible, and in every respect the same as they appeared before. A short time after they disappeared, and did not appear any more.

Soon after the above appearances, I observed a ship, C , with the hulk below the horizon, xy , passing by the same cliffs, AB ; an inverted image, D , of which appeared against the cliffs, as represented in fig. 6. The ship was in motion, and remained at the same distance on the other side of the horizon. I continued my observations upon it, till it had passed the cliffs for a considerable distance, but there was no change of appearance. The cliffs were illuminated by the sun, and appeared very distinctly; but there was no image above, as in the last case.

Continuing to observe the same cliffs, AB , fig. 7, I soon after discovered two partial elevations, m n , of the sea, by the unusual refraction; they changed their figures a little, and disappeared in the place where they first appeared, and were equally distinct in every part.

About this time, I discovered a very thick fog coming upon the horizon from the other side, rolling upon it with a prodigious velocity, curling as it went along, like volumes of smoke sometimes out of a chimney. This appeared several times: I conclude, therefore, that there was a considerable fog on the other side of the horizon.

The last phenomenon which I observed, was that which is represented in fig. 8, where xy represents the horizon, ab two partial elevations of the sea meeting at c , and continued to d ; e , another partial elevation of the sea, of which kind I observed several, some of which moved parallel to the horizon with a very great velocity. I conjecture, therefore, that these appearances were, in part at least, caused by the fog on the other side the horizon; for though I did

did not at the same time see the motion of these images, and that of the fog, yet from memory, I judged the motions to be equal; and they were also in the same direction. A fog, which by producing an unusual refraction, might form these images, would, by its motion, produce a corresponding motion of the images.

I have here described all the different phenomena which I observed from the unusual refraction, of most of which I saw a great many instances. Every ship which I observed on the other side of the horizon of the sea, exhibited phenomena of the kind here described, but not in the same degree. Of two ships, which in different parts were equally sunk below the horizon, the inverted image of one would but just begin to appear, whilst that of the other would represent nearly the whole of the ship. But this I observed, in general, that as the ship gradually descended below the horizon, more of the image gradually appeared, and it ascended; and the contrary, when the ships were ascending upon the horizon in different parts, one ship would have a complete inverted image; another would have only a partial image; and a third would have no image at all. The images were in general extremely well defined, and frequently appeared as clear and sharp as the ships themselves, and of the same magnitude. Of the ships on this side of the horizon, no phenomena of this kind appeared; there was no fog upon our coast, and the ships in the Downs, and the South Foreland, exhibited no uncommon appearances. The usual refraction at the same time was uncommonly great, for the tide was high, and at the very edge of the water I could see the cliffs at Calais a very considerable height above the horizon; whereas they are frequently not to be seen, in clear weather, from the high lands about the place. The French coast also appeared both ways to a much greater distance than I ever observed it at any other time, particularly towards the east, on which part also the unusual refraction was the strongest.

During the remainder of my stay at Ramsgate, which was about five weeks, I continued daily to examine all the ships in sight; but I discovered no phenomena similar to those which I have here given a description of. The phenomenon of the ship, observed by Mr. Huddart, differed altogether from those above described, as the inverted image which he observed was below the ship itself. An appearance of this kind I observed on August the 17th, about half an hour after three o'clock in the afternoon, of which fig. 9 is a representation. The real ship is represented by A, and the image by B; *e r*, *m v*, the hulks; *s t*, the flag; and *w x*, its image, just touching it, with the sea, *x y*, below. Between the two hulks, some faint dark spots, and lines appeared, but I could not discover what they were. The representatives of the vessel, at the time of this appearance, was not quite come up to the horizon; and as it approached it, the image gradually diminished, and totally disappeared when the ship arrived at the horizon.

It remains now, that we enquire into the causes which might produce the very extraordinary effects which have been above related. From the phenomena, we are immediately led to the nature of the path of the rays of light to produce them; and we may conceive that the air may possibly be in such a state as will account for the unusual tract which they must have described.

described. For let b (fig. 10), be the surface of the sea; $a b z$, an object; E , the place of the eye; $a r E$, $b s E$, the progress of two rays by the usual refraction from the extreme parts of the object to the eye; to these curves, draw the tangents $E a'$, $E b'$, and $a' b'$ will be the image of the object, as usually formed. Now if we take the case represented in fig. 4. let $a'' b''$ represent the inverted image, and $a''' b'''$ the erect image. Join $a'' E$, $a''' E$, and $b'' E$, $b''' E$, and these lines must respectively be the directions of the rays entering the eye, from $a b$, in order to produce the images $a'' b''$, and $a''' b'''$, hence these lines must be tangents at E , to the curves which are described by the rays of light; let, therefore, $a n E$, $a m E$, $b v E$, $b w E$, be the curves described. We have, therefore, to assign a cause which may bring rays passing above the rays $a r E$, $b s E$, to the eye at E . Now if there were no variation of the refractive power of the air, a ray of light passing through it would describe a straight line; therefore the curvature of a ray of light passing through the atmosphere depends upon the variation of the refractive power of the air. If, therefore, we suppose the air lying above $a r E$ to vary quicker in its refractive power than the air through which $a r E$ passes, the curvature of a ray proceeding above that of $a r E$ will be greater than the curvature of $a r E$; and upon this principle we may conceive that a ray may describe the curve $a n E$, and in like manner, if a quicker variation of refractive power should take place above the curve $a n E$, than in that curve, a third ray may describe the curve $a m E$. The same may be said for the rays $b v E$, $b w E$, diverging from b . The alterations of the refractive power may arise partly from the variations of its density, and partly from the variations of its moisture; and the passage of the rays through the boundary of the fog, may there suffer a very considerable refraction; for from the motion of the fog, and that of the images above mentioned, I have no doubt that the fog was a very considerable agent in producing the phenomena. When all the causes co-operate, I can easily conceive that they may produce the effects which I have described. If the cause should not operate in the tract of air through which the curves $a n E$, $b v E$ pass, but should operate in the tract, through which $a m E$, $b w E$ pass, an erect image which would be visible, but there would be no inverted image; and should it operate in the latter case, but not in the former, there would be only an inverted image.

As the phenomena are very curious and extraordinary in their nature, and have not, that I know of, been before observed, I have thought proper to lay a description of them, with all the attending circumstances, before the Royal Society. They appear to be of considerable importance; as they lead us to a knowledge of those changes, to which the lower parts of the atmosphere are sometimes subject. If when these phenomena appear, a vessel furnished with a barometer, thermometer, and hygrometer, below, and also at the top of the mast, were sent out to pass below the horizon, and return again, and an observer at land, having like instruments, were to note, at certain intervals, the situation and figure of the images, it might throw further light upon this subject, and lead to useful discoveries respecting the state of the atmosphere, from a conjunction of the causes which affect these instruments.

II.

An Account of some Endeavours to ascertain a Standard of Weight and Measure. By Sir GEORGE SHUCKBURGH EVELYN, Bart. F.R.S. and A.S.

(Continued from p. 107, of the present volume.)

(§. 20.) *Experiment of the Cube of Brass weighed in Air.*

THE cube was suspended to the right arm of the beam, by the scale belonging to it, and the left scale pan, with the mark x, was hung at the other end of the beam, in which were placed the following weights*, made by E. TROUGHTON.

viz. No. 15 of ^{grains.} 16384	
14 -	8192
13 -	4096
12 -	2048
11 -	1024
9 -	256
	84,82

The total weight of the cube in air	}	= 32084,82	{	the barom. being at	inch.
				the therm. - - -	29,0 at 62°,0

(§. 21.) *Experiment of the Weight of the Cylinder in Air.*

No. 15 of ^{grains.} 16384	
13 -	4096
11 -	1024

	grains.		53,37
But a counterpoise of 555,02	}	= +	<hr/>
having been used,			21,557,37
by mistake, in-			
stead of - - 552,34			2,68
Add this excess = 2,68			

And the total weight of the cylinder is	=	21,560,05	{	the barometer at	inch.
				the thermometer at	29,0 - - 62°,0

(§. 22.) *The Cube weighed in distilled Water.*

Sept. 5. Put into the left scale, the counterpoise for the water	{ 300 }	}	=	-	-	grains.
scale	{ 100 }					

The cube, with its scale, was then immerfed in the water.

* This scale contained also 555,02 grains, being the weight or counterpoise to the scale for the cube.

I then

I then restored the equilibrium, by putting into the opposite or left-hand common scale, Mr. Troughton's weights, No. 10.	grains.	
(The barom. standing at 29,47 inches,		= 512,00
the therm. at 60°,2.)		200,
		30,
		3,70
But a counterpoise of	grains.	
having been taken, by mistake, instead of	400	745,70
	442,75	42,75
Deduct the difference, which was so left out	= 42,75	
The apparent weight of the cube in water becomes		= 702,95
Add the correction * for the loss of weight of the 4 wires, by immersion 2½ inches deeper than when the counterpoise was adjusted		= + ,08
And the true corrected weight of the cube in water, with 60°,2 of heat becomes		703,03

(§. 23.) *Experiment of the Cylinder in distilled Water.*

Sept. 5. The thermometer being at from 60°,2 to 60°,5, and the barometer 29,47 inches,

Put into the left scale pan, the counterpoise to the water-scale for the cylinder	grains.	
	300	
	100	= 441,7
	41,7	

The cylinder, with its water-scale, was immersed in water. I then restored the equilibrium, by putting into the left scale,

Mr. Troughton's weights, No. 12	grains.	
	= 2048	
No. 9	= 256	
	200	
	30	
	10	
	4	
	1,10	
Weight of the cylinder in water		= 2549,10

* When the cube was immersed, the water in the glass jar stood 2½ inches higher than when the counterpoise for this water-scale was adjusted, and found to be 442,75 grains (see §. 18.); and 1 inch of alteration in the height of the water having appeared to be = 0,0354 grain in weight (§. 19.), 2½ inches will be = 0,078 grain; and so much must be added, to correct for the loss of weight in the four wires that suspended the scale and cube in water. When the cube was immersed, the surface of the water stood 1,5 inch below the top of the glass jar, and 9,7 inch below the centre of the beam, or index point.

When the cube was in the water, the beam was clearly sensible with 1/10 of a grain.

Add

Add the correction for the loss of weight of the four wires, by being $1\frac{1}{2}$ inch deeper immersed in the water, than when the counterpoise was adjusted } = + 0,05*

Corrected weight of the cylinder in water - - - - = 2549,15

After this experiment, I discovered that some small bubbles of air had insinuated themselves between the cylinder and the scale in which it hung; these therefore were removed, and the experiment repeated, as follows:

* In order that *this* and some other corrections may be the more easily applied, I have computed the three following tables, to be used whenever great accuracy is required.

Table I. Shewing the expansion of cast brass, both in length and solidity, and also of water, in solidity, by the effect of heat: the former is derived from Mr. Smeaton's experiments (Phil. Transf. vol. XLVIII.); and the latter from some of my own, when I was a resident member of the university of Oxford.

Degrees of Heat.	Expansion of Brass.		Expansion of Water.
	In Length.	In Solid y	In Solidity.
	o Millionth Parts.	Millionth Parts.	Millionth Parts.
1	1	3	165
2	2	6	330
3	3	9	495
4	4	12	660
5	5,2	16	825
6	6	19	990
7	7	22	1155
8	8	25	1320
9	9	28	1485
10	10,4	31	1650

Table II. Shewing the correction for the wires, or the diminution of the weight of the water-scales, by immersion in water.

By Immersion in Water.	The 4 Wires of the Cube, or Cylinder.	The 3 Wires of the Sphere lose
Inches.	Grains.	Grains.
1	— 0,035	— 0,078
2	— 0,071	— 0,157
3	— 0,106	— 0,235
4	— 0,142	— 0,314
5	— 0,177	— 0,392
6	— 0,212	— 0,471
7	— 0,248	— 0,549
8	— 0,283	— 0,628
9	— 0,319	— 0,706
10	— 0,354	— 0,785
20	— 0,708	— 1,570

Table III. Shewing the correction of the weight of the sphere in air, on account of the weight, or heat, of the atmosphere.

Barometer.	Correction.	Therm.	Correction.
Inch. $\frac{1}{10}$.	Grains.	o	Grains.
29,5	0,00	50	0,00
1	— ,12	1	+ 0,10
2	,23	2	0,20
3	,35	3	0,30
4	,47	4	0,40
5	,58	5	0,50
6	,70	6	0,60
7	,82	7	0,70
8	,94	8	0,80
9	1,05	9	0,90
10	1,17	10	1,00

N.B. If the barometer is below 29 $\frac{1}{2}$ inches, or the thermometer below 50°, use the contrary signs.

N. B. 80 inches in length, of the wires } grs.
for the scales for the cube and cylinder weigh } 6,16
therefore 1 inch will be, 0,77 grain, }
and 4 wires of 1 inch } = 0,308
Also, 91 inches of the wire for the }
sphere weigh } = 20,71
and 1 inch = 0,227, and 3 wires of }
1 inch } = 0,683
and the specific gravity of the wire is = 8,7 .

VOL. III.—JULY 1799.

Water being taken as heavier than air, as 836 : 1 (see Observations in Savoy, Phil. Transf. for 1777), the barometer being at 29,27, and thermometer 51°, a sphere of air equal in bulk to the brass sphere, viz. = 113 $\frac{1}{2}$ cubic inches, would weigh, when the barometer was 29,5 inches, and the thermometer 50° = 34,57 grains; and 1 cubic inch of such air = 0,304
This correction will serve for any other body whose bulk is known.

X

No.

$$\begin{array}{rcl}
 \text{Weights as before} & \left\{ \begin{array}{l} \text{No. 12} = 2048 \\ \text{No. 9} = 256 \\ 200 \\ 30 \\ 10 \\ 4 \\ 1,10 \end{array} \right. & \begin{array}{l} \text{grains.} \\ \\ \\ \\ \\ \\ \end{array} \\
 \\
 \text{The buoyancy of the air bubbles being removed} & \left\{ = + \right\} & \left\{ \begin{array}{l} 3 \\ 1,07 \end{array} \right. \\
 \\
 \text{Add the correction for the loss of weight in the wires, as before} & \left\{ = + \right\} & \begin{array}{l} 2553,17 \\ 0,05 \end{array} \\
 \\
 \text{And the more exact weight of the cylinder in water becomes} & \left\{ = 2553,22 \right\} & \begin{array}{l} \text{with the temperature } 60^{\circ},5 \\ \text{and the barometer } 29,47 \\ \text{inches.} \\ \text{inches.} \end{array} \\
 \\
 \text{Note, when } \left\{ \begin{array}{l} \text{the cube} \\ \text{the cylinder} \end{array} \right\} & \left\{ \begin{array}{l} \text{was weighed in water, its} \\ \text{centre was below the} \\ \text{surface of the water} \end{array} \right\} & = \begin{array}{l} 2,5 \\ 3,7 \end{array}
 \end{array}$$

that is, the cylinder was the deepest by - - - = 1,2

The repetition of this experiment shews how necessary it is to attend to the most trifling circumstances: there were not more than three or four of these particles of air, and those not larger than a small pin's head. Moreover, it may be noted, the distilled water in which these experiments were made, being afterwards examined with my (Martin's) hydrometer, in the heat of $60^{\circ}\frac{1}{2}$, weighed on that scale = 1,0005; so that I see no reason for diffidence in the quality of the water.

(§. 24.) *A Synopsis of the preceding Experiments.*

	Cube.	Therm.	Cylinder.	Therm.	Barom.
	Inches.	°		°	Inches.
Contents (true to $70^{\circ}\frac{2}{100}$) in inches	124,18917	61	74,94826	62	
	grains.				
Weight in air, true to 0,02 grain	32084,82	62	21560,05	62	29,00
Weight in water, true to 0,10 grain	703,03	60,2	2553,22	60,5	29,47
Weight of an equal bulk of water, true to 0,12 grain, or $70^{\circ}\frac{1}{100}$	31381,79		19006,83		
Weight of a cubic inch of water, from these experiments	252,694		253,600*		

* The weight of a cubic inch of common or rain water has been reckoned about 253 grains, sometimes = 253,33 grains, at others 253,18. But authors do not seem to have agreed in what they meant by *common* water, *rain* water, *pump* water, *spring* water, and *distilled* water; for occasionally they are all confounded, and made to pass for each other; and sufficient notice seems not to have been taken of the temperature to which these weights were assigned. See Martin's *Philosophia Britannica*. Lewis's *Philosophical Commerce of Arts*. Chambers's Dictionary, by Dr. Rees, &c. &c.

The diversity in the result of these two experiments is deserving of notice, and must be explained. It may proceed from two causes, which we will now inquire into. But first it may be observed, that the accuracy in measuring the dimensions of these two bodies, as well as the precision in weighing them, has, I think, been such as to put out of all doubt this part of the experiment. From whence then does this difference arise? Either of two causes may be suspected; *viz.* the pressure of the water against the sides of these two bodies altering their volumes, which, it may be presumed, would have a greater effect on the cube, from its figure, than on the cylinder, and in a direction agreeable to this difference; that is, it would diminish the capacity of the cube more than that of the cylinder, and thus make the apparent weight of a cubic inch less in the experiment of the cube. But also we see, that the cylinder was weighed at a greater depth, by 1,2 inch, than the cube, below the surface of the water. Now, if it be true that water is compressible*, it will become denser, from its weight, at different depths, and this circumstance would act in the same way with that just mentioned; *viz.* would make the apparent weight of a cubic inch less from the experiment of the cube than the cylinder, which we see is the fact.

(§. 25.) In order to dissipate these doubts, I caused a very accurate hollow brass sphere to be made, of about six inches diameter, and of such thickness of metal, *viz.* 0,13 inch, as to be very little heavier than water, and yet of such strength as, together with its form, to resist any probable change of bulk by the pressure of water.

This sphere, which has already been mentioned (§. 10.), was examined in the following manner. The six-inch moveable bar *r* (Plate V. fig. 3.) of the gauge, was compared with the divided scale of inches, fig. 1. The microscopes being adjusted to exactly six inches, or the interval between 26 inches and 32 inches, and the bar placed under them, the excess above 6 inches was found to be as follows, by the micrometer, *n o.*

1st trial. inches.		2d trial, after re-adjustment. inches.	
6+,0055	} thermom. 64°,0	6+,0055	} therm. 64°,0
,0053		,0052	
,0056		,0055	
,0054		,0054	
,0057		,0052	
Mean of the 1st trial = 6 ,00550		6 ,00536	
Mean of the 2d trial = 6 ,00536			

Mean of both, or } length of bar } = 6 ,00543 } in the temperature of 64°.

(§. 26.) The bar was then placed in the rectangular gauge *k l m n*, fig. 2. in the direction *p o*; and the end of the micrometer screw brought to bear against it repeatedly, so as to touch without force, or considerable pressure: and the divisions † cut by the index, on the micrometer plate of the gauge, were as follow:

* See Mr. Canton's experiment in the Phil. Transf. vol. LII.

† Each thread of this screw is = $\frac{1}{101}$ inch, and each revolution of the screw is divided into 100; so that every division on the micrometer plate is = $\frac{1}{10100}$ inch.

Trial 1st. Division on the micrometer.		Trial 2d. Division on the micrometer.		Trial 3d. Division on the micrometer.	
65	} therm. 62°,0	64	} therm. 62°,0	64½	} therm. 62°,3
63		62		65	
66		65		66	
70		63		63	
66		62½		62½	
Mean =	66		63,3 *		64,2

The mean of these three means is 64,5 with the temperature 62°,1.

(§. 27.) The bar was now removed from the gauge, and the sphere put there in its place; and, by means of the three great circles, each of which was divided into 8 equal parts, nine several diameters of the sphere were taken, as follow :

Div. of microm.		Div. of microm.		Div. of microm.		Div. of microm.	
diam. AB	} therm. 62°,½	diam. GH	} therm. 62°,5	diam. CD	} therm. 62°,4	diam. IK	} therm. 62°,5.
40		40		41		44	
50		42		49		46	
47		45		43		47	
42		42		42		45	
46		44		44½		46	
Mean =	45		42,6		43,9		45,6

The above four mean dimensions may be called *equatorial*, viz.

45
42,6
43,9
45,6

The mean of which is

= 44,3

Div. of micr.		Div. of micr.		Div. of micr.	
diam. EF	} therm. 62°,5	diam. 1, 2	} therm. 62°,6	diam. 3, 4	} therm. 62°,8
44		42		40	
46		45		44	
44		45		41	
45		40		40	
45		41		41	
Mean	44,8		42,6		41,1

These three last dimensions, together with the 1st of the preceding set, may be called *meridional*, being in a circle at right angles to the former, viz.

AB = 45
EF = 44,8
1, 2 = 42,6
3, 4 = 41,1

The mean of which is = 43,5 and differs from the former not quite 10000 inch.

* In all these experiments with the gauge, the figures on the micrometer plate increase as the screw goes forward; viz. the higher numbers indicate a less interval or diameter.

In another great circle, 90° from the preceding, comprising the diameters already taken, EF and CD, at the intersection of the two former circles, were taken :

Div. of micr.		Div. of micr.	
The diameter	$\left\{ \begin{array}{c} 41 \\ 44 \\ 44 \\ 41 \\ 40 \end{array} \right\}$	diameter	$\left\{ \begin{array}{c} 40 \\ 41 \\ 42 \\ 40 \\ 42 \end{array} \right\}$
$\alpha \beta$	therm. 63°	$\gamma \delta$	therm. $63^\circ, 1$

The diameters

EF	} taken as before	44,8
CD		43,9
$\alpha \beta$		42,5
$\gamma \delta$		41,0

Mean - - - = 43,0, which is that of another great circle or meridian, at right angles to the former; from whence it will be seen, that not one of the three circles differs from another more than about $\frac{1}{10000}$ inch.

The preceding 9 mean dimensions of the diameter, collected, are

AB = 45,	} the mean of which is = 43,7 in the temperature - $62^\circ, 6$
CD = 43,9	
GH = 42,6	
IK = 45,6	
EF = 44,8	
1,2 = 42,6	
3,4 = 41,1	
$\alpha \beta$ = 42,5	
$\gamma \delta$ = 41,0	

Now the import of the foregoing experiments is this, that when the mean diameter of the sphere is holden between the points of contact of the gauge, near o and p , the index of the micrometer shews - - - - - = 43,7 divis.

but, when the bar r is placed there, it shews - - - - - = 64,5

the difference is - - - - - = 20,8

and by so much is the bar shorter than the diameter of the sphere.

inches.

These divisions, 20,8, are equal to (§. 26.) - - - - - 0,00202

and the length of the bar has already (§. 25.) been found - - - - - = 6,00543

therefore the true diameter of the sphere becomes - - - - - = 6,00745

which quantity I think must be true to within $\frac{1}{10000}$ inch.

(§. 28.) The cube of this diameter, 6,00745 inches \times 5236, as is well known, will give the contents of the sphere in cubic inches, viz. = 113,5194 inches, which must be very near the truth: for, if not, let it be supposed that the inaccuracy in the measurement, or the irregularities in the figure of this sphere, should be such as to amount to $\frac{1}{1000}$ inch, and these so many, without balancing each other, as to produce a spheroidal form, one of whose diame-

ters

ters should exceed the other by $\frac{1}{1000}$ inch; in that case, the error in the assumed solid would not exceed $\frac{1}{1000}$ part of the whole: and this is a position infinitely too extravagant to be admitted, when we recollect, that this diameter has been probably taken to within a tenth part of that error.

(§. 29.) The weight of this sphere, in air and in water, comes next under our consideration; the experiments for which were as follow, made June 12, 1797; the barometer being at 29,74 inches, and the thermometer, in air, at 67°.

Experiment the 1st.

The weight of the sphere in air, the counterpoise, or weight of the scale or cradle, *a b c f* (Plate V. fig. 3.), in which the sphere hung, being allowed for *, so that *this* was the net weight

Troy grains.
= 28722,64

The sphere and scale suspended in water, with it centre 5,6 inches below the surface, and the heat 66°

grains.

= 303,17

Deduct the counterpoise, or weight of the scale, in water, with the same heat of 66°, and same depth † below the surface

= - 253,32

The difference is the net weight of the sphere in water of the temperature 66°, which, deducted from its weight in air

= 49,85

Leaves the weight of a bulk of water = the sphere, in the temperature 66°, and 5,6 inches below the surface

= 28672,79

Experiment the 2d. June 16, 1797.

The barometer being at 30,13 inches, and the thermometer at 68°.

Weight of the sphere, together with the scale, in air

grains.

= 29265,91

Deduct the weight of the scale, or counterpoise, in air

= - 544,03

Remains the total net weight of the sphere in air

= 28721,88

And, to reduce this to the same state of the atmosphere as the preceding observation, viz. 29,74 inches of the barometer, add the correction for 0,39 inch (see table, §. 23.)

= + ,46

Also the correction for 1° of the thermometer

= + ,08

And the net weight of the sphere, in an atmosphere of 29,74 inches, and heat of 67°, becomes

= 28722,42

Weight of the sphere, with its scale, in water, 3,7 inches below the surface, and the thermometer at 66°,1

grains.

= 484,70

* The weight of this scale, with its 3 wires, in air, was = 276,10 grains.

† The sphere having been weighed in the same depth of water that the counterpoise to the scale was determined in, no correction for the greater or less immersion of the scale-wires was here necessary; which however will sometimes be the case. See §. 29. and table II. of correction, §. 23.

From thence deduct the weight of the scale in water	grains.	= 435,09	
The net weight of the scale in water becomes	-	= 49,61	
To which, add the correction for the wires of the scale being immerfed 2,53 inches deeper now, than when its weight in water was determined (fee table, §. 23.)	}	= 0,20	
And the corrected net weight, in water, is	-	-	49,81
Which, deducted from its weight in air, leaves the weight of a bulk of water = the sphere, in temperature 66°,1	}	=	28672,61
Correction for 0°,1 of heat *	-	-	+ ,45
And the true corrected weight of a bulk of water equal to the sphere, reduced to the barometer = 29,74, and thermometer 66°,0, becomes	}	=	28673,06

Experiment the 3d. June 16, 1797.

The true net weight of the sphere in air, reduced to a state of the barome- ter of 29,74 inches, and thermometer 67°, as in last experiment	grains.	=	28722,42
Weight of the sphere, together with its scale, in water, 6,8 inches below the surface; the thermometer at 66°,1	grains.	=	484,20
Deduct the weight of the scale in water	-	-	435,09
The difference is the net weight of the sphere in water, of the temperature 66°,4	}	=	49,11
To which, add the correction for the wires of the scale being immerfed 5,5 inches deeper now, than when its weight in water was determined (fee table, §. 23.)	}	+ ,44	
The corrected net weight, in water, becomes	-	-	= 49,55
Which, deducted from its net weight in air, leaves the weight of a bulk of water = the sphere, and 6 inches below the surface, with the heat of 66°,4	}	=	28672,87
Correction for 0°,4 of heat (fee table, §. 23.)	-	-	= + 1,81
The true corrected weight of a bulk of water = the sphere, in the heat of 66°,0, and with a preffure of the barometer of 29,74 inches, and 6 inches below the surface	}	=	28674,68

* One degree difference of heat in the water will alter the weight of the sphere in water, or the weight of the bulk of water equal to it, = 4,54 grains; so that, by far the greatest source of error, in these experiments, lies in the difficulty of exactly knowing, and preserving, the temperature of the water.

Results

(§. 30.) *Results of the Observations of the Sphere collected.*

Correct weight of a bulk of water = sphere, the barometer
being at 29,74 inches, thermometer 66°, 0.

					At a depth below the surface of the water.	
					grains.	inches.
By the 1st observation	-	-	-	-	28672,79	5,6
2d observation	-	-	-	-	28673,06	3,7
3d observation	-	-	-	-	28674,63	6,8
Mean of all	-	-	-	-	28673,51	5,37

Which, I think, may fairly be presumed to be within 1 part in 50,000 of the truth.

(§. 31.) Now the contents of this sphere having already (§. 28.) been found to be
= 113,519 cubic inches; $\frac{28673,51}{113,519} = 252,587$ grains, will be the weight of a cubic inch

of distilled water, under the circumstances above mentioned, by Mr. Troughton's weights *.

I think it may now be concluded, that the variety in the experiments of the cylinder and the cube, (§. 24.) does not proceed from the different depths † in the water, at which they were made; at least, that the pressure of 3 inches, in perpendicular height of water, does not render that fluid more dense by $\frac{1}{20000}$ part, which may be reckoned an insensible quantity; but that this variety *did* proceed from a difference in the yielding of the sides of the cube and the cylinder. And lastly, I hope it may be trusted, that the weight of a bulk of water = the sphere, has been determined to within $\frac{1}{30000}$ of the whole, and probably to within half that quantity.

(§. 32.) Having then, through the means of Mr. Whitehurst's observations, and of his own instrument, ascertained the length of his proposed standard, in the latitude of London, 113 feet above the level of the sea ‡, under a density of the atmosphere corresponding to 30

* But, as will appear hereafter (§. 41.), these weights are too light, when compared with the standard in the house of commons, by about 1 in 1523,92; the correction, therefore, for this difference, would be = 0,165 grain, to be deducted from

252,587 grains.
— ,165

And the weight of a cubic inch of distilled water, in grains of the parliamentary standard, }
will be } = 252,422

† By means of an alteration and addition to my apparatus, since the experiment above mentioned was made, I have been able to repeat it at greater depths below the surface of the water, viz. when the centre of the sphere was 5 inches, 13 inches, and 21 inches, below, without any appearance of water having a sensible difference of density at different depths. The vessel I used for this purpose was of wood, 32 inches high, and 12 square, containing 16 gallons, with two sides of plate-glass, to admit the light; and the wires by which the sphere were suspended were 45 inches long, and stronger than before, viz. 100 inches of the single wire weighed 24,14 grains; and due allowance was made for the different weight of the scale and wires, in air and water, from actual experiment.

‡ The height, as I have been informed, of the room of Mr. Whitehurst's observations.

inches

inches of the barometer, and 60° of the thermometer, which is full as satisfactory, for all practical purposes, as if it had been done *in vacuo**, which I conceive to be nearly impossible; and, having determined the weight of any given bulk of water, compared with this common measure, I believe it now only remains, to ascertain the proportion of this common measure and weight, to the commonly-received measures and weights of this kingdom.

(§. 33.) It is perfectly true, that if I chose to indulge in fanciful speculation, I might neglect these comparisons, as an unphilosophical condescension to modern convenience, or to ancient practice, and might adopt some more magnificent integer than the *English pound* or *fathom*; such as the *diameter* or *circumference* of the world, &c. &c. and, without much skill in the learned languages, and with little difficulty, I might ape the barbarisms of the present day. But in truth, with much inconvenience, I see no possible good in changing the quantities, the divisions, or the names of things of such constant recurrence in common life; I should, therefore, humbly submit it to the good sense of the people of *these* kingdoms at least, to preserve, with the measures, the language of their forefathers. I would call a yard a yard, and a pound a pound, without any other alteration than what the precision of our own artists may obtain for us, or what the lapse of ages, or the teeth of time, may have required.

(§. 34.) The difference of the length of the two pendulums, from Mr. Whitehurst's observations, appearing to be 59,89358 inches, on Mr. Troughton's scale; and a cubic inch of distilled water, in a known state of the atmosphere, having been found to weigh 252,587 troy grains according to the weights of the same artists, it remains only to determine the proportions of these weights and measures, to those that have been usually, or may be fitly, considered as the standards of this kingdom; and herein a small discrepancy between themselves, in these authoritative standards, will have no influence on the general conclusion I propose to draw; which is, not so much to say what *has* been the standard of Great Britain, as what it *shall* be henceforward, and may be immutably so; and which shall differ but a very small quantity, and that an assignable one, from those that have been in use for two or three hundred years past. By these means, no inconvenience would be produced from change of terms, or subdivisions of parts, or from sensible deviation from ancient practice: all that will be done, will be to render that certain and permanent, which has hitherto been fluctuating, or liable to fluctuation. To give effect and energy to these suggestions, is the province of another power.

* It is perfectly true, that this supposes the experiment to be made with a pendulum similar to Mr. Whitehurst's.

(To be concluded in a future Number.)

III.

On certain useful Properties of the Oxygenated Muriatic Acid. By the Rev. ALEXANDER JOHN FORSyth.*

THE oxygenated muriatic acid has of late been employed with much success in the art of bleaching and discharging colour; but I do not know that any one has hitherto observed that its combinations with alkalies and earths, not only possess the property of discharging colour, but also of brightening and rendering many colours much more intense.

As I hope that information on this subject may be acceptable to some of your readers, I shall give you an account of a few experiments made with the oxymuriate of carbonated potash with an intention not to discharge, but to brighten colours. I preferred this salt, having a considerable quantity of it ready made, and from a few experiments tried with the oxymuriates of carbonate of soda, soda, and lime, I thought they were more apt to discharge the colour, and seldom produced it so clear as the former. Until very lately, I did not think of making any experiments with the oxymuriate of ammoniac, but from these I am led to suppose that it will have a still better effect than the oxymuriate of carbonated potash.

I discovered this property of the oxymuriate of carbonated potash by accident; having dropped a little of the aqueous solution of this salt upon clean writing-paper, when the paper became dry, I was surprised to see my ink write much blacker where the saline solution had spread, than any where else. I then spread some of the oxygenated muriatic acid upon the same paper, but here the ink became lighter coloured than usual. I mixed a little ink in a phial with clear water, and dropped into it a small quantity of the above saline solution; at first, the ink became remarkably black, but whenever it had attained its greatest intensity, the very next drop, in some degree, discharged the colour. After many trials, being persuaded that this saline solution when properly used would improve ink, I filled a quart bottle with the thinnest part of common ink poured off from the sediment, and made it as black as possible with this saline solution. It has now stood without being moved for two months, has let fall no precipitate, writes remarkably black (which to many people would be a very great recommendation), flows easily from the pen, and continues upon the paper much blacker than any patent ink I have ever seen of the same thickness: ink with little gum in it answers best, because the saline solution precipitates from gum a white substance, insoluble in water. It is likewise proper, not to add so much of the saline solution as to make the ink perfectly black all at once, for it improves by standing in the bottle, and there is less danger of any of the colour being discharged. Of the saline solution which I used, two drachms and forty grains was sufficient for a quart bottle of ink, or one-fifth part of the weight of the sulphates of iron and copper used in the composition of the ink to be improved: this proportion occasions no precipitation of the metallic oxids.

* Communicated by the Author.

Having remarked the good effect which the oxygenated muriate of carbonated potash produced upon ink, I next tried if it would improve the colour of the substances commonly used in dying. The aqueous extract of galls by a small addition of this saline solution at first becomes a little white and turbid, a little more immediately renders it as dark coloured as it becomes by long exposure to the action of the air and light; still more of the saline solution discharges the dark colour, and changes it to a kind of yellow: but galls require a much greater quantity of the salt to discharge their colour, than any vegetable colouring substance I have tried. The colours of logwood and weld, are rendered much brighter and more intense by the saline solution: cochineal and archil, are improved: the colours of brazil wood and madder, are little altered. But if a solution of alum be poured into the aqueous extract from any of the abovementioned substances, and suffered to remain at rest until no more precipitate falls down, and if the clear liquor be then poured off, and a little of the saline solution mixed with it, a very abundant and better coloured precipitate immediately subsides. By adding, alternately, solutions of alum and oxygenated muriate of carbonated potash, the whole of the colouring particles may be precipitated from the liquor, and thus a much more abundant and better coloured precipitate procured, than could have been got by using alum alone. Great care must be taken not to add too much of the saline solution at one time, for if any of the colour is discharged, it cannot be recovered. It is likewise proper, to add little of the alum at one time, for too much of it injures the colour, and the first precipitates are generally the best. No heat must be applied after the addition of the saline solution: the colour of all the preparations I could make of indigo, was injured or discharged by being treated with this saline solution.

As yet, I have tried the effect the solution of this salt would have upon the dying of cotton with weld only, and in every instance it has been an improvement where the small pieces of cotton were of the same quality, immersed in the same mordants and colouring baths, for the same length of time, with this difference only, that before one of them was taken out, a little of the saline solution was added to the colouring bath, the colour was brightened by the addition, and about one-third part less weld, produced the same shade as when none of the saline solution was used. The bath must be cold before the saline solution is added, otherwise the colour will be injured; and I imagine that this salt will be employed to most advantage, where the stuff to be dyed is immersed in a cold colouring bath. It certainly has a tendency to extract the colouring particles from the dye, and to brighten them when cold, but in every case injures, and, in some cases, entirely discharges their colour when warm.

I cannot precisely say, to what degree the carbonated potash ought to be saturated with the oxygenated muriatic acid, so as to produce the best possible effect upon colours; but I have found, that the degree of saturation has a powerful influence, and, in many cases, changes the shade of colour completely. Different colours also require different degrees of saturation in the salt, so that a great many experiments must be tried before it can be exactly ascertained what will answer best in every case: for ink, it should be saturated to such a degree, as that

bubbles of air are just beginning to rise in the saline liquor in the operation of making it: or if it is saturated above that point, a little more alkali may be added, or the phial in which it is preserved, may be left open until the air-bubbles cease to rise.

I have observed in all the experiments made with this salt, that it produces the same effect upon most colours as exposure to the air and sun, with this difference only, that its operation is much more rapid and complete: therefore, I have accounted for its effects, by supposing that when a proper quantity of the salt was added, the colouring particles attracted as much of oxygen from the salt, as was sufficient to give them their greatest degree of intensity and brightness; but when too much of the salt was added, so much oxygen united with the colouring particles as to oxydate them completely, and destroy their colour: probably the indigo (the colour of which is more or less discharged by all proportions of this saline solution) is sufficiently oxygenated in the operation of making it.

As in all the experiments tried upon colours, the oxygenated muriate of carbonated potash produced a better effect than the oxygenated muriate of potash; it is probable that the carbon, or carbonic acid, acts a very important part. What renders this more probable is, that charcoal, when used for purifying any substance, but particularly ardent spirits, extract of galls, or water become putrid, by standing too long in glasses, with flowers, &c. if left on the filter, exposed to the air and sun, and not suffered to dry too quickly, assumes a variety of beautiful colours, principally blue, yellow, and purple. When the colour, therefore, is improved by the oxymuriate of carbonated potash, both the oxygen and carbon may unite with the colouring particles, and produce a joint effect.

The oxymuriate of carbonated potash, when not used in excess, precipitates many of the metals from their solvents, with a great deal of colour, and, in many cases, may be advantageously used to detect the presence of a metallic oxyde dissolved in water.

Soap is improved by mixing it with a small proportion of this saline solution; common soap, if well mixed with it in a mortar till it becomes white, answers better for washing the hands, or shaving, than any of the kinds of soap most strongly recommended for these purposes.

All the fat and expressed oils are rendered as white as milk, and partly soluble in water, by mixing them with this saline solution; and they may be restored to their former state, by a small addition of any of the mineral acids, excepting a small quantity of a mucilaginous substance, which remains for a while suspended in the water, and at last collects upon its surface: common whale-oil treated in this way, loses much of its offensive smell, and becomes better.

These are a few of the observations I have lately made upon the effects produced by oxygenated muriate of carbonated potash. Though I have not had an opportunity of making so many experiments, or of considering their results with all that attention which they seem to deserve, yet as many of them are entirely new to me, and promise to be useful to society, I have presumed to lay them before the public, even in their present imperfect state, through the medium of your very excellent publication.

Belhelvie, by Aberdeen, June 2, 1799.

A. I. F.

On

IV.

On the Management of Fire, particularly with Regard to the Construction of Boilers.

By BENJAMIN, COUNT OF RUMFORD*.

THE sixth of Count Rumford's experimental essays is divided into six chapters. In the first, the curious and interesting nature of the subject is displayed, and the importance of œconomy in fuel, is strongly impressed. It is stated, in general terms, that no less than seven-eighths of the heat which might be produced and usefully applied from any given quantity of fuel is lost in the common methods of application. But not to leave the subject to rest upon general induction, the author proceeds to relate various interesting experiments which shew the difference of effect with the same vessel. Accordingly as the fire was applied in the common method without confining it, or according to the improved methods of the Count, the proportions of fuel required to raise the temperature of equal quantities of water, differed incomparably more than common observers might have been led to expect. In this first chapter, the Count enters into the midst of his subject, by giving a description of the arrangements in the kitchen of the house of industry at Munich; the construction and judicious provisions of the fire-place, the form of the vessels and their covers, and the saving of heat, by causing the steam, and even the smoke, to perform useful offices. He states the advantages of wooden boilers in many cases, over those of metal, and after an interesting detail of particulars, we arrive at the valuable fact, that the quantity of fuel consumed in this establishment is only one-tenth part of what is required to produce like proportional effects in private families. This narration is followed by accounts of some of the provisions in the construction of the best fire-places, and the figure of the boilers, and is concluded by a confession, which does honour to the understanding and the principles of the Author: namely, that the want of method in this chapter, is intentional; that he was desirous rather of writing a useful book, than a splendid performance; for which purpose he rather chose to decoy the reader into a situation where he should have an inviting view of the whole prospect; than to present a regular series of elementary principles to the multitude, who have neither time nor patience to labour through so abstruse a subject.

The second chapter accordingly presents us with a popular and perspicuous account of the generation of heat in the combustion of fuel. Whether heat be matter or motion, forms no part of the present discussion; the laws of its action constitute the object of practical research. Whether the heat be afforded exclusively by the air, which is decomposed in combustion, or not, it is evident that the quantity will be greater or less, accordingly as the combustion or

* In consequence of the letter received from my correspondent W. see p. 86 of the present volume, I applied to Count Rumford, for permission to copy the engravings which constitute plate VIII. of the present number, which he granted with the utmost readiness. As the whole of the present article consists of abridged matter from his sixth essay, or such remarks and facts as I have received from him in conversation, I have thought it most proper to subjoin his name to the title, though I have, for the sake of conciseness, used my own language, and have spoken of him in the third person.

decomposition of the fuel is more complete. This greatly depends on the stream of air, which passes through the burning fuel; that is to say, upon the velocity of this stream, which must be definite, in order to produce the greatest possible effect with fuel of any particular kind. Without air there is no combustion; a small portion of air will maintain the combustion but feebly; a greater quantity will give intensity to the heat, and rapidly decompose the air and the fuel; a still greater quantity will carry off more heat than it can generate, that is to say, it will blow the fire out*. The current of air, by which a fire is excited, may be made to flow by mechanical means, such as bellows; or by the statical effect of its own expansion. This last is the common process operated by chimnies; and it is of great practical advantage to be able to regulate this current. If we suppose the dimensions of the fire-place, and the height of the chimney, to be such as are best calculated to produce a rapid combustion of the fuel, it may, in many instances, be desirable to moderate that combustion, by diminishing the quantity of air; this is done by perfectly closing the door or aperture, through which fuel is conveyed to the fire-place, and by securing a command of the other two passages, namely, that of the ash-hole under the grate and that of the chimney, by proper registers, which may be opened to any required extent, or closed at pleasure. By these arrangements, the combustion is governed by the operator, and may be entirely stopped by closing both registers. The Author observes, that nearly the same effects as those of the chimney register may be produced, by causing the smoke to descend several feet after it has quitted the fire-place before it ascends again; and from a late conversation with him, I understand that this descent is of the utmost importance and value in its effect. As the cooled air or vapour always occupies the lowest place, it must follow, that no part of the stream can pass up the chimney, till it has imparted so much of its heat, as to cause it to descend to the bottom; whereas, in the ordinary form of chimnies, it is the most heated part which flies immediately and rapidly up the chimney. Many fire-places have been greatly improved by this simple addition. The size of the fire-place, as well as of the fuel, are both of considerable importance. The air, which in common constructions is suffered to pass over the fire, is a thief which carries off heat without assisting the combustion: a large grate, partly covered with fuel, produces a similar effect by admitting air through the vicinities, which simply becomes heated by robbing the boiler, and the rest of the apparatus with which it may come in contact. The Count recommends a grate in the form of a segment of a sphere, which affords the advantage of the fuel rolling to the lowest part, as it becomes smaller; and he gives the form of a cone to the passage beneath the grate, converging downwards, till the diameter becomes only one third of that of the grate. Instead of the grate itself, he has introduced earthen pans perforated with holes to admit the air, and thinks they answer even better than the grates themselves.

The heat generated in the combustion of fuel, manifests itself in two ways; namely, in the hot vapour which rises from the fire, and in the rays which pass off in straight lines in all directions. It is not known what proportion this radiant heat, as it has been called, bears to that

* Philosophical Journal, I. 515

which passes off in flame and heated vapour. It seems probable that it may vary according to the volatility of the combustible matter and other circumstances; but, in general, it seems that the quantity of the former is much less than that of the latter. Open fires warm apartments by their radiant heat only, all the heat of the vapour being entirely lost up the chimney. It is a remarkable fact, that radiant heat is emitted in great abundance by all bodies which are capable of ignition, whether they be solid or fluid, combustible or incombustible.

The means of confining heat and directing its operations, constitute the object of the third chapter. It is well known that heat passes more speedily through some bodies than others. The useful applications of this property are seen in a variety of instances; among which, the wooden handle of a tea-pot, and the cloth made use of in handling hot iron, are familiar examples. The Author mentions some others, and remarks that the conducting powers of no two bodies, with regard to heat, are exactly alike.

To confine heat, is nothing more than to prevent its escape out of the heated body, by surrounding that body with a covering, through which it cannot readily pass. If a covering could be found perfectly impervious to heat, there seems no reason why the heated body, thus defended, should not continue hot for ever; but it is not probable that any such body exists.

All metals are remarkably good conductors. Wood, and, in general, all light spongy bodies, or such as contain fluids in their interstices, are bad conductors*. Mercury, water, and all fluids, are conductors by circulation, but not perceptibly so in any other way. Air and the elastic fluids are very slow conductors, even when permitted to circulate; probably on account of their very small mass: pulverised bodies conduct worse than the same bodies in the aggregate. Very dry powder of charcoal is one of the best we know; but common air is the substance employed by nature to confine heat, and is certainly the best which can be used.

The warmth of the fur of beasts, is undoubtedly owing to the air entangled or confined in their interstices†. Double windows and walls, so useful in cold countries to confine heat, and in hot climates to prevent its admission, operate in the same way‡. The Author proceeds to give a short account of his experiments formerly made on these subjects, and shews by a curious experiment, that steam is not a conductor of heat in any respect, except by the intestine motion of its parts. The same experiment is applied to shew that heated air and vapour, are also non-conductors, and may be used with the greatest advantage in our operations for confining heat.

These interesting speculations lead in the fourth chapter to an enquiry how the heat is communicated from flame to other bodies. If the conducting quality of bodies be not altered by any increase of temperature which does not change their chemical combination, it will be proper to consider flame as a hot wind. It is necessary, in order that a fluid should receive or impart heat, that all its parts should severally come into contact with the body which gives or receives. Hence a hot body immersed in air is not cooled except so far as the air has motion; and the

* Philosophical Journal. I. 289-295

† Philosophical Transactions, 1792.

‡ Double walls and windows are scarcely used in hot countries. I have seen light double structures of bamboo in India. N.

effects of wind, or a blast, becomes active in carrying off the heat. If flame be merely a hot wind, it will follow that the stream of a blow-pipe, when used to impel the flame and not to excite the fuel, must probably act by impelling the ignited vapour with greater force and quantity against bodies, and no otherwise; and, consequently, that carbonic acid gas, or any other elastic fluid, will have the same effect under these circumstances as oxygen. The Count found this to be the case with those very fluids, as well as with atmospheric air, urging the flame by the blow-pipe, and used to fuse the end of a stick of glass. It is probable that this result may require to be modified, and the experiments varied, in order to reconcile it with the effects produced by the use of oxygen, in fusing flint and other incombustible bodies by Lavoisier, Erhman, and others; but there is no doubt that it is strikingly conclusive with regard to the practical object, to which our author applies his reasoning. *The boiler to be heated must not only expose as large a surface as possible to the flame, and hot vapour; but it must be of such a form as to cause the flame which embraces it to impinge against it with force, to break against it, and to play over its surface in eddies and whirlpools.* It is therefore against the bottom, and not the sides, that the principal efforts must be directed.

The next chapter, V. presents an account of experiments with fire-places and boilers of various forms and dimensions: a philosophical and practical course of unparalleled felicity, whether we consider the ability and conduct of the director, or the means afforded to perform them. In order to obtain a result capable of comparison, it is taken for granted that equal quantities of fuel similarly expended, will raise the temperature of water through the same number of degrees. Whence, by knowing the original temperature and quantity of water, together with that of the fuel expended to raise it to the boiling temperature, the result may be expressed by stating the quantity of water at 32°, which would have been raised 180° degrees by 1lb. of the fuel in that furnace*. The Count calls this the *precise result*, and sometimes adds the quantity of water which might have been kept boiling one hour by 1lb. of the fuel; which last, as he remarks, cannot be very exact.

From the indispensable motive of brevity, it becomes necessary to refer the reader to the essay itself for the particulars of the respective constructions, and the useful consequences to which they point. I shall, therefore, mention some of the leading facts, and then give the description of the furnace and boiler delineated in the plate.

The fuel made use of was wood; and it is a fact of no inconsiderable value, that pine wood, which, weight for weight, costs, in most places, only half the price of beech, affords more heat in its combustion. It was not found, as might be expected, that the saving of fuel was greater the larger the scale of operation; but, on the contrary, the experiments shewed that there is a maximum of effect with single fire-places, which will be departed from when the quantities of liquid either exceed or fall short of a certain definite quantity. The causes of this are pointed

* In the form of a rule. Multiply the quantity of water by the number expressing the degrees actually raised; multiply the number of pounds of fuel expended by 180. Divide the first product by the latter, and the quotient will express the water which would have been raised 180° by 1 lb. of the fuel. N.

out, namely, that flame will act more effectually the greater the surface; and this is proportionally greatest in small furnaces: and, secondly, that the cooling effect of the masonry and apparatus will be greater the greater its mass; and this also is proportionally greater in small furnaces. The exact size to produce the greatest effect is not easy to be ascertained; but supposing this to be done, it will follow, as the Count has remarked to me, that in all establishments of considerable magnitude, it will be better on this and many other accounts, to heat the vessel by a number of smaller fires, instead of one large one.

In the construction hereafter to be described, the flame was at first made to circulate round the sides of the boiler; but by subsequent experiments, in which the circulation through those side flues was prevented (the hot vapour being still suffered to enter them), it was found that the effect was accelerated by the speedier draft of the chimney, without any greater expence of fuel.

In every furnace there seems to be a rate of working, at which the heat is more cheaply produced than at any other; that is to say, if there be too little fuel in the fire-place at once, the process will be longer, and the loss, by the conducting power of the apparatus, more considerable: if there be too much, the business will be performed more speedily, but at a greater expence, chiefly from the escape of heated vapour by the chimney. In some cases, it may be most profitable to save time; in others, fuel. In the experiments, no. 31 and 32, it was found, that to abridge the time one-third, there was an additional consumption of about one-eighth more fuel.

The progressive improvements in the saving of fuel, as summed up at page 121 of the essay, are very curious. With a common open fire, carefully managed, the precise result was 1.11. or 1lb. of fuel raised $1\frac{1}{8}$ lb. of water 180° . But in the experiment, no. 20. with the improved fire-place and boiler, the result was 20.1; or $20\frac{1}{5}$ lbs of water, were boiled with 1lb. of wood. So that it appears that the latter process was eighteen times more economical than the former, and that seventeen parts out of eighteen of the fuel were consumed to no purpose in the first experiment. With regard to the absolute quantity of heat, the author deduces from the experiments related in Dr. Crawford's treatise, that 1lb. of charcoal would cause 57.6 lbs. of water to boil, if none of the heat was suffered to escape. And by Lavoisier's experiments, the quantities of heat generated by equal weights of charcoal and dry oak are as 1089 to 600; and the Count takes it to be nearly true, that equal weights of oak and of dry pine wood will afford equal quantities of heat. These positions afford the result, that 31.74 lbs. of water would have been brought from the freezing to the boiling heat by the combustion of 1lb. of his fuel. It follows, therefore, that in the method of boiling over common open fire, nearly 28 parts out of 29 of the heat are lost; and that in the Count's experiments the absolute loss amounted to about one-third. When the generation and loss of steam, the conducting power of the materials, and the quantity of heat which must necessarily be suffered to pass up the chimney in order to produce a draft, are considered, it may be doubted whether the economy of fuel can be carried much farther.

As the fuel of this country consists of coal and coke, and not wood, it seems requisite to state their comparative powers of affording heat by combustion. The experiments of Lavoisier,

quoted by the author, give the following proportions *. Equal quantities of heat are produced by the combustion of

403 lbs of cokes	} or in measure {	17 of cokes.
600 — of pitcoal		10 of pitcoal.
600 — of charcoal		40 of charcoal.
1089 — of oak		33 of oak.

If pitcoal had been used instead of pine-wood, the number of pounds of water made to boil by 1lb. of the fuel would have been 36,3 instead of 20,1; and as, from the experiments of Watt, it is established, that nearly five and a quarter times the quantity of heat is required to convert boiling water into steam as would have raised the same water from the freezing to the boiling point, it will follow, that the heat generated in the combustion of one pound of pit-coal ought to afford very nearly 7lbs. of steam.

In all the experiments made on a very large scale, with brew-house boilers, rather more than one-half the heat actually produced found its way up the chimney and was lost.

The sixth chapter of this essay contains a short account of a number of kitchens, public and private, and fire-places for various uses, which have been constructed under the direction of the author, in different places: of these I must necessarily forbear to speak at present.

Fig. 1. Plate VIII. is a front view of the new boiler of the brew-house called Neuheusel (belonging to the Elector of Bavaria), or rather of its fire-place and cover (the boiler being concealed in the brick-work). The inside door of the fire-place is here represented shut; and, in order that it might appear, the outside door is taken off its hinges, and is not shown. The two vaulted galleries, A, B, in the solid mass of the brick-work, on the right and left of the fire-place (which were made to save bricks), serve for holding fire-wood. The partition walls of the fire-place and the different flues, as also a section of the boiler, are represented by dotted lines. The small circular hole on the left of the fire-place door is the glazed window opening into the fire-place, by which the burning fuel may be most advantageously seen, without disturbing the current by any opening of doors. *a b* is the wooden curb of the boiler: *c d*, a platform on which the men stand when they work in emptying the boiler, &c.: *e f* is a platform which serves as a passage from one side of the boiler to the other. This platform, which is about 18 inches wide, is 12 inches higher than the other platforms, in order that the openings *g* and *h*, into the flues, may remain free. These openings, which are opened only occasionally, that is to say, when the flues want cleaning, are kept closed by double brick-walls. These walls are expressed in the following figure.

Fig. 2. is an horizontal section of the fire-place at a level with the bottom of the boiler. *a, a*, *a, a*, are four openings, by which the flues, which in the first arrangement of this fire-place went round the outside of the boiler, were occasionally cleaned: *b* is the canal by which the smoke went off into the chimney. The entrance into the fire-place, and the conical perforation in the wall

* These experiments, which were communicated to Count Rumford by Mr. Kirwan, are to be found in the Memoirs of the Paris Academy; I think for the year 1781.

of the fire-place, which serves as a window for observing the fire, are marked by dotted lines. The position of the outside door of the fire-place is marked by a dotted line, *c d*. The circular dishing-grate is seen in its place; and the walls of the flues under the boiler are all seen. The crooked arrows in the flues show the direction of the flame.

Fig. 3. is a vertical section of the boiler represented in the foregoing fig. 1. This section is taken through the middle of the boiler of the fire-place, and of the cover of the boiler. A is the ash-pit, with a section of its register door. B is the fire-place, and its circular dishing-grate. C is the entrance by which the fuel is introduced, with sections of its two doors. D is a space left void to save bricks. E is the boiler, and F its wooden cover. *m* is the steam chimney, which is furnished with a damper. R R is the vertical wall of the house against which the brickwork in which the boiler is fixed is placed. *a b* is the curb of timber in which the boiler is set. The manner in which the cover of the boiler is constructed, as well as its form, and the door and windows which belong to it, are all seen distinctly in this figure. /

Fig. 4. is an horizontal section of this fire-place taken on a level with the bottom of the flue which goes round the outside of the boiler, in which flue, before the fire-place was altered, the flame circulated. The flues under the boiler are, in this figure, indicated by dotted lines.

Fig. 5. is an horizontal section of the fire-place of the brew-house boiler, at a level with the top of the flues under the boiler, *after the flue round the outside of the boiler had been stopped up*, or rather the flame prevented from circulating in it. This figure shows the actual state of the fire-place at the present time. The crooked arrows show the direction of the flame in the flues.—*a, b*, are the two canals (each of which is furnished with a damper) by which the smoke goes off into the chimney;—and *c, c, c, c, c, c*, are six small openings communicating with the flues, by which the flame and hot vapour can pass up into the cavity on the outside of the boiler, which formerly served as a flue.

Fig. 6. is a front view of the ash-pit door of this brew-house fire-place, with its register. This door is closed by means of a latch of a particular construction, which is shown in the figure.

Fig. 7. is the door without its register;—and

Fig. 24. the circular plate of the register represented alone.

In constructing these register doors, and in general all iron doors for fire-places, great and small, the door should never be shut in a rabbet, or groove, in the frame, but should merely *shut down on the front edge of the frame*, which edge, by grinding it on the flat surface of a large flat stone, should be made quite level to receive it. If this be done, and if the plate of iron which constitutes the door be made quite flat, and if it be properly fixed on its hinges, the door will always shut with facility and close the opening with precision, notwithstanding the effects of the expansion of the metal by heat; but this cannot be the case when the doors of fire-places are fitted in grooves and rabbets. Where the heat is very intense, the frame of the door should be made of fire-stone; and that part of the door which is exposed naked to the fire should be covered either with a fit piece of fire-stone, fastened to it with clamps of iron, or a sufficient number of strong nails, with long necks and flat heads, or of staples, being driven into that side of the plate of iron which forms the door which is exposed, should be covered with a body about

two inches thick of strong clay, mixed with a due portion of coarse powder of broken crucibles, which mass will be held in its place by the heads of the nails, and by the projecting staples. This mass being put on wet, and gently dried, the cracks being carefully filled up as they appear, and the whole well beaten together into a solid mass, will, when properly burned on by the heat of the fire, form a covering for the door, which will effectually defend it from all injury from the fire; and the door, so defended, will last ten times longer than it would last without this defence. The inside doors of the two brew-house fire-places which the Count fitted up at Munich are both defended from the heat in this manner; and the contrivance, which has answered perfectly all that was expected from it, has not been found to be attended with any inconvenience whatever.

I have lately seen the patterns at Count Rumford's for casting a *regulating door* of his invention, which possesses the advantages of simplicity, cheapness, and effect, in a very striking degree. It has neither hinges nor sliding groove: it closes with utmost precision; and from the very small quantity of work it requires to finish it, the price ought not to exceed two or three shillings. I am not at liberty to anticipate the inventor, who will communicate it to the public as soon as this can be most effectually done, namely, by directing them to a place where they may see what they are to imitate.

V.

Extraction of Sugar from Carrots, &c.—Experiments on Barytes and Strontites.

Extract of a Letter from Mr. WILLIAM HENRY of Manchester, to the Editor, dated June 20th, 1799.

MY friend, Doctor Peschier of Geneva, who is now at Vienna, sends me the following information respecting the extraction of sugar from carrots, &c. I subjoin also some experiments of my own, on the sulphate and Prussiate of barytic and strontian earths.

“ Professor Jacquin rasped and expressed 40lbs. of roots of carrots (*daucus carota*). The juice, which was pretty clear, and of an agreeable taste, was evaporated to the consistence of syrup; clarified with some albumen of egg, after being mixed with some ounces of quick-lime; and filtrated. After cooling a few days, it contained, in the bottom of the vessel, small crystals, which, when washed in cold water, were white and diaphanous. To these succeeded others of a yellow colour. The whole of the crystals amounted to 12 oz.; they tasted exactly like the purest sugar, and preserve their dryness to this day. There remained about 4lb. of a brown sugar perfectly agreeable to the taste.

“ Second experiment. Three roots of *beta cyclo altissima*, weighing, after having been peeled, 4lb. were pounded and expressed. The juice was thick, mucilaginous, and sweet, but disagreeably mixed with the taste of turnips. It was evaporated, according to the method of Mr. Achard of Berlin, without any addition, and skimmed at intervals. This scum was produced

duced by the albumen, which, you know, is to be found in most vegetables, and coagulates at a certain degree of heat. When thus cleared, it had completely lost its sharpness, but after standing at rest for some days, in a cool place, it shewed no tendency to crystallize. It was then evaporated in a water-bath, and afforded a paste of a light brown colour; that deliquesced in a short time, and resumed the consistence of a thick syrup, to the weight of $\frac{1}{2}$ lb.

“ Third experiment. 11 lb. of the same roots were expressed, as in the second experiment, but were purified as in the first. They gave 3 lb of a clear syrup, which afforded 3 oz. of small crystals of sugar, of a pleasant taste. The remaining syrup was brown and very sweet. It is to be observed, that as these experiments were made in the spring, the roots had already begun to shoot, and contained rather more mucilage than sugar. It is necessary, also, to inform you, that the little plate, affixed to the work of M. Achard, represents the *beta vulgaris*, while the description is that of *beta cycla abtissima*, which is very different. On the whole, the extraction of sugar from the carrot and turnip, on a larger scale, promises to become an object of profitable speculation.

“ It has been discovered lately, that the water, remaining after obtaining starch, contains a large quantity of sugar.”

I know not whether you will think the following experiments on barytes and strontites worthy of a place in your journal.

The analysis of the native sulphate of strontites from the neighbourhood of Bristol, which has been so well executed by Mr. Clayfield, establishes, beyond all controversy, the nature of that compound. If any testimony were required to the accuracy of Mr. C.’s experiments, I might state, that early last winter I read to the Literary and Philosophical Society of this town, an analysis of the same substance, from which I assigned as its components, in 100 grains, $41\frac{1}{2}$ of sulphuric acid, $58\frac{1}{4}$ pure strontites, and one-fourth of a grain of water, besides a scarcely notable portion of iron. These proportions do not differ more than a quarter of a grain from those of Mr. C.

If Mr. Clayfield’s analysis be imperfect in any respect, I think it is in the deficiency of proof that the mineral under examination contains no portion whatsoever of barytes. To ascertain the presence of the lastmentioned earth, Mr. C. poured on the precipitate obtained by boiling the native sulphate of strontites with carbonate of potash, “ a quantity of muriatic acid, sufficient to dissolve only a few grains of the earth. Had any barytes been present, it would have been taken up in preference to the strontites, from its superior affinity for the acid; the solution, however, after digestion for several hours, still crystallized in needles, and afforded a copious precipitate to barytic lime-water.” (Phil. Journ. III. 38.) It will be found, however, that on the addition of dilute muriatic acid to a mixture of carbonates of barytes and strontites, a portion of each earth is dissolved, as might indeed be expected from several known facts respecting chemical affinity, which shew that facility of combination and strength of affinity by no means keep pace together. At a certain point of concentration, the muriatic acid even dissolves strontites in preference to barytes, on account of the greater solubility of the resulting compound.

compound.—The method by which I examined whether the mineral under examination contained barytes, was as follows.

1. The spar was decomposed by digestion with carbonate of potash, as in Mr. Clayfield's experiments. The substance thus obtained, consisting of the earthy part of the mineral combined with carbonic acid, I shall call the precipitated earth.

2. Nitric acid was gradually diluted with water, till it became capable of dissolving strontites but not barytes; for to dissolve the latter, much further dilution is necessary. The precipitated earth was wholly dissolved by this acid, which would have left undissolved any portion of barytes.

3. Barytes, in a pure and caustic state, according to Dr. Hope (Edinb. Transact. vol. iv.), precipitates strontites and all other earths from muriatic acid. I exposed, therefore, to an intensely strong fire, a portion of the precipitated earth; and thus deprived it of its carbonic acid so completely, that it readily dissolved in hot water, and crystallized on cooling. Now, if the peculiar mineral under examination contained any barytes, this watery solution of its pure earthy part, on being added to a solution of the precipitated earth in muriatic acid, would occasion a separation of strontites. On making the experiment, however, no precipitation took place, which shows that the watery solution contained no barytes, but consisted of pure strontites only. The same fact also proves that the muriatic solution contained neither lime, magnesia, alumine, nor any of the metals, any one of which would have been precipitated by the strontitic water.

The following experiments are, perhaps, worth relating, as they furnish additional proof that barytes and strontites are really distinct and peculiar earths.

Dr. Hope, and I believe the generality of writers on this subject, assert that barytes (in contradistinction to strontites) is precipitated from muriatic acid by the Prussiate of potash. But Mr. Kirwan, on grounds which he has not stated, testifies the contrary. "Most earths," he observes (Elements of Min. I. 3.), "are soluble in some acid or another, and many in all acids; the Prussian alkali can precipitate none of them from these solutions; whereas it precipitates all metallic substances except platina from their acid solvents. This forms a distinct line of separation between earths and metals. The exception, formerly made in favour of barytic earth, is now found to have arisen from a mistake." As the decision of this question is of some importance, from its influence on mineral analysis, I determined to satisfy myself of the truth by the proper experiments; the relation of barytes and strontites to the Prussic acid, not having been an object of attention either with Dr. Hope, M. Klaproth, or Mess. Pelletier, Fourcroy, and Vauquelin.

1. *Relation of Barytes to the Prussic Acid.*

To a solution of caustic crystallized barytes in hot water, Prussiate of iron, which had been well washed with boiling distilled water, was gradually added, till it ceased to be discoloured. The solution of barytes acquired a yellow tinge, resembling that of Prussiated potash. It was gently evaporated, and when cold, a number of crystals had formed, which were slightly tinged by iron, and appeared to be very minute rhomboidal parallelopipeds.

These

These crystals were sparingly soluble in water, 4 oz. of which, at 65° Fahrenheit, took up barely one grain; and of boiling water, each ounce dissolved only between five and six grains. The watery solution gave a precipitate of sulphate of barytes, on adding sulphuric acid, and of Prussiated iron, with the solutions of that metal. In a low red heat, the salt was soon changed into a black mass, consisting of carbonate of barytes and charcoal. The crystals dissolved readily in diluted nitric and muriatic acids, apparently without decomposition, for the solutions precipitated salts of iron. They were decomposed by carbonate and sulphate of potash, a double exchange of principles ensuing.

Part of the precipitate, observed on adding the Prussiate of potash to muriated barytes, is doubtless often occasioned by the presence of sulphate of potash in the Prussiated alkali; an adulteration, which it is difficult entirely to avoid; but that a double elective affinity is exerted between the two salts, clearly appears on examining the precipitate. After being repeatedly washed with distilled water, muriatic acid will be found to dissolve the greater part of it; and the solution of the precipitate, in this acid, betrays the presence of Prussiate of barytes, on applying the proper tests. It may, therefore, be received as an established fact, that an insoluble, or difficultly soluble Prussiate of barytes is formed, on adding Prussiate of potash to the muriated earth, a property in which barytes differs from all other earthy bodies, and resembles the metals: the simple affinity of potash for Prussic acid is superior to that of barytes; for caustic barytes does not precipitate a perfectly pure Prussiate of potash. Barytes attracts Prussic acid more powerfully than lime, for Prussiate of lime is decomposed by pure barytes; the order of these affinities, I believe, has not before been ascertained.

2. Relation of Strontites to the Prussic Acid.

A solution of pure crystallised strontites deprives Prussiate of iron of its tinging acid; but the solution of Prussiated strontites is much less disposed to crystallise than the homologous barytic salt. Indeed I have repeatedly evaporated solutions of Prussiated strontites very low, without having been able to obtain any crystals. I therefore expelled from one portion the whole of the water by a gentle heat; the dry mass thus obtained dissolved readily in water at 65°, an ounce of which took up 120 grains, and even then did not appear to be saturated: but having no more of the salt, I was prevented from ascertaining its exact solubility. The dry salt does not deliquesce: in its other properties, it resembles Prussiated barytes.

A solution of caustic strontites does not precipitate Prussiated lime, as barytes does; still, however, a decomposition probably takes place, but is prevented from being apparent by the solubility of the new compound. This may, also, be the case, when muriated strontites is added to Prussiate of potash, or of lime.

Pure barytes, when added to Prussiated strontites, occasions a precipitate, but a much less abundant one, than might be expected; hence it is probable that the affinities of these two earths for Prussic acid are nearly equal in strength, as Dr. Hope has shewn they are, with respect to carbonic acid.

Dr. Pefchier acquaints me, that the sulphate of strontites abounds in Syria and in Hungary, but not the carbonate.

Concerning

VI.

Concerning those perpetual Motions which are producible in Machines, by the Rise and Fall of the Barometer, or the Thermometrical Variations of the Dimensions of Bodies. W. N.

(Concluded from p. 128.)

SUPPOSE this apparatus to be put together at a certain temperature in the day-time; and that in the night the temperature becomes colder; in this case, the curvature of all the bars will diminish, and the distance between A and B will be increased by the action of the intermediate spring: but as the plate A is prevented by the click C from receding, the plate B will be pushed forward, and the interior cylinder will gather a certain number of its teeth upon the click D. The next day, when the temperature again rises, all the expansion bars will bend, and the space between A and B will be diminished; this, however, cannot happen by the motion of B, which is held fast by the click D. The external part will, consequently, be now carried forward, and will act upon the apparatus E: a second lowering of the temperature, by whatever cause, will occasion the interior part again to advance; and, in this manner, the accumulations of force may be incessantly reiterated.

Experience must determine how far the properties of these compound bars may be changed in the course of time*. It seems probable, that the mere changes communicated by the atmosphere, could scarcely produce any sensible effect; and whether this effect would be detrimental to the general result, may also be questioned. Considerations of this nature, lead to the enquiry, whether our object may not, with equal facility, be obtained by the direct push or pull of bars of metal, as in the gridiron pendulum, or that of Ellicott†.

If a succession of bars of brass were disposed round a cylindrical face of less expansible metal, so as to form an helical line from the one end to the other; or, otherwise, if we suppose a brass clock chain, with a right-lined edge, to be wrapped round such a cylinder; or again, if, instead of the cylinder, we suppose the chain to pass over a succession of rollers, whether disposed in a cylindrical system, or according to the form of a pulley, the result will be the same; that is to say, the chain will contract and expand about the ten-thousandth part of its length, for every ten degrees of Fahrenheit's thermometer. In the way of rough estimate, let us, therefore, assume a cylinder of cast-iron, one foot in diameter, and one foot in length, having a groove turned in its surface, like a screw, with twelve turns in the inch, for the purpose of lodging a system of friction rollers to receive the brass chain, wrapped round it. Such a chain‡, consisting of 144 turns, would measure 450 feet §, and would contract nearly 0.54 inches for every 10 degrees, or one-twentieth of an inch for each degree of change of temperature; but as the cylinder itself contracts, the whole effect will be somewhat less than half that quantity; that is to say, each degree of the thermometer will be one-fortieth of an inch.

* Philos. Journal, I. 62.

† Ibid, p. 59, 60.

‡ And would cost about 25l.

§ For pyrometrical data, see Philos. Journal, I. 58.

The philosophical world is aware, that hygrometers have been made, on this principle, with cat-gut, hair, whalebone, and other materials. It seems probable, that the first of these substances would exert considerable force as a first-mover, but it would scarcely prove durable; and what is still worse, the variations of moisture in the atmosphere are little suited to operate upon machinery preserved in a case in an apartment.

If the increase of the space moved through by the expansion of metals upon the principle of Ellicott's pendulum, should be adopted instead of the compound bars in fig. 4, the project of fig. 5, may be followed. A bar of steel, A K, is fixed beside a bar of brass B I, the joinings, I K, being inflexible; but those at A, B, in the lever A F, being jointed, the difference of expansion between the two metals will be magnified at F, in the proportion of A F to A B. The lines, L C, G D E, represent a second combination of the same kind, in which the point G will have a similar and equal motion to that of F, but the bar E L, being prolonged to F, so as to bear upon the lever E F, the whole of the second combination will be pushed forward by the expansion of the first; on account of which, the motion of G will, in fact, be double that of F; by the addition of a third combination, the motion will be tripled, &c. A sufficient number of these, properly placed in the cavity, between the two cylinders, fig. 4, would afford the same consequences; but it may be doubted, whether any contrivance of this last kind could afford the same power in as small a space as that occupied by the compound bars.

I shall now proceed to form an estimate of the quantities of force communicated by these several contrivances.

The apparatus, fig. 1, plate VI. or barometrical clock, is driven by a force which may be estimated in its annual quantity from the sum of the deviations of the barometer taken from a Meteorological Journal, such as that in the Philosophical Transactions; together with the quantity of mercury so moved, which may be derived from the dimensions of the surfaces in the tube and basin. From these variations, severally, must be taken the quantity of what mechanics call lost time, or the ineffectual movement between the direct and retrograde actions on the machinery. The whole power will be measured by the entire column of variation, supposed to descend through half its height; for this will be the case when a perpendicular tube empties itself by the descent of any fluid contained in it. I have not taken the trouble to collect these elements; but it may easily be imagined, that the sum of all the variations during the year, would amount to no great quantity. I understand, from the ingenious constructor of this apparatus, that the accumulated power was not sufficient to allow the clock to be maintained by a force equal to that which drives a common watch, namely, six ounces, with the daily fall of twelve inches.

In the investigation of the power of expansion, in fig. 4, a variety of curious objects of physical and mathematical research offer themselves to our consideration. In a former part of this Journal*, it has been shewn, that the curvature assumed by a straight compound bar, having each of its parts uniformly thick, will be circular; whence it follows, from the nature

* I. 62.

of versed sines of small arcs, that the distance AB , fig. 2, will, *ceteris paribus*, be as the square of CA ; and it should seem, as if a considerable advantage might be derived from using the whole length of the bar, as in that figure, 2, instead of the two half-lengths in fig. 3. But it must be considered, that the effect of hammer-hardening the lower part, and wire-drawing the upper, of the compound bar CA , is twice as great at C , fig. 2, as it is at C , in fig. 3, and is shewn in the greater spring, or yielding of the parts; and that the action at A , in this figure, is doubled at the opposite extremity of the bar:—so that upon the whole, the action at A , on account of the short lever AC , fig. 3, being twice as powerful as that at A , in fig. 2, and being exerted through the space AB , fig. 3, of one-fourth part of the space AB , in fig. 2, will be half the action at the end of fig. 2. But as both extremities of the bar are made to act in fig. 3, the whole of its action will be precisely equal to that in fig. 2. The combination, fig. 3, appears, therefore, to be preferable on account of its convenient figure only.

It may also be questioned, whether these bars should be made extremely thin, or the contrary. If they be very thin, the effect of the reaction being equivalent to a pull, or push endways upon the bar, which is greater than the reaction itself, in the proportion of the length of the bar to its half thickness, it may easily be imagined, that the texture and cohesion will be most strongly affected; but, on the contrary, if the bar be very thick, the effect from change of temperature may resolve itself intirely into an action upon the parts near the contiguous surfaces without producing any flexure at all. It appears, therefore, that there is a thickness which is practically better than any other; but what this thickness may be, remains to be determined by trials. As the quantity of motion is inverfely as the thickness (Philos. Journal, I. 576), and the force directly as that thickness, it must follow, that the quantity of mechanic effect in all similar bars, neither extremely thick, nor extremely thin, will be the same upon equal changes of temperature. I should give the preference to thin bars, not so much reduced as to have any perceptible spring.

If the fig. 4. be supposed of such dimensions, as that the circular arc struck through the middle parts of all the bars might be three feet in length, and the bars were each six inches long in the radial direction, with a thickness nearly equal to that of the second experiment related at the last-quoted article of our Journal; the space moved through, by each bar, upon an alteration of 146 degrees, would be about 0,05 inch, or half a tenth: but 300 of these bars might, with ease, be contained in the circular space of three feet, and these would produce a motion of fifteen inches by the same change of temperature, or one-tenth of an inch for every degree of Fahrenheit. In order to determine the force with which this change of position would be affected, we are in want of some experiments on the expansions of metals. It is generally supposed that a rod, or wire, will contract or dilate, by change of temperature, in the same manner, whether it be at perfect liberty to move horizontally, or be made to support a weight hung from its extremity, or placed on its upper end. This is, in fact, supposed to be the case in the estimates for constructing gridiron pendulums, and if it were strictly true, the power of this wheel would be constantly equal to the reaction against which it should be exerted. But it would be to little purpose to institute a calculation upon data assumed

at random: I shall therefore only remark, that the power of this wheel is very considerable, and may be encreased almost at pleasure, by enlarging the dimensions of the bars, in the direction parallel to the axis of the cylinder.

Whatever question there may be, with regard to the force and durability of this system of bars, neither of which I am disposed to doubt, there can be scarcely any with regard to the spiral chain round the cylinder. The direct expansion and contraction of metals is certainly very powerful, and was shewn in a striking manner by the Rev. Mr. Jones in an experiment related, if my memory be correct, in George Adams's Philosophical Lectures. He hung a very heavy weight to the longer end of a lever, the shorter arm of which pressed upwards against the longer arm of another lever, and the shorter arm of this last was supported by a rod of metal. By this mechanical arrangement, it will be understood, without difficulty, that a very slight motion of the arm which bore upon the metal, might be attended with a very considerable motion of that extremity which supported the weight; and the dimensions were such, that this was in fact the case. The flame of a candle applied to the bar of metal caused it to expand, and carried up the load without difficulty.

Hygrometric contractions and dilatations are known to be performed with immense force; but want of durability in the materials, and the difficulty with regard to exposure, which has been already mentioned, seem to forbid the practical use of this power for the purposes to which our attention is now directed.

The contrivance, fig. 5, may be considered as effectual; but the objections which have been made to Ellicott's pendulum, are still more strongly applicable to this, namely, that the friction, the wear, and the irregular action of the joints, must be hurtful to the general effect.

VII.

Essays on the Art of Dying, by Means of the Solutions of Tin, and the coloured Oxides of that Metal. By J. M. HAUSSMAN.*

THIS memoir of Citizen Haussman contains the results of his numerous experiments on the solutions of tin, and the oxydes of that metal; results which, as well as the observations of the author, are very interesting, not only with regard to the improvement of the art of dying, but likewise with regard to our scientific knowledge of the different degrees of the oxygenation of metals, the union of their oxydes with their solvents, and the surcompositions of which those oxydes are susceptible.

The author commences his paper by announcing, that since the time of his researches concerning the Turkey red, of which a description is inserted in the twelfth volume of the *Annales de Chimie*, he has discovered a red which is no less simple than beautiful and solid, the process of which he intends speedily to publish.

* Abstract from the memoir of the author bearing the same title, by C. A. Prieur, in the *Annales de Chimie*, XXX, 15, of which the present article is a translation.

He then proceeds to give an account of his experiments, concerning the means of combining the colouring matter of madder, cochineal, and all the several dying drugs, with tin, in order to fix them immediately on piece-goods, in the way of solution and precipitation.

His experiments were made on the solutions of tin, by the nitro-muriatic, muriatic, sulphuric, and acetous acids, and by potash; and he mixed the colouring decoctions of plants, in some instances, with the acid solutions; in others, with the oxydes precipitated from them; or, otherwise, he re-dissolved in alkali the oxydes already coloured. The following is a summary of the principal circumstances and effects of these operations.

When the fluids do not immediately afford a precipitate, he causes it to take place by immersing a piece of silk, cotton, or woollen. The precipitation, in some instances, takes place by the simple mixture of the two liquids, and the result consists of oxydes, coloured according to the nature of the vegetable substance. These coloured oxydes appear likely to be useful in painting. In other experiments he forms these coloured powders, by first precipitating the oxyde of tin from its acid solution, by the mere addition of water; and the edulcorated oxyde seizes part of the colouring matter from the tincture in which it is plunged. Lastly, these coloured oxydes being dissolved by potash, and the stuffs impregnated with the solution, the colouring matter afterwards become fixed, either by simple exposure to the air or by immersion in an acid solution of tin or of alumine, according to the object proposed by the operator.

The colours which are produced differ in kind, in shade, in brightness, and in solidity, from various causes, namely, the species of vegetable, the kind of acid, and the oxydation of the tin; the portion of acid, or alkali, which may exist in the precipitate, according to the choice of the operator; the greater or less proportion of water in the solutions; the other circumstances of manipulation; or the several ingredients which may have been employed.

Each acid differs principally by the degree of oxydation to which it is disposed to carry the tin. The nitro-muriatic acid being capable of variation in its dose of oxygen, is, on this account, more or less proper to advance the oxydation of the metal. The oxydation is also more perfect, if the solvent be more concentrated, or if the action of heat be added, or if the solution be expedited, by adding a considerable quantity of the metal at one time. The opposite conditions are attended with the contrary effect. In order to regulate, at pleasure, the greater or less oxydation of the metal, and in some measure at the will of the operator, it will be sufficient, if, to a nitro-muriatic solution of tin, there be added a certain quantity of the muriatic solution of the same metal; for this last may easily be obtained at the least possible degree of oxydation.

It is a well-known fact, that the spontaneous progress of the oxydation of the solutions of tin, or the addition of water, in sufficient quantity, cause the separation of a precipitate. This effect may be prevented when necessary, by leaving or adding an excess of acid to the solution; or by adding a quantity of the muriatic solution of tin, which has the property of supporting a much greater quantity of water without precipitation; or, lastly, by adding muriate of soda,

or

or any other muriatic salt, the effect of which seems to be, that they saturate the excess of water which would else have occasioned precipitation.

To hasten the muriatic solution of our metal, it is advantageous to sprinkle it with the concentrated acid, and then leave it exposed to the air. The surface of the metal becomes oxyded with such rapidity, that a sensible heat may be produced; arising, no doubts from the caloric taken from the oxygen gas absorbed from the atmosphere.

The muriatic solution of tin, when concentrated, affords many crystals at the temperature of ice, and these crystals are permanent in a cold place. The heat of thirty or forty degrees above zero (q. of Reaumur?) liquifies them. When dissolved in water, they afford a solution of tin, which may be used as occasion requires. This solution becomes oxyded to the maximum (and will, consequently, produce different effects on the colouring substances) if it be left exposed for a certain time to the air, the contact being favoured by an extensive surface.

The oxyde of tin, precipitated from its muriatic, or nitro-muriatic solution, by an alkaline carbonate, may, if not too much oxyded, be very easily, and without decomposition, dissolved in the diluted nitric or sulphuric acids, and also in the acetous acid. These solutions produce, with colouring substances, shades, which are more especially governed by the oxydation of the tin, and are disposed to change more or less speedily in the air, by a superoxydation of the metal, and according to the nature of the solvent. The sulphuric acid accelerates this effect more than the nitric, and the acetous acid the most rapidly of any; which circumstance shews that the acetic solution of tin, if it be required to be kept without spontaneous precipitation, must be preserved in well-closed bottles; and that, on this account, it would be preferable to the muriatic solution for eudiometric essays.

The disposition of the coloured oxydes of tin to change their shade by superoxygenation from the air, appears to depend on the circumstance that they retain a portion of the acid solvent. If, on the contrary, they should retain a portion of alkali, the shade is different, and continues fixed in its first state. These precipitates do not, however, appear to unite with the carbonic acid, whether it be that this acid has very little attraction for the oxyde of tin, or whether, as is more probable, that the colouring substances exclude it.

A very remarkable phenomenon takes place in these changes of shade, by the degree of oxydation, when the infusion of cochineal is made use of: the least-oxyded precipitates are of a violet colour, which by exposure to the air becomes more or less speedily converted into beautiful carmines. Ammoniac causes them to approach somewhat towards crimson; but they preserve their primitive colour, and are even more beautiful when the ammoniac has evaporated. If this alkali were employed upon the carmine red produced by precipitation from the nitro-muriatic acid, the new shade would be brown, and would not recover its original lustre.

In general this kind of colours depends on several complicated circumstances: when applied on piece-goods, they resist the action of acids better than that of the air and the sun. They become still more unchangeable by substituting, instead of the nitro-muriatic solution of tin, that made with muriatic acid, or a mixture of both. The coloured oxydes appear likewise to gain both in solidity and brilliancy when they are prepared by a solution of which the acid is not capable

pable of decomposition, and in which the oxygen is afforded to the metal by the decomposition of water. The muriatic acid, and, which is still better, the acetous acid, afford this advantage.

The phosphoric, oxalic, gallic, and tartareous, acids presented no valuable properties with regard to the production of these colours. The precipitates formed by the union of these acids with the oxyde of tin do not discolour the infusion of cochineal.

The quantity of water which may be used contributes likewise to the greater or less intimate combination of the oxyde of tin with colouring matters, and modifies the shade which is afforded. For example, in the production of the carmine oxyde by the infusion of cochineal, too little water affords a dull colour, and too much prevents the metallic oxyde from acquiring colour enough; so that it may even be completely discoloured by repeated washing.

After having given a variety of interesting details respecting the coloured oxydes of tin, and carefully related the essential conditions for producing each of these oxydes in particular, the author purposes to use, as a mordant in dying, the solution of the acetite of tin, which he prefers obtaining by the cold mixture of the aqueous solutions of the acetite of lead, and the muriate of tin in crystals, in order that the acetous solutions of tin thus obtained may be very concentrated, and loaded with the metal. We cannot do better in this place than copy the words of the author, which will explain the nature of the operation, its importance, and the precautions it requires.

“ In order to apply the acetic solution of tin upon cotton or linen, either by the block or the pencil, it is necessary that it should contain gum, and be left at rest for several days (upon the piece), during which time it quits its acid solvent, and deposits its oxyde of tin, which by affinity of adhesion will remain fixed on the piece by attracting the oxygen of the atmosphere. Before the piece is exposed to the dying action of any substance whatever with heat, it must be boiled a few minutes in bran and water, or with cow-dung, after which it must be washed in running water. The colours to be produced will be different, according to the dying drugs made use of. Madder, vegetable kermes, cochineal, and fermambouc, will afford reds of various degrees of beauty; the wood St. Martin affords a brown; logwood various shades of violet; yellow wood, quercitron, yellow berries (graines d’Avignon), &c. &c. afford yellows. By mixing all these ingredients in different proportions, by weakening the acetic solution of tin more or less with the gummed water, or by adding the acetic solution of iron, an infinity of shades may be produced. It is essential to gum this mordant before it is applied on the linen or cotton. If this circumstance be forgotten, the colours will be less bright; the same precaution must be attended to when the acetic solution of alumine is added. But, on the contrary, silks and woollens impregnated with an ungummed solution, then dried for several days, and dyed with the beforementioned ingredients, exhibit very bright colours. Wool more particularly, dyed with cochineal, then passed through boiling water, and afterwards washed, presents the most beautiful purple.

“ The intensity of these colours will be much increased by using the muriatic solution of tin,
“ instead

“ instead of the acetic. But this solution, which becomes more and more oxygenated as it dries, and successively disengages a portion of acid, weakens the cotton or the linen, which are impregnated with it; but wool and silk suffer less. To avoid this inconvenience, it is necessary to defend the piece by a solution of one part of Marseilles soap, and sixteen of water *. The article, after drying, is to be plunged in the muriatic solution of tin diluted with water, and when well washed, it may be dyed. The colours obtained by means of soap are more lively†.”

The author here makes a very important remark with regard to the play of the affinities; namely, that in order to have the coloured oxydes of tin beautiful and solid, it is necessary that there should remain a small quantity of combined acid; and that nevertheless the same colours are obtained upon goods dyed by the intermedium of the solutions of tin, though these goods may have been previously passed through boiling water, which must necessarily carry off the salt or acid which might remain, and prove an impediment to the attraction of the colouring parts. The author inclines to attribute this effect in linens and cottons to part of the gum, with which they are impregnated, remaining combined with the oxyde, and serving instead of the acid, while in wool and silk the animal part acts in a similar manner.

Passing afterwards to the operations made with the alkaline solutions of the coloured oxydes, which he succeeded in obtaining, deprived of all causticity, he gives the following description. “ I commonly use for these solutions, when the coloured oxyde is in the form of powder, a solution of potash made with one part of the carbonate of potash in crystals, one part of lime, and eight parts of water. After decantation, and reducing the liquid to one-half, I pound and stir well the powder with this liquor. The reduction of the fluid is to be made to one-fourth, when the oxyde is intended to be dissolved simply as it comes from the filter without drying. It is thus put into a vessel, and shaken with the fluid.

“ To avoid an excess of potash, the solution must be poured a little at a time upon the oxyde, and some oxyde must be kept in reserve, in case too much alkali should have been added. This may be easily known by applying a small drop to the tongue.

“ When these solutions do not possess sufficient consistence, they must be gummed. By this means they become applicable by the block or the pencil. They fix upon the cotton or linen the coloured oxydes, which, by some weeks repose, attract the carbonic acid of the atmosphere, is with which the alkali becomes saturated by degrees.

“ The precipitation of these oxydes may likewise be made speedily, and immediately after the piece is dried, in which method the colours are also more vivid. For this purpose nothing more is requisite but to steep the piece for fifteen minutes in a muriatic solution of tin, diluted with twenty parts of water, or, which is better, in a solution of fulphate of alumine made with eight parts of

* I have given in the *Annales de Chemie* preparations of soap, which may be used instead of that of Marseilles. Note of the author.

† *Journale de Physique Pluvioise*. Annual Register, vol. v. p. 114, where the entire memoir is to be found.

water, the excess of acid being absorbed by adding one-eighth part of carbonate of lime, while the liquor is hot. Either of these processes with the alkaline solution of the carmine red oxyde, afforded by muriate of tin and an infusion of cochineal, produce nearly the same shade of carmine, inclining to crimson. The shade, on the contrary, will be poppy colour, if, instead of the muriatic solution, the nitro-muriatic solution of tin be added, diluted with the same quantity of water; to which, in order to prevent spontaneous precipitation, one-fourth of the muriate of soda has been added. These colours are as solid as the oxydes from which they arise."

Citizen Hauffman then gives a detail respecting the shades of colour, of infinite variety, which may be obtained from the various mixtures of the alkaline solutions of coloured oxydes: very beautiful scarlet reds are afforded by a mixture of the solution of the carmine oxyde, with that of the oxyde coloured by yellow wood. The solutions of indigo made by means of arsenic, antimony, or muriate of tin, may be used, but not those made by the sulphurets of the same metals, because the immersion of the piece-goods, which must afterwards be made in the muriatic solution of tin, or in that of the sulphate of alumine, would precipitate the sulphurets, and render the colours obscure.

The shades of green, of violet, and of prune monfieur, are particularly remarkable; the blue grounds can scarcely be obtained of an even tinge but in the vats. The solutions of indigo, gummed, and applied either alone or mixed with other coloured solutions, being usually unequal; too much excess of alkali in the blue solutions would render the colours dull.

A fine blue may likewise be produced without the solution of tin, by that of gummed indigo, dried on the piece, and afterwards steeped in the solution of sulphate of alumine: an ingredient affording a yellow colour will complete the desired shade of green.

Some of the coloured oxydes of tin, present singular facts; that which is coloured by fernambouc becomes clear and pale as it dries on the piece, but the colour becomes of a deep red by immersion in a solution of sulphate of alumine: a similar operation causes the colour from campeachy wood to become a deep violet, from a bright reddish grey.

The alkaline solution of the oxyde of tin, coloured by madder, requires more precautions for its success than the foregoing, and perceptibly differs from them in its properties. The author describes the most proper method of obtaining the infusion of madder, to which, a proportionate quantity of carbonate of potash is added; the coloured oxyde is obtained by mixing it with the muriatic solution of tin: but in order to obtain an intense colour, it is absolutely necessary to filter and dry the oxyde before it is dissolved in the potash. By treating this solution in the same manner as was directed for that of the carmine oxyde, by means of the solution of the sulphate of alumine, and not that of the muriate of tin, which is not applicable to the present experiment, a shade of superfine pulverized madder is obtained; by boiling with bran and water, the shade, known in manufactories by the name of the second red, will be obtained, which will become brighter by the addition of gum-water.

The alkaline solution of the oxyde, coloured by madder, when it is well made, is sufficiently consistent to be used without gum.

It is to be regretted, that goods coloured by these alkaline solutions, retain the excess of colour and of gum so strongly, that it is necessary to rub them, in order to clear it off; this inconvenience is considerable in deep shades, but less so in those which are bright.

Lastly, the author briefly exhibits the properties of zinc, by way of comparing its application with that of tin, as a colouring agent. If, like this last metal, zinc be susceptible of forming lakes with vegetable decoctions, or infusions, its properties differ much in other respects from those of tin; for, on the one hand, the acetite of zinc cannot be used as a mordant, because its acid does not become disengaged by drying, and the oxyde requires an alkaline carbonate to fix it on the piece; on the other hand, the alkaline solution of the oxyde of zinc not being capable of superoxydation, is not proper to co-operate with the solution of indigo: and the alkali not losing its causticity, cannot be advantageously used in the solution of the coloured oxydes of zinc.

Here concludes the task I have undertaken of giving an account of the labours of Citizen Hauffman. His paper abounds with so many facts, and excellent views, that it is with regret that I have abridged his recital. I have been careful to omit nothing of importance. Some conjectural ideas of the author, respecting the production of phosphorus, during the solution of tin by the muriatic acid, and the composition of metals, presumed, from their analogy with alumine, are passed over in silence, because the author himself does not seem to attach any considerable value to these notions, and has offered them with that modesty which ought always to be shewn with regard to assertions, not yet supported by decisive experiments.

C. A. PRIEUR.

VIII.

Observations on the Manners, Habits, and Natural History of the Elephant. By JOHN CORSE, Esq..*

SINCE the remotest ages, the elephant, on account of his size, his sagacity, and his wonderful docility, has attracted the notice, and excited the admiration of philosophers and naturalists, both ancient and modern; and few travellers into Asia, or Africa, have omitted giving some account of him.

A residence, however, of more than ten years in Tiperah, a province of Bengal, situated at the eastern extremity of the British dominions in Asia, where herds of elephants are taken every season, afforded me frequent opportunities of observing not only the methods of taking them, but also the habits and manners of this noble animal.

From the year 1792 till 1797, the elephant hunters were entirely under my direction; so that I had it in my power to institute such experiments as I thought likely to discover any par-

* *Philos. Transf.* 1799. p. 31.

ticulars, not formerly known, in the natural history of the elephant. Soon after my arrival at Tiperah, while informing myself of the methods of taking wild elephants, I had occasion to observe, that many errors, relative to the habits and manners of that useful animal, had been stated in the writings of European authors, and countenanced by some of the most approved writers.

The elephant has been declared to possess the sentiment of modesty in a high degree; and, by some, his sagacity was supposed to excite feelings for the loss of liberty, so acute as to cause him to refuse to propagate his species while in slavery, lest he should entail on his progeny a fate similar to his own; whilst others have asserted, that he lost the power of procreation in the domestic state.

So circumstanced, I was desirous of taking advantage of my situation, and of making such experiments and observations, as might tend to render more perfect the natural history of this useful animal.

Early in the year 1789, I gave an account of the methods then used for taking and training wild elephants, to the Asiatic Society in Calcutta, which was published in vol. iii. of their Researches: and the following experiments and observations, made since that period, on the natural history of the elephant, will not, I hope, prove unworthy the attention of the Royal Society.

The young of the elephant, at its birth, is about 35 inches high; and, as a knowledge of its progressive growth forms the best criterion by which we can judge of the age of this animal, I shall here note down some observations made on this subject, till the elephant has attained its full size; for, after this period, till signs of old age appear, I do not know any marks by which a tolerable guess can be made of the number of its years, unless we could examine the teeth accurately; and, even then, there would be much uncertainty.

Very erroneous notions have been entertained, with respect to the size of elephants, in different parts of India; for which reason, I have collected such facts as were likely to ascertain their general height. The following observations, of the gradual increase of growth, were made upon a young elephant of Mr. Stephen Harris, which was accurately measured from time to time, and upon a female elephant of my own, till I left Tiperah.

Mr. Harris's elephant, at its birth, October 16, 1789, was 35 inches high.

			Feet.	Inches.
In one year he grew 11 inches, and was			3	10 high.
In the 2d year	8	-	4	6
In the 3d year	6	-	5	0
In the 4th year	5	-	5	5
In the 5th year	5	-	5	10
In the 6th year	3 $\frac{1}{2}$	-	6	1 $\frac{1}{2}$
In the 7th year	2 $\frac{1}{2}$	-	6	4

Except during his 4th and 5th years, the above measurement shows a gradual decrease in the

the proportion of growth for every year; and there was no opportunity of tracing the growth of this elephant further than its 7th year.

Another elephant, six feet nine inches high, at the time she came into my possession, was supposed to be fourteen years old; but, as the accuracy of the hunters cannot be depended on, it will be proper to take Mr. Harris's elephant, whose age is exactly known, as a standard: and, judging from its annual increase, this will lead us to consider the elephant, at the time I received her, to be only eleven years old; giving a period of four years, for the addition of five inches. I have made a greater allowance of time, on account of this elephant being a female, and Mr. Harris's a male, which there is much reason to believe grows faster.

During the next five years, before she was covered, she grew only six inches; but, what is extremely curious, while pregnant, she grew, in twenty-one months, five inches: and in the following seventeen months, though again pregnant, she grew only half an inch; at which time, she was sent from Comillah, as I was then preparing to leave India.

At this time, she was about nineteen years old, and had, perhaps, attained her full growth. Her young one was then (Nov. 1796) not twenty months old; yet he was four feet five inches and a half high, having grown eighteen inches since his birth, which is the greatest progressive growth, in the elephant, that I have known.

These observations, when applied to the general growth of elephants, are to be taken with some allowance; since, during the state of the first pregnancy, there is so great an irregularity in the growth of female elephants, as alone occasions considerable difficulty, even supposing the progressive growth nearly equal in the species. It is probable, however, that this is not by any means equal: for, as the elephants vary greatly in size, and as males are generally much taller than females, we must conclude they either grow faster, or are longer in attaining their full growth*. But it may be safely asserted, that elephants, like most quadrupeds, propagate their species before they have acquired their full growth. Many females have been known, when taken while pregnant, to have grown several inches higher before delivery; and, as it has been stated, that the female elephant on which my observations were made, could not exceed sixteen years when she received the male, it is probable the wild female elephants are in heat before that period.

If, from the above data, it may be allowed to form a probable conjecture, elephants attain their full size between eighteen and twenty-four years of age. The height of the elephant, I believe, has been generally much exaggerated. In India, the height of females is, in general, from seven to eight feet; and that of males, from eight to ten feet, measured at the shoulder.

I have never heard but of one elephant, on good authority, that much exceeded ten feet: this was a male, belonging to Asoph ul Dowlah, the late vizier of Oude. His dimensions, as obligingly communicated to me by Mr. Cherry, then resident at Lucknow, were as follow.

* A male elephant, belonging to the Cudwah Rajah, till he was above twenty years of age, continued to increase in height, and was supposed not to have attained his full size, when I left Tiperah: he was then above twenty-two years old.

He was measured on the 18th of June, 1796.

	Feet.	Inches.
From foot to foot over the shoulder - - - -	22	10 $\frac{1}{2}$
From the top of the shoulder, perpendicular height - -	10	6
From the top of the head, when set up, as he ought to march in state	12	2
From the front of the face to the insertion of the tail - -	15	11

Captain Sandys, of the Bengal establishment, obligingly shewed me a list of about 150 elephants, of which he had the management during the late war with Tippoo Sultaun, in Myfore, and not one of them was ten feet, and only a few males nine feet and a half high. I was very particular in ascertaining the height of the elephants employed at Madras, and with the army under Marquis Cornwallis, where there were both Ceylon and Bengal elephants; and I have been assured, that those of Ceylon were neither higher, nor superior, in any respect, to those of Bengal; and some officers assert, that they were considerably inferior, in point of utility.

The Madras elephants have been said to be from seventeen to twenty feet high; but, to shew how much the natives of India are inclined to the marvellous, and how liable Europeans themselves are to mistakes, I will relate a circumstance that happened to myself.

Having heard, from several gentlemen who had been at Dacca, that the Nabob there had an elephant about fourteen feet high, I was desirous to measure him; especially as I had seen him often myself, during the year 1785, and then supposed him to be above twelve feet. After being at Tiperah, and having seen many elephants caught, in the years 1786, 1787, and 1788, and finding all of them much inferior in height to what I supposed the Nabob's elephant, I went to Dacca, in 1789, determined to see this huge animal measured. At first, I sent for the driver*, to ask some questions concerning this elephant; he, without hesitation, assured me he was from ten to twelve cubits, that is, from fifteen to eighteen feet high; but added, he could not, without the Nabob's permission, bring me the elephant to be examined. Permission was accordingly asked, and granted: I had him measured exactly, and was rather surprised to find he did not exceed ten feet in height.

The honourable company's standard, for serviceable elephants, is seven feet and upwards, measured at the shoulder, in the same manner as horses are. At the middle of the back, they are considerably higher; the curve or arch of which, particularly in young elephants, makes a difference of several inches.

After an elephant has attained his full growth, it is a sure sign of old age when this curve becomes less; and still more so, when the back is flat, or a little depressed. A partial depression of the spine is, however, no uncommon occurrence, even in very young elephants; and I am convinced it happens from external injury. I have been surprised to see the violence used (in herds of wild elephants just taken) by the large elephants, both male and female, putting the projecting part of the upper jaw, from which the tusks grow out, on the spine of the young ones, and pressing them to the ground, while they roared from pain.

It has been stated, that the sagacity of the elephant is so great, and his memory so retentive,

* Or *mabote*, as he is generally called.

that when once he has received an injury, or been in bondage, and afterwards escapes, it is not possible, by any art, again to entrap him. Great as my partiality is for this noble animal, whose modes of life and general sagacity I have had so many opportunities of observing, yet a regard to truth compels me to mention some facts, which contradict that opinion. The following history of an elephant taken by Mr. Leeke*, of Longford Hall, Shropshire, contains many interesting particulars on this subject. The elephant was a female, and was taken at first, with a herd of many others, in the year 1765, by Rajah Kishun Maunick †, who, about six months after, gave her to Abdoor Rezah, a man of some rank and consequence in the district. In 1767, the Rajah sent a force against this Abdoor Rezah, for some refractory conduct, who, in his retreat to the hills, turned her loose into the woods, after having used her above two years, as a riding elephant. In January, 1770, she was retaken by the Rajah; but, in April, 1771, she broke loose from her pickets, in a stormy night, and escaped to the hills. On the 25th of December, 1782, she was driven by Mr. Leeke's elephant hunters into a *keddah* ‡; and, the day following, when Mr. Leeke went to see the herd that had been secured, this elephant was pointed out to him by the hunters, and particularly by a driver who had charge of her for some time, and well recollected her. They frequently called to her by name; to which she seemed to pay some attention, by immediately looking towards them, when her name, *Juggut-Peāurce*, was repeated; nor did she appear like the wild elephants, which were constantly running about the *keddah* in a rage, but seemed perfectly reconciled to her situation.

* He was then the resident of Tiperah, and took some pains to ascertain the facts here mentioned.

† The Rajah is the principal Zemindar in the province of Tiperah, paying the usual revenue for his lands in the low country; but, in the hills he is an independent sovereign, has the power of life and death over his subjects, a mint, and other insignia of sovereignty.

‡ The inclosure in which elephants are secured. *Vide Asiatic Researches*, Vol. III. Art. "Method of catching Elephants."

(To be concluded hereafter.)

PHILOSOPHICAL NEWS AND ACCOUNTS OF BOOKS.

Experiments on the Vibrations of Plates of Glass. By Professor Chladni, of Berlin.

IN the year 1787, M. Chladni published, at Leipzig, a work in German, entitled, *Discoveries respecting the Theory of Sound*; in which he announces, that if glass, sprinkled with fine powder, be made to sound, this powder will be so distributed by the effect of the vibrations, as to form very remarkable figures, which are constantly produced under similar circumstances.

These

These experiments have been lately repeated at Paris. In order to make them with success, it is necessary to take a square piece of glass, three or four inches wide, not too thick, and without either bubbles or knots. This plate is to be held firm between two very pointed pieces of cork, and then powdered, with very fine sawdust or sand; and when the bow of a violin, well rosin'd, is drawn against the edge of the glass, blunted or rounded by grinding, a sound will be produced, and, at the same time, the powder will be seen to dispose itself in lines which afford different figures, according to the manner or place at which the glass is held, the bow is drawn, and the sound produced.

If, for example, the square be pinched by its centre, and the bow drawn along the middle of one of its sides, the powder will dispose itself in two lines, nearly diagonal to the square. If the bow be drawn at the distance of one-fourth of the side from the angle, the two lines will become the sides of an octagon, and the sound will be the octave of the preceding tone.

By varying the position of the point at which the glass is held, the figures also become changed.

If the plate of glass be circular, and the bow be a little inclined, the six radii of an hexagon will be formed.

In this manner, Mr. Chladni obtained 166 different figures, which he calls resonant figures. Without precisely explaining the cause which produces these figures, their analogy with the stationary and vibrating parts of a musical string evidently shews that the vibrating surface divides itself into a number of portions which move separately, but, no doubt, in an isochronous or commensurate manner, when the tones are clear and musical. The lines in which the powder is collected, are a kind of gutters formed by the points which remain at rest, while the other parts become alternately convex and concave.

These experiments, which succeed equally with plates of metal, and even of wood, being carefully made and classed systematically, promise to throw much light on the manner in which surfaces vibrate, and may, perhaps, tend to the improvement of the theory of wind instruments, and other musical apparatus, which is still very imperfect, notwithstanding the labours of Euler, in attempting to reduce them to computation.

Decade Philos. No. 17. Art. VII.

Philosophical Transactions of the Royal Society of London, for the Year 1799. Part the First. Quarto, 156 Pages, with three Plates; London, sold by Elmsly.

This part contains, 1. The Croonian lecture. Experiments and observations upon the structure of nerves. By Everard Home, esq; F.R.S. 2. The Bakerian lecture. Observations upon an universal horizontal refraction of the air; with remarks on the variations to which the lower parts of the atmosphere are sometimes subject. By the rev. S. Vince, A.M. F.R.S. and plumian professor of astronomy and experimental philosophy in the university of Cambridge. 3. Abstract of a register of the barometer, thermometer, and rain, at Lyndon, in Rutland, 1797.

With

With some remarks on the recovery of injured trees. By Thomas Barker, Esq. 4. Some additions to a paper read in 1790, on the subject of a child with a double head. By Everard Home, Esq; F.R.S. 5. Observations on the manners, habits, and natural history of the elephant. By John Corfe, Esq. 6. On the decomposition of the acid of borax, or sedative salt. By Lawrence de Crell, M.D. F.R.S. Lond. and Edinb. and M.R.I.A. translated from the German. 7. A method of finding the latitude of a place, by means of two altitudes of the sun, and the time elapsed betwixt the observations. By the rev. W. Lax, A.M. Lowndes's professor of Astronomy in the university of Cambridge. 8. A fourth catalogue of the comparative brightness of the stars. By William Herschel, LL.D. F.R.S. 9. On a submarine forest on the east coast of England. By Joseph Correa de Serrea, LL.D. F.R.S. and A.S. Appendix. Meteorological journal, kept at the apartments of the Royal Society, by order of the president and council.

Traité des Montres à Longitudes, &c. or a treatise on time-pieces, containing the description, construction, and all the details relative to the workmanship of these machines; their dimensions, the method of trying them, &c.; together with, 1. An instructive memoir on the fabrication of clocks and time-pieces; 2. Description of two astronomical clocks; 3. Trial of a simple method of preserving the relation of weights and measures; and of establishing an universal and perpetual measure. One volume in quarto, with seven engraved plates; by Ferd. Berthoud, of the national institute of France, and mechanic to the marine. Sold at Paris by the author aux Galleries du Louvre, 1792.

This work *, though it has been printed seven years, has not been published till lately; and we hasten to announce it to the public. It is an important supplement to the *Essai sur l'Horlogerie*, 2 vols, in quarto, published in 1763 and 1786, and to the *Traité des Horloges marines*, 1773, of the same author.

The first article consists of the description of a marine time-piece, portable and vertical, denoted by the number 46, which has been used at sea. It is slung in jimbals; and when used on shore it is carried in the pocket. On this occasion we find a description of a new free escapement.

The second chapter presents a description of a portable vertical watch, without a suspension, distinguished by the number 47. It has the common form of a pocket watch.

In the third and fourth chapters descriptions are given of two small horizontal watches intended to serve as regulators, remaining at rest in the vessel, carried by their suspensions: that which is denoted by number 45 makes four vibrations per second. In the other time-piece, no. 48, the balance makes two vibrations per second.

Chapter V. contains the most essential details for the execution of watches and small time-pieces for the longitude.

* This account is given by Lalande in the *Magasin Encyclopédique*, VII. 113.

Chapter VI. treats of the compensation for temperature produced by the balance itself.

In the seventh chapter, cit. Berthoud gives the construction and means of executing a portable watch, the compensation of which is produced by the balance and the pendulum spring. And, lastly, he explains the construction of a vertical watch, without a fusee, and carried by suspension. A method is given to remedy the want of isochronism in the pendulum spring by means of the balance itself.

The second volume of this work is entitled, *Suite du Traité des Montres à Longitudes, &c.* or Supplement to the *Treatise on Time-pieces*, containing the construction of portable vertical watches (montres), and of horizontal time-pieces (horloges), for the longest voyages: the description and trials of small horizontal * watches, very simple and portable; with two engraved plates. Paris, in the 5th republican year.

This volume, which has been printed a year, has not yet been announced or published. It is divided into two parts. Citizen Berthoud, in the first place, treats of the question of the best position for portable watches, and small time-pieces. In the next place, he treats of the most convenient number of vibrations of the balance, to diminish the frictions, and render the watch most convenient for the observer. He describes the construction of the balance, which bears its own compensation; the most simple and sure construction of the free escapement, and the spiral springs of watches to render the vibrations of the balance isochronal; the description of the elastic balance, serving for the measure and proof of the spiral springs; and, lastly, he explains the method of trying time-pieces.

In the second part, we find the construction of the vertical watch, no. 56.—the portable vertical watches, no. 60 and 62;—of the small horizontal time-piece, no. 63. constructed to give the longitude in the longest voyages;—of the small horizontal time-piece, no. 66. without (rouleau). And in the conclusion the author treats of vertical watches, and small horizontal time-pieces. The author gives the preference to the latter.

This work is terminated by a supplement, containing the result of the trials made with the horizontal watch, no. 65. in which the pivots of the balance turn simply in holes made in copper (cuivre); and, lastly, we find the construction of a very simple portable watch, and of a small horizontal time-piece improved from no. 65.

In the essay on weights and measures, the author gives the dimensions of a cylindrical pendulum, vibrating on knife edges, representing the French foot, and the results of trials made in 1791, to determine the oscillations, which proved to be 7710 per hour. Though the pendulum has been abandoned, in order to substitute the ten-millionth part of the quadrant of the meridian, yet the labours of citizen Berthoud to preserve the dimensions of any determinate pendulum, cannot be viewed without interest.

Upon the whole, this new work is calculated to add to the reputation of its illustrious author, and exhibits, without mystery, those practices which have procured him repeated success in his marine time-pieces, of which the production is of so much importance to navigation.

* I suppose the words horizontal and vertical in the whole of this account, to relate merely to the position in which the time-piece is to be habitually kept, and not to the nature of their escapements. The word horizontal has not, I believe, been applied to any escapement by the French. N.

The Effects of unusual horizontal Refraction?

Fig. 2.

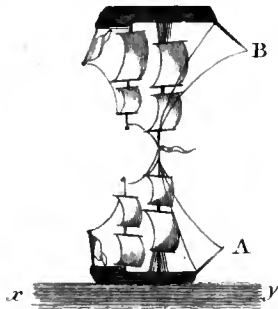


Fig. 3.

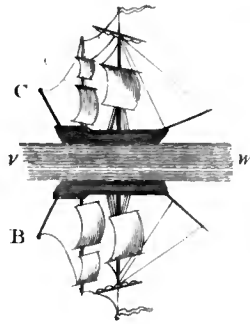


Fig. 4.

Fig. 1.

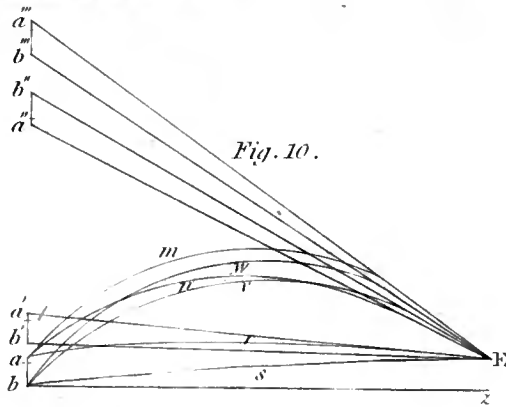


Fig. 5.

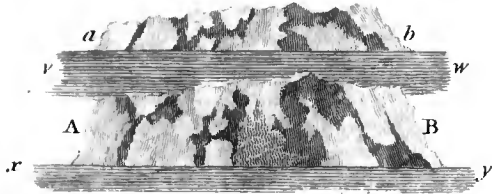


Fig. 6.

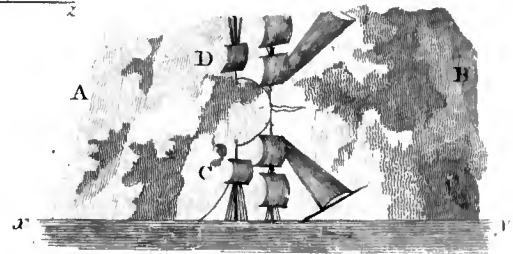


Fig. 9.

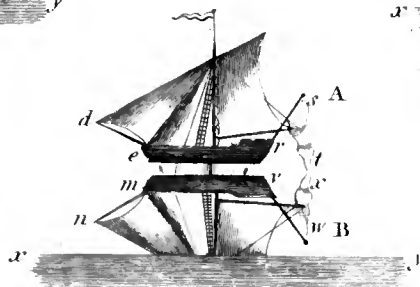


Fig. 7.

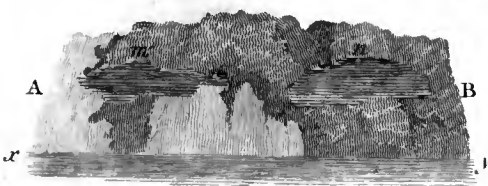
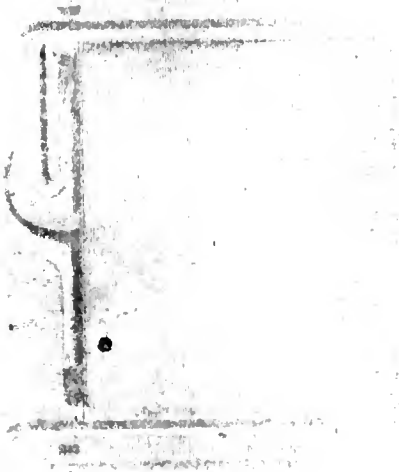
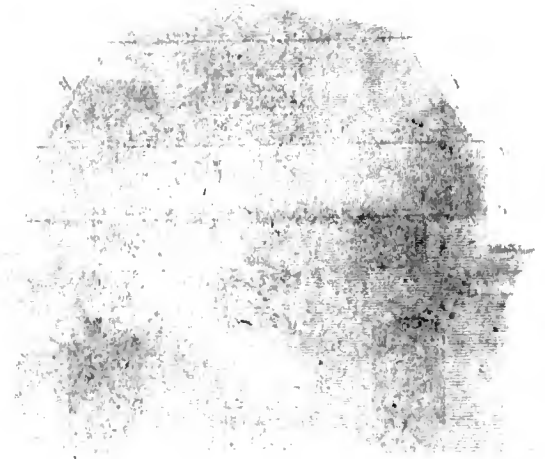


Fig. 8.





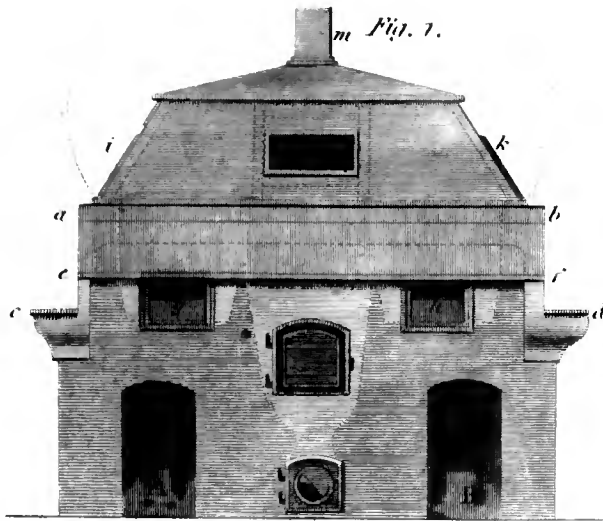


Fig. 2.

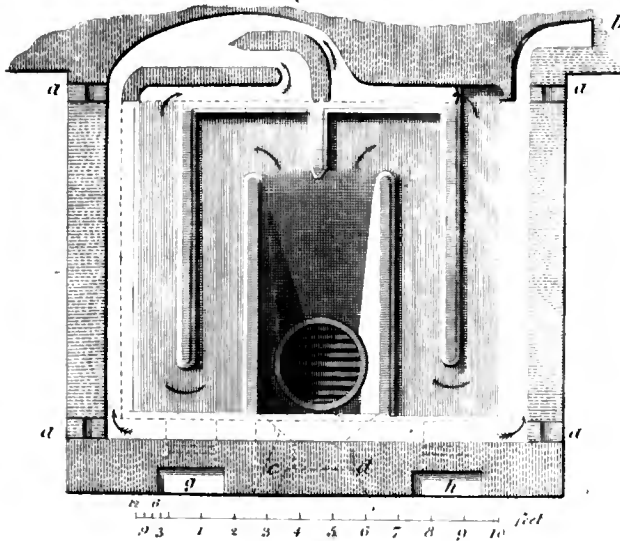
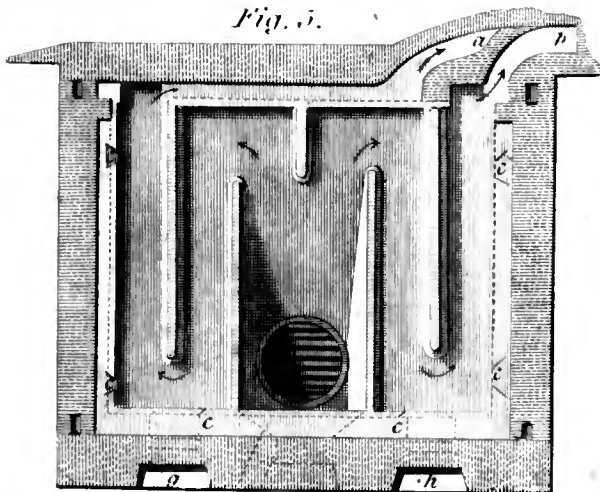


Fig. 5.



Barlow's engr.

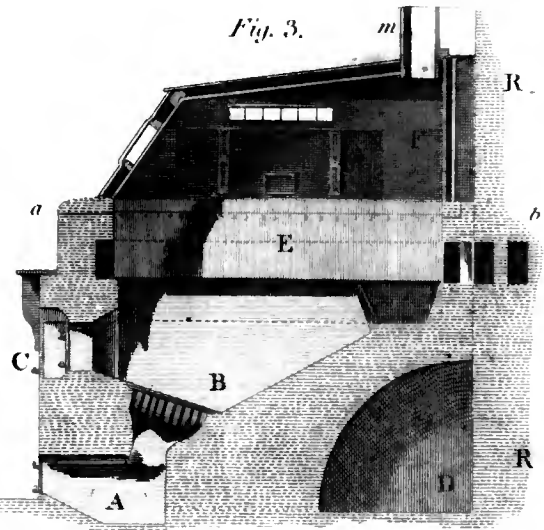


Fig. 4.

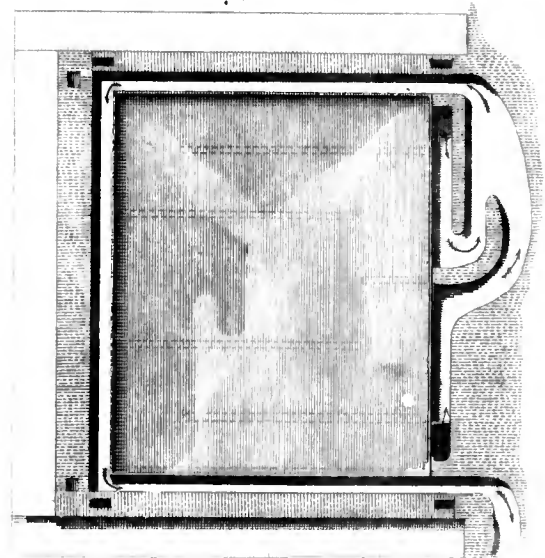


Fig. 8.

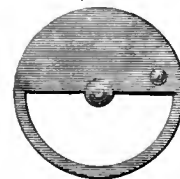


Fig. 7.

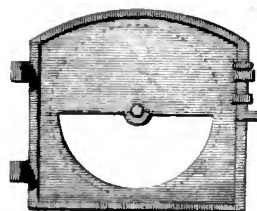
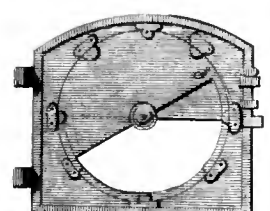


Fig. 6.



0 12 24 inches



A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

AUGUST 1799.

ARTICLE I.

*Supplement to the Paper on the Philosophical Uses of a Common Watch. By the Rev. W.
PEARSON, of Lincoln.*

TO MR. NICHOLSON.

SIR,

AS the paper "On the Philosophical Uses of a common Pocket Watch," which I sent you, was deemed sufficiently important to be inserted in your Journal, I am induced to transmit to you some additional numbers, suitable for a watch that may indicate hours, minutes, seconds, and quarters of a second, without any extra-wheel work; in order that, out of the different varieties, the watch-maker may select such numbers as are already marked upon the plate of his engine; for I understand that 64, one of the numbers proposed for a new watch, though as easily divided as any other number, is not usually put upon the common plates of watch-makers.—That the different practicable varieties may be expressed in as concise a manner as is consistent with perspicuity, I will divide the whole train of a watch into three portions: that part which comprehends the fusee, the great wheel, and its pinion, may be denominated the first portion; the second portion may be the centre wheel, with the pinion which it actuates, and the third wheel with the pinion actuated by it, which is placed

VOL. III.—AUGUST 1799.

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on the axle of the contrate wheel; and the third portion will then be the contrate wheel, with the pinion actuated by it, the crown wheel, and the palats: the first portion determines the time that a watch shall go at one winding up, as has been observed before; the second portion determines the mutual velocities of the minute and second hands; and the third portion regulates the number of beats per second: hence any one of these portions may be constructed variously, without affecting the calculations of the two others.

If a watch be required to have 6 spirals on the fusee, and to go just 30 hours, we shall have for the first portion $6 \times \frac{45}{9}$, $6 \times \frac{50}{10}$, $6 \times \frac{55}{11}$, and $6 \times \frac{60}{12}$: for the second portion we may take $\frac{48}{6} \times \frac{45}{6}$, or $\frac{45}{6} \times \frac{48}{6}$, $\frac{60}{6} \times \frac{42}{7}$, or $\frac{42}{7} \times \frac{60}{6}$, $\frac{60}{6} \times \frac{48}{8}$, or $\frac{48}{8} \times \frac{60}{6}$, $\frac{60}{6} \times \frac{54}{9}$, or $\frac{54}{9} \times \frac{60}{6}$, $\frac{60}{6} \times \frac{60}{10}$, or $\frac{60}{10} \times \frac{60}{6}$, $\frac{60}{7} \times \frac{49}{7}$, or $\frac{49}{7} \times \frac{60}{7}$, $\frac{60}{7} \times \frac{56}{8}$, or $\frac{56}{8} \times \frac{60}{7}$, $\frac{63}{7} \times \frac{60}{9}$, or $\frac{60}{9} \times \frac{63}{7}$; and, lastly, as already given, $\frac{60}{8} \times \frac{64}{8}$, or $\frac{64}{8} \times \frac{60}{8}$: likewise, for the third portion, we may put $\frac{64 \times 15 \times 2}{8}$, $\frac{56 \times 15 \times 2}{7}$, and $\frac{48 \times 15 \times 2}{6}$, any one of which formulæ, divided by 60, the seconds in a minute, will give 4 for the number of beats per second.

Here then we have got four varieties of the first portion, nine of the second, without inverting the order, and three of the third, which, by the rule for finding* compositions, will produce $4 \times 9 \times 3 = 108$ different forms of contraction, to answer the same purpose, without making any alteration in the number of spirals on the fusee, or in the dial-work; and where no number out of all the wheels exceeds 64, or is less than 42.

The allowance to be made in correcting the seconds gained by the beats of any watch must be $\frac{1}{440}$ of the whole, for every minute in the daily error in the rate of going, *plus* or *minus*, accordingly as the rate is too slow or too fast: therefore, in the second note (N), page 50, where, for an error of five minutes, it is said that the allowance will be "*less than* $\frac{1}{300}$ " of the whole, it ought to have been "*upwards of* $\frac{1}{300}$:" for $\frac{5}{1440} = \frac{1}{288}$ is a greater portion of unity than $\frac{1}{300}$, though inadvertently it might be taken as constituting a smaller.

It may probably appear too formal to notice here two typographical errors in the description of the numbers proposed for a new watch in my last paper, where "2 pivots" ought to have been "2 palats," (or "pallets," or "palettes," for different authors spell this word differently); and 6 spirals on the "barrel," 6 spirals on the "fusee;" but it is absolutely necessary to advert to another passage therein, which, partly from my own inaccuracy, which you have already noticed, and partly from the printer's omission of the signs + and -, as it now stands, is completely unintelligible. At the asterisk in page 53, let the sentence be understood to be thus: as 23 h. 56 m. 4.098 s. (the length of a sidereal rotation of the earth),

* Vide Hutton's Course of Mathematics, vol. i. p. 134.

plus or *minus* the daily error in the rate of going; are to 360° :: so is the number of observed seconds of time : to the quantity of the horizontal angle required.

These are all the additional observations which seem necessary to make, respecting the numbers of a new watch calculated for philosophical uses, and the method of applying it; but there is another instrument frequently to be met with, which is capable of various constructions, that will measure smaller portions of time, than it is usually made to measure; and which is sufficiently portable for being carried small distances: I mean the spring clock, the vibrations of which are regulated by a short pendulum. Out of the many instruments of this kind which I have noticed, I do not remember any one which measures, or, at least, which indicates seconds: I shall, therefore, subjoin such numbers as are proper for a spring clock, that shall indicate seconds, and also make a given number of vibrations in a second.— If 39,2 inches be taken as the true length of a pendulum, to swing seconds in our climate, which varies not one-tenth of an inch from the result of Mr. Whitehurst's and George Graham's experiments on the lengths of pendulums, the error in the length of small pendulums, calculated therefrom, will fall within the threads of the adjusting screw at the inferior end of the rod; on which account the lengths, so deduced, may be put down as the true lengths without impropriety: thus,

4	vibrations in a second will require a pendulum	-	2,45 inches long.
3	do.	-	4,35
$2\frac{1}{2}$	do.	-	6,27
$2\frac{1}{4}$	do.	-	7,74
2	do.	-	9,8

The two first of these pendulums appear to be too short to perform with steadiness, and consequently will be considered as unworthy of further notice. The first portion of a train, suitable for any of the three last pendulums, for a spring clock to go upwards of a week, may be $\frac{84}{8} \times 17$ turns on the fusee, or $\frac{84}{7} \times 15$ turns, or $\frac{96}{8} \times 12$ turns, or indeed any other similar numbers producing a like result: for the second portion, any one of the formulæ given for a watch in the former part of this article, will be proper; but for the last portion, each different pendulum will require different numbers: a pendulum to make $2\frac{1}{2}$ vibrations on a second, may have the contrate wheel 24, its pinion 8, and the crown wheel 25; or, otherwise, the contrate wheel 40, with a pinion of 8, and the crown wheel 15: where the vibrations are $2\frac{1}{4}$ in a second, the contrate wheel may be 36, with a pinion of 8, and the crown wheel, as before, 15: and for exactly two vibrations in a second, the contrate wheel will be required to be 32, with a pinion of 8, and the crown wheel 15, as in the two last instances. In all these calculations, a hand, placed on the axle of the contrate wheel, will indicate seconds without apparent recoil; and the trains, composed of any of the various portions laid down, will be equally accurate, and admit of many varieties.

If a spring clock is to be used for measuring small portions of time, by the vibrations of its pendulum, which falls not under the description of any of those constructions, the vibrations

and

and fractional parts of a vibration are calculable by the general rule for watches already given.

I am, fir, with much esteem, your's, &c.

Lincoln, May 9, 1799.

W. PEARSON.

P. S. Since the preceding part of this article was written, I have met with a train proposed for a new watch to indicate seconds, and quarters of a second, by the beat, under the word "clock," in vol. v. part i. p. 76, of the *Encyclopædia Britannica*, which, by the mode of expression adopted before, will stand thus:

48	great wheel	
12	60	second or centre do.
	10	60 third do.
7½	turns on the fusee.	6 48 contrate do.
To go 30 hours.		6 15 crown do.
		2 palettes.

The reader will perceive that these numbers constitute one of the 108 varieties given above, except that the great wheel is given smaller here, by reason of the fusee having more spirals. Under the same article, I also find that, as I supposed, spring clocks are not constructed to shew seconds, though it has been shewn that they are capable of many different constructions, which are equally calculated to answer this purpose, as well as to make, at the same time, a given number of vibrations in a second.

Likewise, I have just had an opportunity of examining a small larum, or alarum clock, going by a suspended weight, and regulated by a short pendulum only 4¼ inches long, which, consequently, makes upwards of three vibrations in a second, and which, I understand, measures time pretty accurately. I mean as accurately as these clocks are intended to do; for great accuracy cannot be expected from a clock with a very short pendulum, particularly if it be a spring clock that goes a week; on account of the difficulty of forming the fusee to correspond exactly to the action of a powerful spring for many successive days, as well as on account of the imperfections of the pendulum: but whatever be the error in any day, the error in the time of a beat of that day will be a proportional part thereof; and provided the rate of going be tolerably uniform throughout each successive day, after winding up, the beats will also be nearly uniform: I shall, therefore, give the numbers proper for the third portion of a train suitable for a pendulum, to vibrate three times in a second, which I had purposely omitted. If any of the 1st or 2d portions, already specified, be adopted, the contrate wheel will be 48, with a pinion of 8, and the crown wheel 15. I have preferred a pinion of 8, from a persuasion that this number will render the works of a watch or clock more perfect, than either 7 or 6 would do; and is the number we find in the best finished instruments. If any person should wish to try the action of a pendulum to swing the quarters of a second exactly, any of the trains proposed for a new watch will be found applicable to this purpose. The reason why an *odd* number is always fixed upon as proper for a crown wheel, will need no explanation.

May 11th.

II. Obser-

II.

Observations on the Manners, Habits, and Natural History of the Elephant. By JOHN CORSE, Esq. (Continued from p. 185.)

FROM the 25th of December to the 13th of January (a space of eighteen days), she never went near enough the outlet (or *roomee*) to be secured; from a recollection, perhaps, of what she had twice before suffered*. Orders, however, had been given, not to permit her to enter the outlet, had she been so inclined, as Mr. Leeke wished to be present when she was taken out of the *keddah*. On the 13th of January, 1783, Mr. Leeke went out, when there were only herself, another female, and eight young ones, remaining in the inclosure. After the other female had been secured, by means of the *koomkees*† sent in for that purpose, the hunters were ordered to call *Juggut-Peāuree*. She immediately came to the side of the ditch, within the inclosure: on which, some of the drivers were desired to carry in a plantain tree, the leaves of which she not only took from their hands, with her trunk, but opened her mouth, for them to put a leaf into it, which they did, stroking and caressing her, and calling to her by name. Mr. Leeke, seeing the animal so tame, would not permit the hunters to attempt tying her; but ordered one of the trained elephants to be brought to her, and the driver to take her by the ear, and order her to lie down. At first, she did not like the *koomkee* to go near her, and retired to a distance, seemingly angry; but, when the drivers, who were on foot, called to her, she came immediately, and allowed them to stroke and caress her, as before; and, in a few minutes after, permitted the trained females to be familiar. A driver, from one of these, then fastened a rope round her body, and instantly jumped on her back; which, at the moment, she did not like, but was soon reconciled to it. A small cord was next fastened round her neck, for the driver to put his feet in, who, seating himself on the neck, in the usual manner, drove her about the *keddah*, the same as any of the tame elephants.

After this, he ordered her to lie down, which she instantly did; nor did she rise till she was desired. He fed her from his seat, gave her his stick to hold, which she took with her trunk, and put into her mouth, kept, and then returned it, as she was directed, and as she formerly had been accustomed to do. In short, she was so obedient, that had there been more wild elephants in the *keddah* to tie, she would have been useful in securing them.

Mr. Leeke himself then went up, took her by the ear, and bade her lie down; a command she instantly obeyed.

I have known several other instances of elephants being taken a second time; and was myself a witness both of the escape and retaking of one, as related in the following account.

* When elephants were secured in the outlet from the *keddah*, they bruised themselves terribly. *Fide Asiatic Researches*, Vol. III.

† *Koomkees* are female elephants, trained for the purpose of securing wild elephants, and more particularly those large males which stray from the woods, named *goondabs*. *Fide Asiatic Researches*, Vol. III.

In June, 1787, *Jättra-Mungul*, a male elephant, taken the year before, was travelling, in company with some other elephants, towards Chittigong, laden with a tent and some baggage, for our * accommodation on the journey. Having come upon a tiger's track, which elephants discover readily by the smell, he took fright, and ran off to the woods, in spite of the efforts of his driver. On entering the wood, the driver saved himself, by springing from the elephant, and clinging to the branch of a tree under which he was passing; when the elephant had got rid of his driver, he soon contrived to shake off his load. As soon as he ran away, a trained female was dispatched after him, but could not get up in time to prevent his escape; she, however, brought back his driver, and the load he had thrown off, and we proceeded, without any hope of ever seeing him again.

Eighteen months after this, when a herd of elephants had been taken, and had remained several days in the inclosure, till they were enticed into the outlet, there tied, and let out in the usual manner, one of the drivers, viewing a male elephant very attentively, declared he resembled the one which had run away. This excited the curiosity of every one, to go and look at him; but, when any person came near, the animal struck at him with his trunk, and, in every respect, appeared as wild and outrageous as any of the other elephants.

At length, an old hunter, coming up and examining him narrowly, declared he was the very elephant that had made his escape about eighteen months before.

Confident of this, he boldly rode up to him, on a tame elephant, and ordered him to lie down, pulling him by the ear at the same time. The animal seemed quite taken by surprise, and instantly obeyed the word of command, with as much quickness as the ropes, with which he was tied, permitted; uttering, at the same time, a peculiar shrill squeak through his trunk, as he had formerly been known to do; by which he was immediately recognized, by every person who had ever been acquainted with this peculiarity.

Thus we see that this elephant, for the space of eight or ten days, during which he was in the *keddah*, and even while he was tying in the outlet, appeared equally wild and fierce as the boldest elephant then taken; so that he was not even suspected of having been formerly taken, till he was conducted from the outlet. The moment, however, he was addressed in a commanding tone, the recollection of his former obedience seemed to rush upon him at once; and, without any difficulty, he permitted a driver to be seated on his neck, who, in a few days, made him as tractable as ever.

These, and several other instances which have occurred, clearly evince, that elephants have not the sagacity to avoid a snare into which they have, even more than once, fallen.

The general idea, that tame elephants would not breed, has doubtless prevented trials being made, to ascertain whether, under particular circumstances, this supposed reluctance could be got the better of.

I was however convinced, from observation, as well as from some particular facts, that elephants had their seasons in which they were in heat; I shall, therefore, first mention the

* Mr. Buller and myself,

circumstances which induced me to attempt breeding from tame elephants, and then relate the success of the experiments instituted for this purpose.

The circumstances to which I allude, happened in January, 1790, at a *keddah* near to Comillah, the capital of Tiperah.

Messrs. Henry Buller and George Dowdeswell, of Chittigong, being then on a visit at Comillah, accompanied me and several others, to see a herd of elephants which had been lately taken. Our visitors then proposed a trial being made, of tying the wild elephants immediately, in the *keddah*, in the manner practised at Chittigong, instead of waiting till they were enticed, one after another, into the narrow outlet, there to be secured, and led out in the usual manner*.

This mode they recommended so earnestly, from a conviction of its superior utility†, that Mr. John Buller, to whom the *keddah* belonged, assented to the trial being made, and gave orders for the trained females, and proper assistants, to go directly within the inclosure. Having but few trained females present, it was judged advisable to send in a fine male elephant, taken many years before, and thoroughly broke in, to assist them, as well as to keep the herd in awe. He had no sooner entered the inclosure, and been brought near the herd, than, discovering one of the females to be in heat, impelled by desire, and eager to cover her, he dashed through the herd, regardless of the orders and severe discipline of the driver, and had nearly accomplished his purpose. The driver, being alarmed for his own safety, exerted in vain all his strength, to turn him, and bring him from among the wild elephants; but the drivers of the trained females, coming speedily to his assistance, soon surrounded this furious animal, and separated him from the herd. In resentment, however, of his disappointment, he attacked a small *koomkee*, with such violence as completely overturned her and her rider; and, had he not been of a particular species, called *mucknah*, which have only small tusks, he most probably would have transixed, and killed her on the spot: fortunately, neither she nor her driver received any considerable hurt. This accident prevented the trial being then made, to tie the wild elephants in the manner proposed.

Reflecting on the disobedience shown by an elephant remarkably docile, and which had

* *Vide Asiatic Researches*, Vol. III. article, "Method of catching wild Elephants;" where this process is particularly described.

† Though fully convinced of this, I could not bring the hunters to adopt the Chittigong method, till the year 1794. After this, during the last three years I remained at Tiperah, I did not lose one elephant in twenty; whereas, by the former method, of tying them in the *roomee*, near one-third of those taken died in less than a year, in consequence of the hurts they received from their violent efforts to get free, before they could be properly secured. The natives of Tiperah, and indeed of most parts of India, are extremely attached to old customs; and it was with the utmost difficulty I prevailed on the hunters to deviate from the practice of their ancestors, though the method recommended was followed at Silhet, as well as at Chittigong. The method was, simply to surround a herd, in the first convenient place, with a ditch and palisade; and, when this was finished, to send in the *koomkees*, and proper persons to tie the wild elephants on the spot, and then conduct them, one by one, through an opening in the palisade, from the *keddah*, as soon as they were tied,

been domesticated for many years, when his passions were excited, and recollecting also, that a wild elephant had covered a female, in February, 1778, before many spectators, just after the herd had been secured in the inclosure, I was assured in my own mind, that it was not from any sense of modesty, either wild or tame elephants did not gratify their passions in public; but no opportunity offered of prosecuting this inquiry, till 1792. Having then taken upon myself the management of the elephant hunters, a very fine male was caught in November: he was both young and handsome, and also of a most docile disposition; I therefore promised his driver a considerable gratuity, if he would get him into high order, so that I might have an opportunity of bringing his procreative powers to trial, with a tame female.

In the month of March, 1793, the driver of a favourite female elephant informed me, that she had then signs of being in heat; and that, if the male and she were kept together, and highly fed, an intimacy would probably soon take place. They were therefore, shortly after this, brought near to Comillah, where a spacious shed was erected for their accommodation.

In the day, they went out together, to feed; they also brought home a load of such succulent food as their drivers and attendants could collect. After their return, they stood together, slept* near each other, and every opportunity was granted them to form a mutual attachment. In the evening, they had each from ten to twelve pounds of rice soaked in water, to which a little salt was added; and, from the middle of May till the latter end of June, some warm stimulants, such as onions, garlic, turmeric, and ginger, were added to their usual allowance of rice. Long before this, however, a partiality had taken place, as was evident from their mutual endearments, and caressing each other with their trunks; and this without ceremony, before a number of other elephants, as well as their attendants.

Near the end of June, I was satisfied the male would not, even to regain his freedom, quit the object of his regard; I therefore ordered the keepers to picket the female, by one of her fore-legs only, in the house where they stood, but to leave the male at full liberty. Fearful, however, of hurting their supposed delicacy, and thinking the nearness and sight of the attendants might possibly give umbrage to their modesty, I desired them to remain quiet in a little hut, erected on the outside of the building appropriated to the elephants, where they could see equally well as if nearer.

On the evening of the 28th of June, 1793, the male was let loose from his pickets; and, soon after, he covered the female without any difficulty, although before this she never could have received the male, being taken when very young, about five years and a half prior to this period. The male was then led quietly to his stall; but, early on the morning of the 29th, he became so troublesome, that the drivers, in order, as they said, to quiet him, but

* It is always a good sign, when an elephant lies down to sleep, within a few months after he is taken; as it shews him to be of a good temper, not suspicious, but reconciled to his fate. Elephants, particularly *goon-dabs*, have been known to stand twelve months at their pickets, without lying down to sleep; though they sometimes take a short nap standing.

partly, I suspect, to indulge their own curiosity, permitted him to cover her a second time; which he readily did, before the usual attendants, as well as a number of other spectators. After this, the driver brought me a particular account of the whole process. Though much pleased with the success of the experiment, yet I was rather chagrined he had not given me notice, that I might have been myself an eye-witness; and therefore told him, he should not receive the promised reward, till I had satisfied myself of the fact.

About two in the afternoon of the same day, I was desired to repair to the place where the elephants stood, as the male had been trying to get nearer the female. On this, I proceeded to the spot, with my friend captain Robert Burke Gregory: when we arrived, I ordered the male to be freed from his shackles; and, after some toying, and a few mutual caresses, we had the satisfaction to see him cover the female.

When the male mounted, he placed one of his fore-legs on each side of her spine, with his feet turned to, and pressing against, her shoulders, and his trunk over her forehead; supporting himself firmly in this situation, during coition, which he continued nearly the same time, and in the same manner, as a horse with a mare.

The female remained perfectly still, during the coitus. When the male had finished, he stood quietly by her side, while she caressed him with her trunk; and, as they then appeared well pleased, and gentle as usual, I went up and patted them both, as I had formerly been accustomed to do, without the smallest apprehension. In the evening, they were brought home to be fed; and, though only a few hours had elapsed since his last embrace, the male seemed inclined to make another attempt; to which I would have consented, to gratify a crowd of people then present, had I not now learned, that he had covered the female in the open plain, about ten in the morning, when going out for food, in spite of the exertions of the drivers and attendants; at least so they alleged, in excuse for having permitted it, contrary to my orders. As he had already covered four times in about sixteen hours, I was afraid a further indulgence might be prejudicial, and therefore would not permit it; especially as Mr. Imhoff, to whom he then belonged, was absent. That gentleman, however, returned two days after: but, when the two elephants were brought together, in order that Mr. Imhoff's curiosity might be indulged with so novel a sight, the female, being no longer in heat, was so uncivil as to give the male a kick in the face, when he was using what she then thought improper liberties; nor did she afterwards permit him to cover her, though, when standing together, they mutually indulged in a few caresses.

(To be continued.)

III.

Description and Use of a portable Instrument for comparing the Force of Gunpowder. By
Citizen REGNIER.*

IN processes for the improvement of gunpowder, it is necessary to make comparative experiments. Different contrivances, exhibiting various degrees of ingenuity, have been made use of for this purpose, which are too well known to require description in this place. I shall, therefore, only remark, that trials of gunpowder, on a large scale, are always the best; but as these experiments require apparatus and conveniences, which are not in the power of every one, a small powder-proof has long since been received and commonly used, which is constructed in the form of a pistol, the blast from which drives a small toothed, or turned, wheel, which rubs against a spring, and is moved through a greater or less space, according to the force of the powder. But this machine, which is very defective in its results, and can be of no value when an absolute, or at least approximate, indication is required of the relative forces of a given measure of powder intended for fire-arms, which are charged with quantities determined in that manner.

In fact, these proof instruments have an arbitrary graduation. Their friction varies according to the force of the spring, and the cleanness or oxydation of the mechanism.

To obviate these different inconveniences, I made a number of experiments, which led me to a more valuable, and, in fact, a much more accurate principle; and, at length, with some modifications, I adapted a small brass cannon to the spring of the common weighing instrument. By this application, I immediately acquired the means of weighing the effort of the blast, and, consequently, a comparable method of estimating its action. This machine has likewise the valuable property of operating without friction. Its graduation is justly deter-

* From *Mémoires explicatifs du Dynamometre et autre Machines inventées par le C. Regnier.* 36 Pages, in Quarto, printed at Paris in the Year VII. The instruments described in this pamphlet are; 1. The dynamometer, which, by means of apparatus connected with a strong spring, measures the reaction exerted against the powers of men, horses, and other agents, at work. 2. A safeguard for the priming, in musketry. It consists of a cylindrical piece in brass, out of which the cavity, or pan for holding the priming, is excavated, and another hollow cylindrical piece, which covers the first, and is cut through in such manner as to present an opening corresponding with that of the pan. When this hollow cylinder is so disposed, that the two openings correspond, the musket may be discharged; but when, by turning the external part half round, the pan becomes completely covered, the priming is very secure, and the piece cannot be discharged either by accident or design. 3. The powder-proof, which is the subject of the present article; and, 4. An electrical machine. This machine operates by the friction of a circular plate. The principal singularity of this apparatus, is the conductor, which is not cylindrical, but has the form of a flat table, with thick rounded edges to prevent the escape of the electric fluid. A plate of coated glass is attached to the lower surface, which answers the same purposes of convenience and advantage, as the jars which Nairne put into cylindrical conductors about sixteen years ago. Electricians will not require that the conveniences of the flat conductor, or table, should be here pointed out. N.

mined, because it expresses the weight which served for its graduation, and the instrument may likewise be usefully applied for weighing such bodies as fall within the limits of its scale.

The little attention I paid to render this instrument public, and the prejudice which, in most cases, gives a preference to things brought from foreign countries, have caused it to be designated as the English or German powder-proof; but the fact is, that I invented it myself in Burgundy, where, in the time of the academy of Dijon, it was considered as the best instrument, on a small scale, for determining the strength of gunpowder.

Description. Plate IX. fig. I. A. A perspective view of the instrument. B. C. A spring bended into an angle. D. A small cannon of brass, containing exactly a gramme of fine gunpowder ($15\frac{1}{2}$ grs). E. Arc of division graduated into kilogrammes (nearly 2lbs and a quarter avoirdupois each), terminating in a screw, which serves as the breech-pin to the cannon. F. A cap, which may be considered as the projectile. It closes the mouth of the small cannon, upon which it presses with a force equal to four kilogrammes. This stopper is firmly fixed to one of the spring radii, by means of a nut at the opposite end of its branch or tail. G. A piece of hard brass wire fixed to a projection, rivetted into one of the radii, by means of a small screw. H. An index of woollen cloth, or leather, which slides by a gentle friction upon the wire, when the branches of the spring are pressed together by the inflammation of the powder. This index remains at the place to which it has been pushed, and shews precisely the distance to which the springs have been made to approach.

Method of using this instrument. 1. The two extremities of the spring are to be pressed together, in order to separate the cap from the mouth of the small cannon. 2. Powder is poured either by means of card or paper into the cannon, so as exactly to fill it; the stopper is then suffered to apply itself gently to the muzzle, and close it exactly, without leaving any grain of powder between them. 3. The circular piece of cloth, or leather, which serves as the index, must be moved into contact with that branch of the spring, to which the tail of the stopper is fixed. 4. Priming being then put into the small pan of the cannon, it must be discharged, holding the instrument suspended by the ribband or string which is passed through the angular bend.

The effects of the explosion are, that the powder occupying a greater space by its inflammation, drives back the stopper, which carries with it that branch of the spring in which its tail is firmly fixed. This branch cannot move relatively to the other, which is also moved by the recoil of the gun, without driving before it the small index piece, and the space through which this is carried will shew the force of the powder.

The arcs moved through by the explosion of different samples of powder, will shew, by the numbers of their graduations, the comparative forces of each. There is a star marked on the graduated arc which shews the force of powder of a medium quality, in order that the relative value of any sample, with regard to that medium, may be known. The friction of the index is the only friction to which this instrument is subject, and this is so slight, that it may be considered as nothing. The index itself may be easily renewed when worn out.

It is obvious, that by adding a hook to the perforation at the extremity of the divided arc, and a ring in the eye of the stopper, this instrument may be used for weighing: the divisions are half kilogrammes; (they might conveniently be pounds avoirdupois).

IV.

An Account of some Endeavours to ascertain a Standard of Weight and Measure. By Sir GEORGE SHUCKBURGH EVELYN, Bart. F.R.S. and A.S. Continued from p. 157.

(§. 35.) **T**HE chief standards of longitudinal measure, as far as I can learn, that carry any authority with them, are those preserved in the exchequer; in the house of commons; at the Royal Society, and in the Tower. The first alone, indeed, bear legal authority, and have been in use for more than 200 years; the last is considered as a copy of them, and is not used for sizing generally. The two remaining ones are of modern date; and, although they do not carry with them *at present* any statuteable authority, yet, from the high reputation and acknowledged care of the artists who made them (the celebrated Mr. George Graham, and Mr. John Bird), are undoubtedly entitled to very great respect; and are probably derived from a mean result of the comparisons of the old and discordant ones in the exchequer. I shall begin with that of Mr. Graham, which contains also the length of the Tower standard laid down upon it; will proceed then to Mr. Bird's, and finally conclude with those at the exchequer.

(§. 36.) May 5, 1797. I went to the apartments of the Royal Society, at Somerset House, and, with the ready assistance of Mr. Gilpin, at the kind instance of sir Joseph Banks, I made the following observations on Mr. Graham's * brass standard yard, made in 1742. This scale is about 42 inches long, and half an inch wide, containing three parallel lines engraven thereon, on the exterior and interior of which are three divisions, expressing feet, with the letter E at the last division; and, by a memorandum preserved with it in the archives of the society, is said to signify English measure, as taken from the standard in the Tower of London. That with the letter F denoting the length of the half of the French toise, put on here, by the authority and under the inspection of the Royal Academy of Sciences, then subsisting at Paris, to whom it was sent in 1742, for the purpose of comparing the French and English measures. The middle line, marked Exch. of the three abovementioned, denotes, as is supposed, the standard yard from the exchequer.

(§. 37.) This bar of Mr. Graham's had been previously laid together with my scale divided by Mr. Troughton, for twenty-four hours, to acquire the same temperature; they were also of the same metal, and, by placing it under my microscopes, adjusted to the interval between 10 and 46 inches, I found the interval on the Tower standard exceed

* This rod was not made by Mr. Graham, but, at his instance, procured by him from Mr. Jonathan Sisson, a celebrated artist of that time. See Phil. Transf. Vol. XLII.

$$\begin{array}{r} \text{mine, by — } 0,00127 \\ \quad ,00135 \\ \quad ,00128 \\ \hline \text{Mean = } ,00130 \end{array} \left. \vphantom{\begin{array}{r} 0,00127 \\ ,00135 \\ ,00128 \\ \hline ,00130 \end{array}} \right\} = \text{the total length therefore } 36,00130 \text{ inches, the therm. at } 60^{\circ},8.$$

The interval on the line marked Exch. was shorter than mine

$$\begin{array}{r} \text{by — } ,0066 \\ \quad ,0066 \\ \quad ,0068 \\ \hline \quad ,0067 \end{array} \left. \vphantom{\begin{array}{r} ,0066 \\ ,0066 \\ ,0068 \\ \hline ,0067 \end{array}} \right\} = \text{the total length} = 35,9933 \text{ inches, the therm. at } 60^{\circ},6.$$

And the Paris half-toise, which had been supposed by the Academy to be = 38,355 English inches, was found, compared

$$\begin{array}{r} \text{with mine, to be = } 38,3561 \\ \quad ,3563 \\ \quad ,3559 \end{array} \left. \vphantom{\begin{array}{r} 38,3561 \\ ,3563 \\ ,3559 \end{array}} \right\} \text{Mean} = 38,3561^*.$$

* Dr. Maskelyne says, this standard yard of Mr. Graham's was $\frac{1}{1000}$ inch longer on three feet than Mr. Bird's divided scale, which he generally made use of in all his operations of dividing; and, from one made conformably to this of Mr. Bird's, Mr. Troughton divided my scale of 60 inches. This remark seems to agree with my 1st and 3d comparison, but not with the intermediate one. See Phil. Transf. for 1768, p. 324.

As I am now upon the subject of foreign measure, it may not be quite out of place to say a word on the length of the ancient Roman foot, which I am enabled to do with some precision.

Some years ago, when I was in Italy, I had several opportunities of ascertaining the length of this measure, by actual examination of the Roman foot rules, of which I have met with nine, viz. two in the Capitol at Rome; one in the Vatican; five in the museum at Portici, near Naples; and, lastly, one in the British museum, sent from Naples by sir William Hamilton. They were all of brass, except one half-foot, of ivory, with a joint in the middle, resembling our common box or ivory rules: and, by reference to my journal kept at that time, I find the mean result from all the nine rules, viz. by taking both the whole and the parts of each (for they were divided into 12 inches, and also into 16ths, or digits), gave, for the length of the old Roman foot, in English inches, correspondent to Mr. Bird's measure, = 11,6063.

In confirmation also of this conclusion, and agreeably to the idea of M. de la Condamine, in the "Journal of his Tour to Italy," I took the dimensions of several ancient buildings, viz. the interior diameter of the temple of Vesta; the width of the arch of Severus; the door of the Pantheon; and the width of the base of the quadrilateral pyramid of Cestius, which, it is curious to observe, I found exactly 100 old Roman feet, and 125 feet high. This I do not remember to have seen noticed by any former traveller.

The mean result of these experiments gave me — — — — 11,617 English inches.
 Ditto, as before, from the rules — — — — 11,606 ditto.
 The mean of the two modes of determination is — — — — 11,612 ditto.


I may add, that in the Capitol is a stone, of no very ancient date however, let into the wall, on which is engraven the length of several measures, from whence I took the following:

The ancient Roman foot, = 11,635 English inches.
 The modern Roman palm, = 8,82 ditto.
 The ancient Greek foot, = 12,09 ditto.

The

	inches.
The 1st of the preceding observations giving	36.0013
The 2d	35.9933
The mean length of Mr. Graham's standard becomes	35.9973

(§. 38.) From the information in the report of a committee of the house of commons, that at in the year 1758, I learnt that Mr. Bird's parliamentary standard had been in the custody of some of its officers, but of whom nobody knew: however, under the authority of the speaker, who was so good as to furnish me with a room in his house, to make the comparisons in, I at last discovered this valuable original in the very safe keeping of Arthur Benson, esq. clerk of the journals and papers, and which, I believe, had never seen the light for five-and-thirty years before. It is a brass rod or bar, about 39 inches long, and 1 inch

square, inclosed in a mahogany frame, inscribed "standard  1758;" at each extremity of

Geo. 2d.

it is a gold pin, of about $\frac{1}{8}$ inch in diameter, with a central point, and these points are distant = 36 inches. It bears, however, no divisions; but there was found with it, in another box, a scale divided into 36 inches, with brass cocks at the extremities, for the purpose of sizing or gauging other scales or rules by. Besides these, I found another standard, in size, and in all respects, similar to the last, inscribed 1760, having been made for another committee, that sat in that year; this also was accompanied with a similar divided scale of 36 inches.

These bars being too thick to be conveniently placed under the microscopes of my instrument, the interval of 36 standard inches was laid down on my scale with a beam-compass, two fine points made, and, compared with Troughton's divisions, was = 36.00023 inches; the thermometer being at 64°. I then examined the other standard, marked "standard, 1760," and found it to agree exactly with that of 1758; at least it did not differ from it more than .0002 inch*.

(§. 39.) I was now to examine the old standards kept in the exchequer: these Mr. Charles Ellis, deputy chamberlain of the tally court at the receipt of the exchequer, was so good as to supply me with; viz. the standard yard of the 30th of Eliz. 1588, and also the standard ell of the same date. These are what have been constantly used, and are indeed the only ones now in use, for sizing measures of length†. They are made of brass, about 0.6 inch square, and are very rudely divided indeed, into halves, quarters, eighths, and sixteenths; the lines being two or three hundredths of an inch broad, and not all of them drawn square, or at right angles to the sides of the bar, so that no accuracy could possibly be expected from such measures. However, the middle point of these transverse lines, between the sides of the bar, was taken as the intended original division; and these divisions, such as

* These quantities then being so small, I shall consider them as wholly insensible; and shall say, that Mr. Bird's parliamentary standards of 3 feet exactly correspond with Mr. Troughton's scale.

† There was also a standard yard of Henry VII. but of very rude workmanship indeed; now quite laid by, and at what time last used, no information remains: but of this more hereafter.

they were, were transferred, by a dividing knife, to the reverse side of my brass scale made by Mr. Troughton, the thermometer being at 63°; and, at my leisure afterwards, I found as follows.

The ends of these venerable standards having been bruised a little; or rounded, in the course of so many years' usage, I conceived a tangent to be drawn to the most prominent part, which was about the centre or axis of the bar, and this point being referred to Troughton's scale, between 6 and 42 inches, the entire yard of 1588, measuring from one extremity to the other, was found to be shorter than this, by —,007 inch: but these comparisons will be better exhibited in a table.

Exchequer standard of 1588.	Difference from Troughton.	Length in inches.	Difference on 36 inches.	Mean difference on 36 inches.
Entire yard - -	—,007	35,993	—,007	Inches: = + 0,015
$\frac{1}{2}$ yard, from 24 to 42 inches	+ ,063	18,063	+ ,126	
$\frac{2}{3}$ yard, from 15 to 42 inches	—,008	26,992	—,011	
$\frac{7}{8}$ yard, from 10 $\frac{1}{2}$ to 42 inches	+ ,022	31,522	+ ,025	
$\frac{1}{2}$ yard, from 8 $\frac{1}{2}$ to 42 inches	—,055	33,695	—,059	= + 0,016 viz. the exchequer measure is by so much the longer, or about 1 in 2322.
Entire ell, from 2 to 47 inches	—,036	44,964	—,029	
$\frac{1}{2}$ ell, from 2 to 24 $\frac{1}{2}$ inches	+ ,032	22,532	+ ,052	
$\frac{3}{4}$ ell, from 2 to 35 $\frac{3}{4}$ inches	+ ,017	33,767	+ ,018	
$\frac{7}{8}$ ell, from 2 to 41,375 inches	—,001	39,374	—,001	
$\frac{1}{2}$ ell, from 2 to 44,1875 inches	+ ,051	42,239	+ ,043	

(§. 40.) It appears then, from the above table, that the ancient standards of the realm differ very little from those that have been made by Mr. Bird, or Mr. Troughton, and consequently, even in a finance view (if one might look so far forward), nothing need be apprehended, of loss in the customs, or excise duties, by the adoption of the latter.

(§. 41.) I shall now endeavour to shew the proportion of the weights that I have used, compared with the standards that were made by Mr. Harris, assay master of the mint, under the orders* of the house of commons, in the year 1758. They are kept in the same custody with Mr. Bird's scales of length, and appear to have been made with great care, as a mean result from a great number of comparisons of the old weights in the exchequer, which have been detailed at length in that report. Mr. Harris having been of opinion that the troy pound was the best integer to adopt, as the standard of weight, I venture to conclude that this was the most accurate, and most to be depended upon, of all the various weights and duplicates that he made for the use of this committee; for he made them of 1, 2, 4, 8, 16, lb. and of $\frac{1}{2}$, 1, 2, 3, 6 ounces. It will therefore be sufficient for my purpose, to compare the 1 and 2 pounds troy, and their duplicates, with the weights of Mr. Troughton.

I did this, June 2d, 1797; the barometer being at 29,72 inches, and thermometer 67°.

* See the report referred to in the note of page 106.

		Troughton's weights.	
		lb. grains.	grains.
The standard weight of 1 troy pound, or 5760 grains, marked 1758, kept at the house of commons, in a small box by itself, by Mr. Benfon, weighed		= 1 3,75 } = 5763,745	
A duplicate of the preceding, kept with some other weights, in a box marked B		= 1 3,70 } = 5763,685	
The mean weight of the troy pound, from these two		= 5763,715	
The two-pounds weight, from the house of commons, kept in a deal box, marked A		$\left\{ \begin{array}{r} 10000 \\ 1000 \\ 400 \\ 100 \\ 20 \\ 7 \\ 0,84 \end{array} \right\} = 11527,84$	
A duplicate of the last-mentioned 2lb. weight, preserved in a deal box, marked B		$\left\{ \begin{array}{r} 10000 \\ 1000 \\ 400 \\ 100 \\ 20 \\ 7 \\ 0,55 \end{array} \right\} = 11527,55$	
The thermometer now stood at 68°.			
Therefore the mean weight of 2lb. troy, from the two last trials, is		= 11527,70	
And consequently 1lb. becomes		5763,85	
But, from the examination of the two single pound weights, as above, 1 pound is		5763,71	
Therefore the mean of all is		= 5763,78	
That is, Mr. Troughton's weights are too light by $\frac{278}{576000}$		= 0,6562	
grain on 1000 grains, or 1 in 1523,92 grains.			

(§. 42). In conclusion, it appears then that the difference of the length of two pendulums, such as Mr. Whitehurst used, vibrating 42 and 84 times in a minute of mean time, in the latitude of London, at 113 feet above the level of the sea, in the temperature of 60°, and the barometer at 30 inches, is = 59,89358 inches of the parliamentary standard; from whence all the measures of superficies and capacity are deducible.

That, agreeably to the same scale of inches, a cubic inch of pure distilled water, when the barometer is 29,74 inches, and thermometer at 66°, weighs 252,422 parliamentary grains; from whence all the other weights may be derived.

As a summary of what has been done, I hope it may now be said, that we have attained these three objects;

1st. An invariable, and at all times communicable, measure of Mr. Bird's scale of length, now preserved in the house of commons; which is the same, or agrees within an insensible quantity, with the ancient standards of the realm.

2dly. A standard weight of the same character, with reference to Mr. Harris's troy pound.

3dly. Besides the quality of their being invariable (without detection), and at all times com-

communicable, these standards will have the additional property of introducing the least possible deviation from ancient practice, or inconvenience in modern use.

(§. 43.) Before I close this paper, after having said so much on the subject of weights and measures, it may not be improper to add a few words upon a topic that, although not immediately connected, has some affinity to it; I mean the subject of the prices of provisions, and of the necessaries of life, &c. at different periods of our history, and, in consequence, the depreciation of money. Several authors have touched incidentally upon this question, and some few have written professedly upon it; but they do not appear to me to have drawn a distinct conclusion from their own documents. It would carry me infinitely too wide, to give a detail of all the facts I have collected; I shall therefore content myself with a general table of their results, deduced from taking a mean rate of the price of each article, at the particular periods, and afterwards combining these means, to obtain a general mean for the depreciation at that period; and lastly, by interpolation, reducing the whole into more regular periods, from the conquests to the present time: and, however I may appear to descend below the dignity of philosophy, in such æconomical researches, I trust I shall find favour with the historian, at least, and the antiquary *.

(The author's appendix hereafter.)

V.

Description of a new Arrangement of the Bars in the Gridiron Pendulum. With a Drawing.
(W. N.)

FIG 2, 3, 4, &c. in plate IX. represent a pendulum which I constructed some months ago. It differs only in the arrangement from the modern gridiron pendulum of five bars. The same letters denote the same parts in all the figures. A A is a cylindrical box of brass, filled with lead, constituting the principal mass of the pendulum. In fig. 4 a section of this piece is seen in the section of its axis. A piece of brass, P P P P, is let through the middle of this cylinder and firmly secured by a flanch piece screwed to the brass face. Two holes are made quite through the cylinder, and the piece of brass P P, parallel to the diameter of its face, for the purpose of suffering the two steel rods B B to pass through. Two steel pieces, L and M N O, are fitted to square cavity in the piece P P. These pieces, of which fig. 7 gives a vertical section, and fig. 8 the section parallel to the horizon, are applied together, by passing the screw M through the end hole in L, and then screwing on the milled head or nut K, in which situation the two steel pieces fit the square hole; the vertical hole in L being applied over the long hole N, and nearly coinciding with one of the diametral perforations in A A, while the other, O, nearly coincides with the other perforation. If

* This table has already been given in our Journal, vol. II. 284.

in this situation, while the nut K is but loosely screwed on, the bars B B be passed through the perforations in the cylinder, the brass and steel pieces, and K, be then screwed up, it will cause the vertical hole O, in the one steel piece, and L in the other, to approach nearer together, and carry the bars B B along with them. But this can take place only to a very small extent, because the bars will be pressed against the inner surfaces of the holes in the brass piece P-P, which very nearly fit those bars. The use of the milled head K is, therefore, to disengage the weight A A from the bars B B, in which situation the former may either be taken off, or slid up and down to any desired position, where it may immediately be fixed by turning the nut.

Fig. 2 shews the whole pendulum, which vibrates half seconds, in its full dimensions, excepting as to its length, which is diminished by a fracture, represented in the middle of the bars, to get it into the plate. The system of bars consists of a cross-piece, G G, attached to another piece, H H, to which the springs of suspension are fixed as usual. From G G proceed downwards two steel bars, which pass through holes in the brass stage E E (represented also in fig. 5), with which they have no connection, and are firmly secured in the stage, D D, which is similar to E E. From the stage E E, proceed a light pair of steel bars, downwards through the stage D D, without connection with this last, and thence through the cylinder A A, where they appear at B B. And, lastly, from the centre of the lower stage D D, proceeds upwards a bar of the same dimensions, but composed of zinc and silver, forming a compound, which expands more than twice as much by heat as steel does. This bar passes through the stage E E, till it almost reaches G G. It is represented by the dotted outline. The stage E E may be slid up and down, so as to vary its distance from D D; but when required to be fixed, the screw pin F is to be turned, which causes a piece to press against the central bar and binds it fast.

It is scarcely necessary to explain this action. The pendulum A A hangs upon the steel bars B B, which are fixed in the stage E E. This stage is supported by the central rod or pillar of zinc and silver which bears on the stage D D, and this last is suspended from G G by the bars C C. If, therefore, the expansion of the compound metal between the stages E and D be equal to the whole expansion of the steel rods, and the rest of the apparatus downwards, the centre of A A will remain at the same distance from the axis of motion whatever may be the temperature. But if the effect of the steel be greatest, it will be necessary to raise the stage E and fix it higher, or in the contrary case it must be fixed lower; taking care to preserve the due situation of A A with respect to I, by means of its apparatus K.

This may seem to be but a rough method of adjusting for temperature. But when it is considered that the whole daily difference in a simple pendulum with a steel rod amounts to six seconds at a mean between summer and winter*, it will be readily understood that the

purpose of compensation for temperature will be obtained, as nearly as observation can indicate its changes, by making the lengths of the bars inversely as their expansions, as known from general experiment, or from tables. Thus, for example, if brass and steel were used, and their expansions taken as 113 to 68, and the whole length of the brass were 10 inches, we may conclude that the changes of temperature would not alter the pendulum, provided the above proportions were accurately kept, namely, that the length of the steel were 16.62 inches. But if the proportions either of expansion or length should be so far mistaken or neglected, as that the brass should be made one-tenth of an inch too short, = 9.9 inches, this piece would compensate only 16.56 inches of steel, and consequently (the steel being lengthened 0.1 inch) there would remain 0.16 inch of steel uncompensated; or 0.016 part of the whole pendulum, which would produce a difference of less than one-tenth of a second between the mean daily rates in summer and winter.

The adjustment for time is made by a screw adapted to the springs of suspension, or by any other of the usual methods; of which I do not mean at present to consider the advantages and defects.

VI.

*On the Epocha of the Discovery of the Telescope, and the Opinion of Boyle, that Plants derive their Nourishment from Water only. By Citizen BOISSONADE *.*

WE sometimes find in books which are little known and read, particulars of information of a very interesting nature. I have found in the *Probabilia* of the learned Wesseling, editor of Diodorus Siculus, a chapter (the eleventh) on the antiquity of astronomical telescopes, of which I send you a translation. The researches it contains appear sufficiently curious to deserve to be better known.

The astronomical telescope †, by means of which the moderns have made discoveries in the heavens, which escaped the penetration of the ancients, is, with justice, placed among the most memorable inventions. It has often been enquired whether they were known to the ancient astronomers, and several writers have decided for the affirmative. They ground their opinion on an ancient drawing in the scholastic history of Peter Comestor, in which Conrad, a monk of the thirteenth century, had represented astronomy ‡. “On the right hand of the figure,” according to father Mabillon, who had seen the manuscript, and mentions it in his travels in Germany, there was “a figure of Ptolemy observing the stars, with a long instrument resembling an astronomical telescope with four sliding tubes §.” This conjecture

* In a letter to citizen Millin, inserted in the *Magasin Encyclopédique*, v. 466.

† I thus translate the words *tubi optici*. Note of the writer.—His object is to avoid the ambiguity of the French word *lunettes*, which signifies *spectacles* as well as *telescopes*. I have not the original work. N.

‡ The Latin of Mr. Wesseling is not clear in this passage, for the understanding of which, it is necessary to refer to the work of P. Mabillon, from which he quotes only a few lines.

§ Ope instrumenti longioris quod instar tubi optici, quatuor ductus habentis, concinnatum est.

was fortified by a passage of Ditmar, bishop of Merfbourgh, who, at the end of his sixth book, speaking of Gerbert, afterwards pope Silvester II. expressed himself thus: "He perfectly knew the course of the stars, and exceeded his cotemporaries in the variety of his knowledge. When driven from his own country, he repaired to the emperor Otho, at whose court he remained a long time. He constructed a clock at Magdebourgh, and to ascertain its accuracy, he observed with a tube the star which directs mariners *." It is thought that Ditmar, by the word tube, meant the astronomical telescope, which, in fact, has a tube. Strabo also mentions a tube of this kind, if we admit that the text is not in this place corrupted. "The disc of the sun acquires a more considerable apparent magnitude at the rising and setting of that luminary out at sea, on account of the quantity of vapours which rise from the water, and because the light, refracted through these vapours, *in the same manner as through tubes* †, receives the images of a larger size."

These passages appear to me to be the strongest which have been brought in proof that telescopes were known to the ancients. But it is very surprizing that this knowledge, if it existed, could have been lost to such a degree, that before the seventeenth century the most skilful and learned astronomers should not even suspect its existence. Men have, at all times, applied to study the course and position of the stars. No one is ignorant with what ardour the Greeks, the Arabs, and the Latins, cultivated astronomy, and how much this study, encouraged by the liberality of princes, has been constantly pursued. Can it be supposed that the ancient observers despised an instrument so useful to their labours? I admit that much of the knowledge of antiquity has been lost by wars, by ravages, and a multiplicity of other causes; but if this subject be attentively examined, it will be seen that the information which has been lost, relates either to objects of luxury, or is of such a nature, that greater subsequent discoveries must have caused them to be neglected; and still they are not so lost but that some trace of their ancient existence remains. But this is not the case with telescopes; for we have no description in the writings of the ancients which can agree with them. Strabo speaks of tubes *αυλων*; but nothing leads us to apprehend that they were used to observe the magnitude and course of the stars; for I cannot say that I place much dependence on the conjecture of Theodore Almelovent, who thinks that Strabo wrote *ὕαλων* (glasses). In fact, crystal, or glass, is particularly endued with the property mentioned in this passage of the geographer. But let us suppose that Strabo wrote *αυλων* (tubes), who shall assure us that these tubes were provided with glasses? The same question may be offered respecting the painting of the monk Conrad. In fact, though it represents an instrument considerably resembling a telescope, who can prove that this instrument contained glasses like our telescopes, which without them would be of little utility? I am almost certain that tube of Gerbert, of which Ditmar speaks, had them not; and in order that I may not be accused of deciding easily, I offer the following proof. Father Mabillon, in his *Analecta*, has

* Considerata per fistulam quadam stella nautarum duce.

† *ὡς ἐν αὐλῶν*. Lib. 3. p. 138.

published a letter of Gerbert to the monk Constantin, on the construction of the sphere, in which he says, “ so that there will be seven perforations, in each of which you will place tubes (fistulas) of six inches. The two extremes, or most remote, are to be placed opposite each other in such a manner that both may be seen through as if composing one tube.— These tubes differ from musical pipes (fistulis organicis) in the equality of their diameter throughout, for fear lest any obstacle should impede the sight while occupied in observing the celestial bodies.” If these tubes differed from those of musical instruments only in the uniformity of their bore, it is clear that they had not glasses; consequently they were not telescopes, but dioptræ, and the glory of the invention of telescopes must continue to belong to the seventeenth century.

I do not consider the celebrated opinion of Vanhelmont and Boyle as equally new, who, from well-made experiments, thought that trees and plants derive their principal nourishment from water. These great observers have informed us, that if a determinate quantity of earth be weighed, and if a twig be planted in the vessel containing this earth, it will be found several years afterwards upon transplanting the tree, that the same quantity of earth will remain as at first. Several centuries before Boyle and Vanhelmont, the author of the Clementine Recognitions had the same idea. His words, book viii. chap. xxvii. are: “ In order to prove by experiment that seeds draw nothing from the earth, but are entirely supported by water, let us suppose that the weight of one hundred talents of earth to be put into a very large receptacle, and that the seeds of various species of plants be sowed therein, and abundantly watered, for several years; let us, moreover, suppose that all the seeds of wheat, barley, and other vegetables, thus annually produced, to be collected until the quantity of grains of each shall weigh one hundred talents; if at this period the roots be drawn out, there will still be found in the vessel from whence so great a product has been obtained, the original weight of one hundred talents of earth. Whence can we say that this quantity of seeds and trees was formed? Is it not evident that it has been produced from the water?” This passage contains exactly the system of Boyle and Vanhelmont. I do not, however, think they have borrowed it. Accurate observers of nature are led by their observations to very similar conclusions.

So true it is, as Terence affirms, that,

Nullum jam dictum est quod non dictum sit prius.

I may add to this chapter of Mr. Wesseling, which has been inserted in the *Journal des Savans*, for February, 1679, the different proofs of the existence of telescopes in the thirteenth century. It would follow also, from a passage of John, abbot of Beaugency, that spectacles were used for reading in the twelfth century, if the word *bustula*, used by this abbot, may be translated spectacles. The following is the passage: “ Statim ut litterarum vestrarum bajulam vidi, bustulam arripiens, non solum avide legi et perlegi *.” The *Journal Encyclopédique*,

* This passage has very often been quoted in favour of the invention of spectacles in the thirteenth century. It is thought, nevertheless, with much probability, that the word *bustula* comes from the word *buxus*.

pédique, for May, 1766, in which there is a long article on the subject, gives his sense to the word, and adds, that the principles, according to which spectacles are fabricated, are found in Euclid and the ancient geometers.

VII.

*Observations on the Proportion of real Acid in the three ancient known mineral Acids, and on the Ingredients in various neutral Salts and other Compounds.** By RICHARD KIRWAN, Esq. LL.D. F.R.S. and M.R.I.A.

THE fundamental experiments on which the proportion of real acid in the three mineral acids anciently known, and also the proportion of ingredients in many neutral salts, were determined, are set forth in a paper of Mr. Kirwan, in the IVth vol. of the Transactions of the Irish Academy. That paper contains tables of the quantity of standard acid existing in 100 parts of each of the acid liquors, of given specific gravities, and also in each of the neutral salts therein mentioned, according to a method which Mr. Kirwan has since discovered to be very inconvenient, because some of these neutral salts contain an acid still stronger than the assumed standard. It is there also noticed, that the strongest vitriolic acid now known exists in *vitriolated tartarin*, the strongest nitrous acid in *nitrated soda*, and the strongest muriatic acid in *muriated tartarin*. Acids of such strength he has, therefore, denominated *real*, as either containing no water, or containing only as much as is necessary to their essential composition, as far as is at present known. The method of transforming the expression *standard* into that of *real*, is given in the same paper at p. 67, and by it he has formed the table here presented. This latter expression he therefore employs in every case instead of that of *standard*, together with the substitution of a more commodious expression of the strength of acids. Another object of his present communication is, to exhibit an illustration or amendment of several of the determinations contained in his last, which, being for the most part single, required confirmation, by shewing their agreement with the experiments of several of the most eminent chemists, made since that publication, that is, since the year 1791, with a few made nearly at the same time. In his former paper he compared his results with those of Bergman and Wenzel, they being almost the only persons who had made this subject the principal object of their enquiry, and had pursued it to a considerable extent. In each particular instance he traced the reason of the difference of their results from his own, when it was such as to deserve notice: he does not, therefore, repeat what was there observed; but cannot avoid again men-

buxus (box), and denotes the little box in which the letter was contained. According to this interpretation, the above may be thus translated. "As soon as I saw the bearer of your letters, I seized the small box which contained them, and frequently read them over with avidity;" and not that I snatched up my spectacles. Note of citizen Millin.

* Abridged from an extensive paper, which will hereafter appear in the Transactions of the Royal Irish Academy. I was favoured, by the author, with this paper, entitled, *Additional Observations*, &c.

tioning

tioning one general source of error attending the mode of investigation adopted by both, and yet noticed by neither, namely, the loss that many neutral salts undergo during evaporation; a loss, whose discovery is of considerable importance, not only to the present enquiry, but also to the conduct of several manufactures, particularly to that of saltpetre, and hence noticed by Mr. Lavoisier, 15 An. Chem. 254.

Other remarks and observations respecting the experiments of Bergman, Wenzel, and other chemists, form a very essential and valuable part of the paper; but the indispensable attention to brevity, which the nature and object of a periodical work demands, will oblige me to confine this abstract to the immediate structure and use of the tables.

When alkalies or earths, combined with fixed air, are dissolved in acids, though far the greater part of the fixed air is expelled during the solution, yet some portion of it is often retained, and may, in some degree, alter the sp. grav. of the solution; this circumstance was not recollected by Mr. Kirwan till lately; it was first noticed by Mr. Cavendish, Phil. Trans. 1766, p. 172, and afterwards by Bergman, in his notes on Scheffer, § 51, but more explicitly by Scheele, Chy. An. 1786, p. 13, and by Butini on Magnesia, p. 149. As to the use resulting from researches of this nature, he remarks that it would be superfluous to attempt to prove it at this day; as the recourse which the most eminent analysts have been obliged to have to it in particular instances, sufficiently evinces it. "Inquiries of this kind (says Mr. Fourcroy) are more difficult and delicate than those which have hitherto been made on salts; whatever requires a precise knowledge of quantities and proportions, presents difficulties so great as often to appear insurmountable, yet without this knowledge no progress can now be made in chemistry," 10 An. Chy. 325; and according to Bergman, "Usus cognitiæ proportionis principiorum ingredientium egregius est et multifarius." 1 Bergm. 137. chap. 1. § 1.

The Experiments made for ascertaining the Changes of Density in the Acids by Change of Temperature, were as follow :

Vitriolic acid 1,8360 at temperature 60°, becomes 1,8292 at 70°;....1,8317 at 65°;....1,8382 at 55°;....1,8403 at 50°;....and 1,8403 at 49°.—Hence we see that vitriolic acid, whose density at 60°, is 1,8360, loses by *ascending* and gains by *descending* 0,00068 for every degree of temperature between 60° and 70°, and 0,00043 nearly by each degree between 60° and 49°.

Again, vitriolic acid 1,7005 at 60°, becomes 1,6969 at 70°;....1,6983 at 65°;....1,7037 at 55°; and 1,7062 at 50°.—Hence vitriolic acid, which at 60° is 1,7005, gains or loses 0,00036 nearly for every degree between 60° and 70°, and 0,00051 by every degree between 60° and 50°.

Lastly, vitriolic acid 1,3888 at 60°, becomes 1,3845 at 70°;....1,3866 at 65°;....1,3898, at 55°; and 1,3926 at 49°.—Hence vitriolic acid, which at 60° is 1,3888, gains or loses

0,00043,

0,00043 nearly by every degree between 60° and 70° , and 0,00034 nearly by every degree between 49° and 60° ; between 49° and 50° no difference was perceived.

The Alterations of Density from Difference of Temperature in Nitrous Acid, were as follow:

Nitrous acid, which was 1,4279 at 60° , became 1,4178 at 70° ;...1,4225 at 65° ;...1,4304 at 55° ;...1,4336 at 50° ; and 1,4357 at 45° .—Hence nitrous acid, which at 60° is 1,4279, gains or loses 0,00101 nearly by every degree between 60° and 70° ; and 0,00052 by every degree between 45° and 60° .

It was formerly found by Mr. Kirwan, that the strongest *spirit of nitre* is most expanded by heat, or contracted by cold. Also, that nitrous acid, whose sp. grav. at 34° was 1,4750, was expanded by heat, as follows: viz. 1,4750 at 34° became 1,4653 at 49° ; whence it gains or loses 0,0097 by 15° , between 34° and 49° inclusively.

Again, that colourless nitrous acid, whose sp. grav. was 1,4650 at 30° , became 1,4587 at 46° , and 1,4302 at 86° .—Whence, by the first 16° , from 30° to 46° , it gained 0,0063, and by 40° , that is, from 46 to 86° , it gained 0,0285.

Again, nitrous acid, whose density was 1,2363 at 60° , became 1,2320 at 70° ;...1,2342 at 65° ;...1,2384 at 55° ;...1,2406 at 50° ; and 1,2417 at 45° .—Whence nitrous acid, which at 60° is 1,2363, gains or loses by every degree between 60° and 70° 0,00043, and 0,00036 by every degree between 60° and 45° ; and we may assume 0,0005 as the variation incident to every degree between 60° and 70° in nitrous acid, whose density at 60° is between 1,3 and 1,4, and 0,0004 for the variation between 44° and 60° .

Of Marine Acid.—It was formerly found that this acid of the density 1,196 at 33° became of the density 1,1820 at 66° ; the alterations of acids of lower sp. grav. were not examined; but it was found that in general its dilatibility is greater than that of nitrous acid of the same density.

TABLE of the Quantity of Real Acid, in 100 Parts of Vitriolic, Nitrous, and Marine Acid Liquors of different Densities, at the Temperature of 60°.

In Vitriolic Acid of different Densities, at the Temperature of 60°.				In Nitrous Acid of different Densities, at the Temperature of 60°.				In Marine Acid of diff. Densities at the Temp. of 60°.	
100 Parts Sp. Gravity.	Real Acid.	100 Parts Sp. Gravity.	Real Acid.	100 Parts Sp. Gravity.	Real Acid.	100 Parts Sp. Gravity.	Real Acid.	100 Parts Sp. Gravity.	Real Acid.
2,0000	89,29	1,4666	44,64	1,5543	73,54	1,3364	41,91	1,196	25,28
1,9859	88,39	1,4427	43,75	1,5295	69,86	1,3315	41,18	1,191	24,76
1,9719	87,50	1,4189	42,86 +	1,5183	69,12	1,3264	40,44	1,187	24,25
1,9579	86,61	1,4099	41,96	1,5070	68,39	1,3212	39,71	1,183	23,73
1,9439	85,71	1,4010	41,07	1,4957	67,65	1,3160	38,97	1,179	23,22
1,9299	84,82	1,3875	40,18	1,4844	66,92	1,3108	38,34	1,175	22,70
1,9168	83,93	1,3768*	39,28	1,4731	66,18	1,3056	37,50	1,171	22,18
1,9041	83,04 +	1,3663	38,39	1,4719	65,45	1,3004	36,77	1,167	21,67
1,8914	82,14	1,3586	37,50	1,4707	64,71	1,2911	36,03	1,163	21,15
1,8787	81,25	1,3473	36,60	1,4695	63,98 +	1,2812	35,30 +	1,159	20,64
1,8660	80,36	1,3360	35,71	1,4683	63,24	1,2795	34,56	1,155	20,12
1,8543	79,46	1,3254	34,82	1,4671	62,51	1,2779	33,82	1,151	19,60
1,8424	78,57	1,3149	33,93	1,4640	61,77	1,2687	33,09	1,147	19,09
1,8306	77,68	1,3102	33,03	1,4611	61,03	1,2586	32,35	1,1414	18,57
1,8188	76,79 +	1,3056	32,14	1,4582	60,30	1,2500	31,62	1,1396	18,06
1,8070	75,89	1,2951	31,25	1,4553	59,56	1,2464	30,88	1,1358	17,54
1,7959	75,—	1,2847	30,35	1,4524	58,83	1,2419	30,15	1,1320	17,02
1,7849	74,11	1,2757	29,46	1,4471	58,09	1,2374	29,41	1,1282	16,51
1,7738	73,22	1,2668	28,57	1,4422	57,36	1,2291	28,68	1,1244	15,99
1,7629	72,32	1,2589	27,68 +	1,4373	56,62	1,2209	27,94	1,1206	15,48
1,7519	71,43	1,2510	26,78	1,4324	55,89	1,2180	27,21 +	1,1168	14,96
1,7416	70,54 +	1,2415	25,89	1,4275	55,15	1,2152	26,47	1,1120	14,44
1,7312	69,64	1,2320	25,—	1,4222	54,12 +	1,2033	25,74 +	1,1078	13,93
1,7208	68,75	1,2210	24,10	1,4171	53,68	1,2015	25,00	1,1036	13,41
1,7104	67,86	1,2101	23,21	1,4120	52,94	1,1963	24,26	1,0984	12,90
1,7000	66,96	1,2009	22,32	1,4069	52,21	1,1911	23,53	1,0942	12,38
1,6899	66,07	1,1918	21,43 +	1,4018	51,47	1,1845	22,79	1,0910	11,86
1,6800	65,18	1,1836	20,53	1,3975	50,74	1,1779	22,06	1,0868	11,35
1,6701	64,28	1,1746	19,64	1,3925	50,00	1,1704	21,32	1,0826	10,83
1,6602	63,39	1,1678	18,75	1,3875	49,27	1,1639	20,59	1,0784	10,32
1,6503	62,50	1,1614	17,85	1,3825	48,53	1,1581	19,85	1,0742	9,80
1,6407	61,61	1,1531	16,96	1,3775	47,80	1,1524	19,12	1,0630	8,25
1,6312	60,71	1,1398	16,07	1,3721	47,06	1,1421	18,48	1,0345	5,16
1,6217	59,82	1,1309	15,18 +	1,3671	46,33	1,1319	17,65 +	1,0169	2,58
1,6122	58,93	1,1208	14,28	1,3621	45,59	1,1284	16,91		
1,6027	58,03	1,1129	13,39	1,3571	44,86 +	1,1241	16,17		
1,5932	57,14	1,1011	12,50	1,3521	44,12	1,1165	15,44		
1,5840	56,25	1,0955	11,60	1,3468	43,38	1,1111	14,70		
1,5748	55,36 +	1,0896	10,71	1,3417	42,65	1,1040	13,27		
1,5656	54,46	1,0833	9,80	The Numbers above the Lines drawn across the Tables of vitriolic and ni- trous Acids were found by Experi- ments; those under the Lines only by Analogy.					
1,5564	53,57	1,0780	8,95 +						
1,5473	52,68	1,0725	8,03	The Affinity of vitriolic Acid to Water decreases in the Ratio of the Square of the Quantity of Water united to it. 23 Ann. Chy. 196 and 197. And to Mr. K. thinks it does to all other Substances; it is the mean Affi- nity that is commonly given.					
1,5385	51,78	1,0666	7,14						
1,5292	50,89	1,0610	6,25						
1,5202	50,00	1,0555	5,35						
1,5112	49,11 +	1,0492	4,46						
1,5022	48,21	1,0450	3,57						
1,4933	47,32	1,0396	2,67						
1,4844	46,43	1,0343	1,78						
1,4755	45,53								

* The Sp. Gravity was 1,3741 in the former Table.

Note. The standard Quantities of Vitriolic Acid were reduced to Real by multiplying them into 0,8929; of the Nitrous, by multiplying them into 0,7354; and the Marine by multiplying them into 0,516, for the Reasons mentioned in the Author's last Paper.

TABLE II.—Quantity of Real Acid taken up by mere Alkalies and Earths.

100 Parts.	Vitriolic.	Nitrous.	Marine.	Fixed air.
Tartarin - - - -	82.48	84.96	56.3	105 almost
Soda - - - -	127.68	135.71	73.41	66.8
Vol-alkali - - - -	383.8	247.82	171	Variable
Barytes - - - -	50	56	31.8	282
Strontian - - - -	72.41	85.56	46	43.2
Lime - - - -	143	179.5	84.488	81.81
Magnesia - - - -	172.64	210	111.35	200 Fourcroy
Argill - - - -	150.9			335 nearly, Bergmann

TABLE III.—Of the Quantity of Alkalies and Earths taken up by 100 Parts of Real Vitriolic, Nitrous, Muriatic, and Carbonic Acids, saturated.

100 Parts.	Tartarin.	Soda.	Vol-alkali.	Barytes.	Strontian.	Lime.	Magnesia.
Vitriolic - - -	121.48	78.32	26.05	200	138.	70.	57.92
Nitrous - - -	117.7	73.43	40.35	178.12	116.86	55.7	47.64
Muriatic - - -	177.6	136.2	58.48	314.46	216.21	118.3	89.8
Carbonic - - -	95.1	149.6	—	354.5	231.4	122.	50.

TABLE IV.—Quantity of Neutral Salts afforded by 100 Parts of the abovenamed Acids when saturated with the abovenamed Bases.

100 Parts	Tartarin.	Soda.	Vol-Alkali.	Barytes.	Strontian.	Lime.	Magnesia.
Vitriolic	221.48	{ 425 chrytall ^d 178.5 deficc ^d }	182.94	300	238	{ 170 in a white heat 198 at 170° 174 well dried, that is, in air 238 in a red heat }	{ 340 chrytallized 158 deficcated }
Nitrous	227.22	188	175.44				
Marine	277.6	257.2	233.9	{ 487.4 chrytall ^d 454.5 deficc ^d }	{ 540 chrytall ^d 313.5 deficc ^d }		286.2 well dried
Carbonic	232.5	{ 69.35 chrytall ^d 150 deficc ^d }		454.5	331.7	222.25	200

TABLE V.—Quantity of Neutral Salt afforded by 100 Parts of different Bases when combined with the Vitriolic, Nitrous, Marine, or Carbonic Acid.

100 Parts.	Vitriolic.	Nitrous.	Marine.	Carbonic Acids.
Tartarin	182.48	193 +	156.3	244
Soda	{ 541.1 chrytallized 22.74 deficcated }	246.42	188.74	{ 463.3 chrytallized. 167 deficcated. }
Vol-alkali	702.94	435	400	
Barytes	150		{ 155.16 chrytallized 142.8 deficcated }	382
Strontian	172.41		{ 250 chrytallized 145 deficcated }	143.16
Lime	{ 312 dried at 50° 284 dried at 130° 262.8 ignited 243 incandescant }	{ 312 dried at 80° 280 fully deficcated }	200 in a red heat	182
Magnesia	{ 588.23 chrytallized 272.62 deficcated }		321.8 gently but sensibly dried	400

TABLE VI.—Of the Proportion of Ingredients in the following Saline Compounds.

100 Parts Carbonic.	Basis.	Acid.	Water.	State.	100 Parts Nitrates.	Basis.	Acid.	Water.	State.
Aerated Tartarin	41, - - -	43,	16, - - -	Crytallized	Nitre	51,8	44,	42 of Compou.	Dried at 70°
Common Salt of Tartarin or Pearl Ash	60, - - -	30,	6 - - -	Dry	Nitrated Soda	40,58	53,21	6,21 of Compou.	Dried at 400°
Aerated Soda	21,58	14,42	64,	Fully Crytallized	Do.	42,34	57,55	-	Ignited
Do.	59,86	40,05	-	Dedicated	Nitrated Vol-alkali	23,	57,	20,	-
Aerated Barytes	78, - - -	22,	- - -	Natural or ignited	Nitrated Barytes	57,	32,	11,	Crytallized
Aerated Strontian	69,5 - - -	30,	- - -	Natural or ignited	Nitrated Strontian	36,21	13,07	32,72	Crytallized
Aerated Lime	55, - - -	45,	- - -	Natural, if pure, or artificial ignited	Nitrated Lime	32,	57,44	10,56	Well dried, that is in Air
Aerated Magnesia	25, - - -	50,	25,	Crytallized	Nitrated Magnesia	22,	46,	-	Crytallized
Common Magnesia	45, - - -	34,	21,	Dried at 80°				22,	-
Aerated Vol-alkali	In the Ratio of 6 to 13 fixed Air	to	13 fixed Air						
<i>Viridic.</i>									
Viridolated Tartarin	54,8	45,2	- - -	Dry	Muriated Tartarin	64,	36,	-	Dried at 80°
Glauber	18,48	23,52	53,	Fully crytallized	Common Salt	53,	47,	aqueous, 38,88 re.1	Dried at 80°
Do.	44,	56,	- - -	Dedicated at 700°	Sal Ammoniac	-	-	-	Crytallized
Viridolated Vol-alkali	14,24	54,66	31,1	- - -	Do.	25,	42,75	32,25	Sublimed
Barofelenite	66,66	33,33	- - -	Natural and pure, artificial ignited	Muriated Barytes	64,	20,	16,	Crytallized
Viridolated Strontian	58,	42,	- - -	Natural and pure, artificial ignited	Do.	76,2	23,8	-	Dedicated
Selenite	32,	46,	22,	Died at 66°	Muriated Strontian	40,	18,	-	Crytallized
Do.	35,23	50,39	14,38	Dried at 170°	Do.	69,	31,	-	Dedicated
Do.	38,81	55,84	53,5	Ignited	Muriated Lime	50,	42,	-	Red hot
Do.	41,	59,	- - -	Incedefcent	Muriated Magnesia	31,07	34,59	-	Sensibly div
Epfom	17,	29, 25	53,65	Fully crytallized	Muriated Silver	75,	16,54	8,46 Oxygen	Dried at 130°
Do.	36,68	63,32	- - -	Dedicated	Muriated Lead	81,77 Calx	18,23	In the Calx	Crytallized
Alum	12, ignited	17,66	51, of Crytal + 19,24 in the Earth	Crytallized	Do.	83, Calx	17,	-	Dedicated
Do.	63,75	36,25	- - -	Dedicated at 700°					
<i>Viridic.</i>									
Of Iron	28, Calx = 12, tal	26,	38, + 8 of Composition	Crytallized					
Do.	45,	41,93	13,07	Calined to Red nels					
Lead	75, Calx = 71, Metal	23,37	1,63						
Copper	40, Calx = 30, Metal	31,	29,						
Zinc	40, Calx = 30, Metal	20,5	39,						

(The use and application of these Tables, with other extracts from this valuable papers, to be given in our next.)

VIII.

On a submarine Forest on the east Coast of England *. By JOSEPH CORREA DE SERRA, LL.D. F.R.S. and A.S.

IN geology, more perhaps than in any other branch of natural history, there exists a necessity of strictly separating the facts observed from the ideas, which, in order to explain them, may occur to the mind of the observer. In the present state of this science, every well ascertained fact increases our still narrow stock of real knowledge; when, on the contrary, the reasonings we are enabled to make are, at best, but ingenious guesses, which too often bias and mislead the judgment. I shall, therefore, endeavour in this paper, to give, first, a mere description of the object unmixed with any systematical ideas, and shall afterwards offer such conjectures on its cause as seem to me to be fairly grounded on observation.

It was a common report in Lincolnshire, that a large extent of islets of moor, situated along its coast, and visible only in the lowest ebbs of the year, was chiefly composed of decayed trees. These islets are marked in Mitchell's chart of that coast, by the name of *clay butts*; and the village of Huttoft, opposite to which they principally lie, seems to have derived its name from them. In the month of September, 1796, I went to Sutton, on the coast of Lincolnshire, in the company of the right honourable president of this society, in order to examine their extent and nature. The 19th of the month, being the first day after the equinoctial full moon, when the lowest ebbs were to be expected, we went in a boat at half-past 12 at noon, and soon after set foot upon one of the largest islets then appearing. Its exposed surface was about thirty yards long, and twenty-five wide, when the tide was at the lowest. A great number of similar islets were visible round us, chiefly to the eastward and southward; and the fishermen, whose authority on this point is very competent, say that similar moors are to be found along the whole coast, from Skegness to Grimsby, particularly off Addelthorpe and Mablethorpe. The channels dividing the islets were, at the time we saw them, wide and of various depths; the islets themselves ranging generally from east to west in their largest dimension.

We visited them again in the ebbs of the 20th and 21st; and though it generally did not ebb so far as we expected, we could, notwithstanding, ascertain that they consisted almost entirely of roots, trunks, branches, and leaves of trees, and shrubs, intermixed with some leaves of aquatic plants. The remains of some of these trees were still standing on their roots; while the trunks of the greater part lay scattered on the ground, in every possible direction. The bark of the trees and roots appeared generally as fresh as when they were growing, in that of the branches particularly, of which a great quantity was found; even the thin silvery membranes of the outer skin were discernible. The timber of all kinds, on the contrary, was decomposed and soft in the greatest part of the trees; in some, however,

* *Philos. Transactions*, 1799.

it was firm, especially in the roots. The people of the country have often found among them very sound pieces of timber, fit to be employed for several economical purposes.

The sorts of wood which are still distinguishable, are birch, fir, and oak. Other woods evidently exist in these islets, of some of which we found the leaves in the soil; but our present knowledge of the comparative anatomy of timbers is not so far advanced as to afford us the means of pronouncing with confidence respecting their species. In general, the trunks, branches, and roots, of the decayed trees, were considerably flattened; which is a phenomenon observed in the *furtastrand*, or fossil wood of Iceland, and which Scheuchzer remarked also in the fossil wood found in the neighbourhood of the lake of Thun, in Switzerland.

The soil to which the trees are affixed, and in which they grew, is a soft greasy clay; but for many inches above its surface, the soil is entirely composed of rotten leaves, scarcely distinguishable to the eye; many of which may be separated, by putting the soil in water, and dexterously and patiently using a spatula, or blunt knife. By this method I obtained some perfect leaves of *illex aquifolium*, which are now in the herbarium of the right honourable sir Joseph Banks, and some other leaves, which, though less perfect, seem to belong to some species of willow. In this stratum of rotten leaves, we could also distinguish several roots of *arundo phragmites*.

These islets, according to the most accurate information, extend at least twelve miles in length, and about a mile in breadth, opposite to Sutton shore. The water without them, towards the sea, generally deepens suddenly, so as to form a steep bank. The channels between the several islets, when the islets are dry in the lowest ebbs of the year, are from four to twelve feet deep; their bottoms are clay, or sand, and their direction is generally from east to west.

A well, dug at Sutton by Joshua Searby, shows that a moor of the same nature is found under ground in that part of the country, at the depth of sixteen feet; consequently, very nearly on the same level with that which constitutes the islets. The disposition of the strata was found to be as follows:

Clay,	-	-	-	-	-	-	-	16 feet
Moor, similar to that of the islets, from	-	-	-	-	-	-	-	3 to 4 do.
Soft moor, like the scowerings of a ditch bottom, mixed with shells and silt,	-	-	-	-	-	-	-	20 do.
Marly clay,	-	-	-	-	-	-	-	1 do.
Chalky rock, from	-	-	-	-	-	-	-	1 to 2 do.
Clay,	-	-	-	-	-	-	-	31 yards.

Gravel and water. The water has a chalybeate taste.

In order to ascertain the course of this subterraneous stratum of decayed vegetables, sir J. Banks directed a boring to be made in the fields belonging to the Royal Society in the parish of Mablethorpe. Moor, of a similar nature to that of Searby's well, and of the islets, was found very nearly on the same level, about four feet thick, and under it a soft clay.

The

The whole appearance of the rotten vegetables we observed, perfectly resembles, according to the remark of sir J. Banks, the moor which in Blankeney fen, and in other parts of the east fen, in Lincolnshire, is thrown up in the making of banks; barks, like those of the birch tree, being there also abundantly found. This moor extends over all the Lincolnshire fens, and has been traced as far as Peterborough, more than sixty miles to the south of Sutton. On the north side, the moory islets, according to the fishermen, extend as far as Grimsby, situated on the south side of the mouth of the Humber; and it is a remarkable circumstance, that, in the large tracts of low lands which lie on the south banks of that river, a little above its mouth, there is a subterraneous stratum of decayed trees and shrubs, exactly like those we observed at Sutton; particularly at Axholme isle, a tract of ten miles in length by five in breadth; and at Hatfield Chase, which comprehends one hundred and eighty thousand acres. Dugdale * had long ago made this observation in the first of these places; and de la Pryme †, in the second. The roots are there, likewise, standing in the places where they grew; the trunks lie prostrate. The woods are of the same species as at Sutton. Roots of aquatic plants, and reeds, are likewise mixed with them; and they are covered by a stratum of some yards of soil, the thickness of which, though not ascertained with exactness by the above-mentioned observers, we may easily conceive to correspond with that which covers the stratum of decayed wood at Sutton, by the circumstance of the roots being (according to Mr. Richardson's observations ‡) only visible when the water is low, where a channel was cut, and has left them uncovered.

Little doubt can be entertained of the moory islets of Sutton being a part of this extensive and subterraneous stratum, which, by some inroad of the sea, has been there stripped of its covering of soil. The identity of the levels; that of the species of trees; the roots of these affixed in both to the soil where they grew; and, above all, the flattened shape of the trunks, branches, and roots found in the islets (which can only be accounted for by the heavy pressure of a superinduced stratum), are sufficient reasons for this opinion.

Such a wide-spread assemblage of vegetable ruins, lying almost in the same level, and that level generally under the common mark of low water, must naturally strike the observer, and give birth to the following questions:

1. What is the epoch of this destruction?
2. By what agency was it effected?

In answer to these questions, I will venture to submit the following reflections.

The fossil remains of vegetables, hitherto dug up in so many parts of the globe, are, on a close inspection, found to belong to two different states of our planet. The parts of vegetables and their impressions, found in mountains of a cotaceous schistous, or even sometimes of a calcareous nature, are chiefly of plants, now existing between the tropics, which could neither have grown in the latitudes in which they are dug up, nor have been carried and depo-

* History of Embanking and Draining. chap. xxvii.

† Philos. Transf. vol. XXII. p. 980.

‡ Philos. Transf. vol. XIX. p. 528.

sited there by any of the acting forces under the present constitution of nature. The formation, indeed, of the very mountains in which they are buried, and the nature and disposition of the materials which compose them, are such as we cannot account for by any of the actions and re-actions which in the actual state of things take place on the surface of the earth. We must necessarily recur to that period in the history of our planet, when the surface of the ocean was at least so much above its present level, as to cover even the summits of these secondary mountains, which contain the remains of tropical plants. The changes which these vegetables have suffered in their substance, is almost total. They commonly retain only the external configuration of what they originally were. Such is the state in which they have been found in England by Llwyd; in France by Jussieu; in the Netherlands by Burtin; not to mention instances in more distant countries. Some of the impressions, or remains of plants, found in soils of this nature, which were, by more ancient and less enlightened oryctologists, supposed to belong to plants actually growing in temperate and cold climates, seem, on accurate investigation, to have been parts of exotic vegetables. In fact, whether we suppose them to have grown near the spot where they are found, or to have been carried hither from different parts by the force of an impelling flood, it is equally difficult to conceive how organized beings, which, in order to live, require such a vast difference in temperature and in seasons, could live on the same spot, or how their remains could (from climates so widely distant) be brought together to the same place by one common dislocating cause. To this ancient order of fossil vegetables, belong whatever retains a vegetable shape, found in or near coal mines; and (to judge from the places where they have been found) the greater part of the agatized woods. But from the species and present state of the trees, which are the subject of this memoir, and from the situation and nature of the soil in which they are found, it seems very clear that they do not belong to the primeval order of vegetable ruins.

The second order of fossil vegetables comprehends those which are found in the strata of clay, or sand; materials which are the result of slow depositions of the sea or of rivers; agents still at work under the present constitution of our planet. These vegetable remains are found in such flat countries, as may be considered to be of a new formation. Their vegetable organization still subsists, at least in part; and their vegetable substance has suffered a change only in colour, smell, or consistence; alterations which are produced by the development of their oily and bituminous parts, or by their natural progress towards rottenness. Such are the fossil vegetables found in Cornwall by Borlase; in Essex by Derham; in Yorkshire by de la Pryme and Richardson; and in foreign countries by other naturalists. These vegetables are found at different depths, some of them much below the present level of the sea, but in clayey or sandy strata (evidently belonging to modern formation), and have no doubt been carried from their original place, and deposited there by the force of great rivers or currents, as it has been observed with respect to the Mississippi *. In many instances,

* La Condreniere sur les Depôts du Mississippi. Journ. de Phys. vol. XXI. p. 230.

however,

however, these trees and shrubs are found standing on their roots, generally in low or marshy places, above, or very little below, the actual level of the sea.

To this last description of fossil vegetables, the decayed trees here described certainly belong. They have not been transported by currents or rivers; but, though standing in their native soil, we cannot suppose the level, in which they are found, to be the same as that in which they grew. It would be impossible for any of these trees or shrubs to vegetate so near the sea, and below the common level of its water; the waves would cover such tracts of land, and hinder any vegetation. We cannot conceive that the surface of the ocean has ever been lower than it now is; on the contrary, we are led, by numberless phenomena, to believe that the level of the waters in our globe is much below what it was in former periods: we must, therefore, conclude, that the forest here described grew in a level high enough to permit its vegetation; and that the force (whatever it was) which destroyed it, lowered the level of the ground where it stood.

There is a force of subsidence (particularly in soft ground), which, being a natural consequence of gravity, slowly though perpetually operating, has its action sometimes quickened and rendered sudden, by extraneous causes; for instance, by earthquakes. The slow effects of this force of subsidence have been accurately remarked in many places; examples also of its sudden action are recorded in almost every history of great earthquakes. The shores of Alexandria, according to Dolomieu's observations, are a foot lower than they were in the time of the Ptolemies. Donati, in his natural history of the Adriatic, has remarked, seemingly with great accuracy, the effects of this subsidence at Venice; at Pola, in Istria; at Lissa, Bua, Zara, and Diclo, on the coast of Dalmatia. In England, Borlase has given, in the *Philosophical Transactions* *, a curious observation of a subsidence, of at least sixteen feet, in the ground between Sampson and Trescow islands, in Scilly. The soft and low ground between the towns of Thorne and Gowle, in Yorkshire, a space of many miles, has so much subsided in latter times, that some old men of Thorne affirmed, "That whereas they could before see little of the steeple (of Gowle), they now see the church-yard wall †." The instances of similar subsidence, which might be mentioned, are innumerable.

This force of subsidence, suddenly acting by means of some earthquake, seems to me the most probable cause to which the actual submarine situation of the forest we are speaking of, may be ascribed. It affords a simple, easy explanation of the matter, its probability is supported by numberless instances of similar events, and it is not liable to the strong objections which exist against the hypothesis of the alternate depression and elevation of the level of the ocean; an opinion which, to be credible, requires the support of a great number of proofs; less equivocal than those which have hitherto been urged in its favour, even by the genius of a Lavoisier ‡.

* Vol XLVIII. p. 62.

† Gough's edition of Camden's *Britannica*. t. III. p. 35.

‡ *Mem. de l'Acad. de Paris*, 1789, p. 351.

The stratum of soil, sixteen feet thick, placed above the decayed trees, seems to remove the epoch of their sinking and destruction far beyond the reach of any historical knowledge. In Cæsar's time, the level of the North Sea appears to have been the same as in our days. He mentions the separation of the Wahal branch of the Rhine, and its junction to the Meuse; noticing the then existing distance from that junction to the sea, which agrees according to D'Anville's inquiries * with the actual distance. Some of the Roman roads, constructed, according to the order of Augustus, under Agrippa's administration, leading to the maritime towns of Belgium, still exist and reach the present shore †. The descriptions which Roman authors have left us of the coasts, ports, and mouths of rivers, on both sides of the North Sea, agree in general with their present state; except in the places ravaged by the inroads of this sea, more apt from its form to destroy the surrounding countries, than to increase them.

An exact resemblance exists between maritime Flanders, and the opposite low coast of England, both in point of elevation above the sea, and of internal structure and arrangement of their soils. On both sides strata of clay, silt, and sand (often mixed with decayed vegetables), are found near the surface; and, in both, these superior materials cover a very deep stratum of bluish or dark coloured clay, unmixed with extraneous bodies. On both sides they are the lowermost part of the soil, existing between the two ridges of high lands ‡ on their respective sides of the same narrow sea. These two countries are certainly coeval, and whatever proves that maritime Flanders has been for many ages out of the sea, must, in my opinion, prove also that the forest we are speaking of was long before that time destroyed and buried under a stratum of soil. Now it seems proved from historical records, carefully collected by several learned members of the Brussels academy, that no material change has happened to the lowermost part of maritime Flanders, during the period of the last two thousand years §.

I am, therefore, inclined to suppose the original catastrophe, which buried this forest, to be of a very ancient date; but I suspect the inroad of the sea, which uncovered the decayed trees of the islets of Sutton, to be comparatively recent. The state of the leaves and of the timber, and also the tradition of the neighbouring people, concur to strengthen this suspicion. Leaves, and other delicate parts of plants, though they may be long preserved in a subterraneous situation, cannot remain uninjured, when exposed to the action of the waves and of the air. The people of the country believe that their parish-church once stood on the spot where the islets now are, and was submerged by the inroads of the sea; that at very low water their

* D'Anville Notice des Gaules, p. 461.

† Nicol. Bergier. Hist. des grands Chemins des Romains. Ed. de Bruxelles, vol. II. p. 109.

‡ These ridges of high land, both on the British and the Belgic side, must be very similar to each other, since they both contain parts of tropical plants in a fossil state. Cocoa nuts, and fruits of the arecas are found in the Belgic ridge. The petrified fruits of Sheppey, and other impressions of tropical plants on this side of the water, are well known.

§ Vide several papers in the Brussels *Memoirs*; also Journ. Phys. t. XXXIV. p. 401.

ancestors could even discover its ruins; that their present church was built to supply the place of that which the waves washed away; and that even their present clock belonged to their old church. So many concomitant, though weak testimonies, incline me to believe their report, and to suppose that some of the stormy inundations of the North Sea, which, in these last centuries, have washed away such large tracts of land on its shores, took away a soil resting on clay, and at last uncovered the trees which are the subject of this paper.

IX.

Optical Remarks, chiefly relating to the Reflexibility of the Rays of Light *. By P. PREVOST, Professor of Philosophy at Geneva, of the Academy of Berlin, of the Society des Curieux de la Nature, and of the Royal Society of Edinburgh.

PART I. Concerning reflexibility. *Section 1.* The word reflexibility is used in two different senses.

1. Newton † indicates, by this word, that property of a ray of homogeneous light, by virtue of which this ray is reflected, if it fall under a certain angle of incidence; and transmitted, if it fall under a certain smaller angle: or, more simply, a disposition to be reflected, and not transmitted at the limit which separates two refracting mediums ‡.

This philosopher thinks that the reflexibility of the rays of light, taken in this sense, is not the same in all the several kinds. He establishes, by experiments, which he concludes to be decisive, that the most refrangible rays are also the most reflexible. So that, according to him, all other circumstances being the same, if a white ray fall under a certain angle on the directing surface, the violet ray will be reflected, while the six others will be still transmitted and refracted. But by augmenting the angle of incidence, the successive reflection of all the rays will be obtained from the violet, which is the most reflexible, to the red, which is the least so.

Mr. Brougham § does not find the experiments of Newton, by which he establishes this proposition, conclusive: and, upon the foundation of another experiment, he establishes the contrary proposition; namely, that all the rays have the same disposition to be reflected, provided the angle of incidence be the same.

2. Mr. Brougham understands by reflexibility, a disposition in the ray to be reflected nearer to the perpendicular, to a certain degree; or, in other words, a property of the homogeneous ray, by which its angle of reflexion bears a certain ratio to its angle of incidence, which is not the ratio of equality, except in certain cases, which he points out.

According to this philosopher, the above ratio varies in each homogeneous ray. The ratio

* Translated from the French original, in the *Philos. Transf.* 1798.

† *Opt.* l. i. part i. prop. 3.

‡ *Opt.* l. i. part i. defin. 3.

§ *Philos. Transf.* 1796. p. 272, or *Philos. Journal.* i. 595.

of equality takes place in those rays, which occupy the confines of the blue and green : that of inequality takes place with regard to the others, and the most refrangible one the least reflexible. So that with regard to the red ray, the angle of refraction is less, and in the violet greater, than the angle of incidence.

It is known that Newton, on the contrary, affirms that the angle of reflexion is, in all cases, equal to the angle of incidence.

Let us examine these opposite opinions.

Section 2. The first question. Do the homogeneous rays of light differ in reflexibility in the Newtonian sense of the term ? Or, more directly ; does it happen, in fact, that the violet ray will be reflected under the same angle of incidence at which the red ray is transmitted, all other circumstances being precisely the same ?

Of the two experiments by which Newton establishes the unequal reflexibility of the rays, it will be sufficient to mention that which Mr. Brougham attacks directly.

Newton caused a white ray to fall perpendicularly upon the anterior surface of a prism ; after which, turning the prism on its axis, he observed the reflexion which took place, from its posterior surface. He saw that the violet ray was first reflected ; and afterwards, the other rays in the order of their refrangibilities, the red ray being reflected the last. Whence he concludes, that the violet ray is reflected under a less angle of incidence than the red. Experiment 9.

This is the conclusion which Mr. Brougham attacks ; and to avoid altering his thought, I shall here transcribe his expressions. “ That the demonstration involves a logical error, appears pretty evident. When the rays, by refraction through the base of the prism used in the experiment, are separated into their parts, these become divergent, the violet and red emerging at very different angles ; and these were also incident on the base at different angles, from the refraction of the side at which they entered : when, therefore, the prism is moved round on its axis, as described in the proposition, the base is nearest the violet, from the position of the rays by refraction, and meets it first : so that the violet being reflected as soon as it meets the base, it is reflected before any of the other rays, not from a different disposition to be so, but merely from its different refrangibility.”

So that Mr. Brougham thinks that the reflection of the violet ray does not precede that of the red ray ; but because the refraction which takes place at the anterior surface, forces the violet ray to arrive at the posterior surface sooner than the red ray.

But it seems that the effect is here the inverse of its cause. Let us first exclude a false sense of the words. It is impossible that the author can mean to say that the eye is capable of appreciating the interval of time, between the arrival of the violet and the red ray at the posterior face of the prism. However, that ray which describes the shortest course, falls nearest the perpendicular let fall from the point of departure ; and from this circumstance alone, we can conclude that its angle of incidence is smaller. Whence it follows, that the red ray ought to be reflected the first, and not the violet.

Let us, in fact, consider the position of the prism at the first moment, as it is represented

in the optics of Newton. The white ray FM , plate xvi. fig. 1, is perpendicular to AC ; and, in this case, it is not refracted, but describes a right line. At this point, Newton represents the violet ray MN as alone reflected; while all the others, such as MH , MI , are transmitted and refracted (at least the violet alone is totally reflected).

It is, nevertheless, certain, that in order to obtain this phenomenon, it was requisite to give the proper degree of inclination, by turning the prism, in order that the experiment might succeed: and Mr. Brougham has very properly observed, that the perpendicularity of the ray on the anterior face AC must then cease; consequently, there must have been refraction, and the several homogeneous rays could not have followed a right-lined course, such as FM , nor have fallen on the posterior face BC , under equal angles.

Let $A'B'C'$, fig. 2, be the new position of the prism, which it has taken by virtue of its rotation on its axis; the ray FP will then fall obliquely upon AC , at the point P , so that the perpendicular PO may be on the side A , and, consequently, comprized in the angle APF . The result will be, 1. What Newton proposed, namely, to increase the angle of incidence on the posterior surface; which angle (formed at the point M , in fig. 1, which represents the first position of the prism) was before too small to produce reflection; and, 2. The precise expressions of Newton affirm, that the prism was turned on its axis, according to the direction indicated by the order of the letters ABC , in his figure, which, as far as relates to my object, is the same as my figure 1.

The ray FP , fig. 2, will, therefore, be refracted, in approaching the perpendicular OP , but the most refrangible ray (the violet) will approach the nearest, and the least refrangible will be the farthest off. So that the courses of the rays respectively, will be well represented by the lines PV , PR . The violet ray will, therefore, make, with the posterior face BC , an angle PVC' , greater than the angle PRC' , formed by the red. Now the angles of incidence at the points V , R , are the complements of the angles PVC , PRC , respectively.

It is, therefore, certain, that by virtue of the refraction, which is produced at the anterior surface, the violet ray meets the posterior surface under a less angle of incidence than the red; and, consequently, the former is under less favourable circumstances for reflection than the latter; yet the former is reflected before the latter. We are, therefore, right in concluding, that it is in its own nature more reflexible, in the Newtonian sense of the word*.

This very proper consideration, which was introduced by Mr. Brougham, affords, therefore, a still stronger conclusion in favour of the Newtonian assertion. We may say, that the violet is not only reflected at the same incidence at which the red is transmitted; but, likewise, that this phenomenon takes place, though the angle of incidence of the violet be more unfavourable to reflection than that of the red.

* This reasoning is equally applicable to the tenth experiment of Newton, in which he uses two prisms joined together, to form one parallelopipedon. In both the experiments (9th and 10th), another accessory prism was used, to render the effect more sensible, by dispersing the reflecting rays. It was unnecessary to attend to this.

In a word, therefore, the rays differ in reflexibility in the Newtonian sense, and the most refrangible is also the most reflexible.

Thus far, in order to give simplicity to my argument, I have left the refracting angle C, of the prism, undetermined. Newton determines it in the ninth experiment of the first book of his optics, part 1. He used a refracting angle of 45° ; and, nevertheless, he says expressly, that the rays entered perpendicularly; whence it follows, that the angle of incidence at the point M was also 45° . We should, therefore, be justified in thinking, that rays falling under this angle of incidence on the surface B C, passing from glass to air, are not totally reflected: Briffon affirms this to be the case, when he explains this same experiment in his Elementary Treatise of Natural Philosophy, printed at Paris, in the year 1789. vol. II. § 1411. Nevertheless, it is well known that the total reflection takes place at a smaller angle, that is to say, about 40° . This has been determined, even with the utmost precision, for which I will quote only a single authority. "A ray of light will not pass out of glass into air, if the angle of incidence exceeds $40^\circ 11'$." Adam's Lectures on Natural and Experimental Philosophy, London, 1794. ii. 163. The determination of Newton does not sensibly differ from this: "Totalis reflexio tum incipit, cum angulus incidentiæ sit $40^\circ 10'$." Opt. II. p. 3. prop. 1. Can we affirm that under the angle of 45° , there still passes a sufficient number of rays to render the experiment preceptible? Or must we suppose that the ray F M, was not exactly perpendicular to the face A C? I think the latter is the truth; that is to say, that, in the first position of the prism, the ray began by being oblique to A C, in the opposite situation to that indicated in fig. 2. So that the angle A P F was less than F P C. Whence it would result, that the most refrangible rays would fall on the posterior-surface, B C, under a greater angle of incidence, and, therefore, more favourable to reflection. Under this form the argument of Mr. Brougham acquires new force.

The optics of Newton, in this place, requires a commentary. The best will be that which his *Lectiores Opticæ* present us—*If. Newtoni Opuscul. Lausannæ et Genève, 1744.* ii. 217. 223. Here the author replies to our doubts as follows: "Ne qua oriatur suspicio, quod refractiones in superficiebus, A C et A B, ad ingressum radiorum in prisma et egressum facta, possint aliquid conducere ad effectus hosce producendos, observare licet quod effectus iidem producuntur, cujuscunque magnitudinis statuatur angulus A C B *; hoc est, quæcunque sit refractionis superficie A C. Imo possis efficere quod cum colores partim reflectuntur et partim trajiciuntur radii perpendiculariter incidant in A C, emergantque ex A B, et sic neutra superficie refringantur, modo statuas angulum A C B, esse grad. 40 circiter, et iidem tamen effectus producentur †." p. 219. The most accurate
writers

* It is ABC, in the text; manifestly by an error of the press. This is, indeed, of no consequence to my object. I do not quote what relates to the equality required between the angles B and C, because it does not affect the present enquiry.

† *Anglice.* Left any suspicion should arise, that the refractions at the surfaces AC, and AB, at the entrance

writers have not overlooked this circumstance. Robert Smith, after having explained the 9th experiment of Newton's Optics, says, or makes Newton say, "I did not observe any refraction at the sides A C, A B, of the first prism, because the light was *nearly* perpendicular to the first sides, and emerged *nearly* perpendicular to the second; and, in consequence, underwent none, or so little, that the angles of incidence at the base B C were not sensibly altered; particularly if the angles of the prism at the base B C were each about 40° . For the rays F M begin to be totally reflected when the angle C M F is about 50° ; and, consequently, they will then form an angle of 90° with A C." *Smith's Optics translated into French, by L. P. P. (Pezenas)*. i. §. 173. p. 190*.

From all this it follows, that when the angle of a prism is well chosen, a white ray, falling perpendicularly on its anterior surface A C, may be decomposed; because it is partly, and not totally, reflected, the violet ray being reflected while the red is still transmitted.

I must here remark, that in order to repeat this experiment, and render it conclusive, it is not necessary to circumscribe the angle C, within the limits which render the ray F M perpendicular (or nearly so) to the anterior face A C. All the reasoning we have offered on this head (§ 2.) will be just, provided that, in the first position of the prism, the angle A P F be greater than its supplement F P C'. But in order that this circumstance should take place, when the reflection is made at the point M, it is sufficient that the refracting angle C' be less than 40° .

Interesting results may be obtained, by varying this experiment; but the authorities I have quoted, leave no doubt with regard to the particular result, which we have to examine. *The most refrangible rays are reflected at a less angle of incidence. They are more reflexible in the Newtonian sense.*

Section 3. But Mr. Brougham supports the contrary opinion, by an experiment which he thus announces: "I held a prism vertically, and let the spectrum of another prism be reflected by the base of the former, so that the rays had all the same angle of incidence; then turning round the vertical prism on its axis, when one sort of rays was transmitted or reflected, all were transmitted or reflected †."

As the complete discussion of this experiment would require some detail, I shall content myself with observing, that the plane of the vertical face cannot be adjusted, so as to receive

entrance and emergence of the rays, with regard to the prism, might, in some respect, assist in producing these effects, it may be observed, that the same effects are produced, whatever may be the magnitudes of the angle A C B; that is, whatever may be the refraction of the surface A C It may even be contrived, that, when the colours are partly reflected . . . and partly transmitted . . . the rays may fall perpendicularly on A C, and A B, at their entrance and emergence, and they be refracted by neither surface, provided the angle A C B be made of about 40° , and yet the same effects are produced. N.

* The passage here marked as quotation, is retranslated from the French; and, consequently, is not given in the very words of Smith, whose work I have not. N.

† The same experiment, tried by Newton, afforded precisely the contrary result. "Radii purpuriformes primo omnium reflectuntur et ultimo rubriformes." *Lect. Opt. Opuscul. ii. 220.*

all

all the rays of the spectrum at once under the same angle ; and even supposing this to have been possible, and done for an instant, yet the rotation of the prism would have changed this disposition, by altering this angle with regard to the several rays. We may, consequently, imagine a variety of different results; and, among others, we may conceive the angles of incidence of the several rays to be such, that the observation of Mr. Brougham may be reconciled with the opinion of Newton, on their unequal reflexibility. But since Mr. Brougham does not enter into this detail, and gives only a single result, it is to be presumed, that he has not repeated, or at least varied, this experiment. This philosopher even appears, by his rapid enunciation, to have considered it of no very great importance.

I think, therefore, that it cannot at present weaken the conclusions of Newton ; and that we are still justified in affirming, in the sense of that philosopher, that the most refrangible rays are also the most reflexible.

Section 4. SECOND QUESTION. Do the rays of light differ in reflexibility, in the Broughamian sense? In other words, does the red ray at the same angle of incidence form a less angle of reflection, and the violet a greater, than the angle of incidence?

Section 5. The fundamental experiment from which Mr. Brougham deduces this unequal reflexibility in the sense of his definition, is this :

A bright polished cylinder, of very small diameter (a metallic fibre), having its convex surface presented to a white ray, reflected a coloured spectrum ; and after proper admeasurement and computation, it appeared that the rays in the confine of the blue and green, were alone reflected, under an angle equal to that of the incidence. The red rays were reflected under a less angle, and the violet under a greater.

The question, therefore, is reduced to determine, whether this experiment be conclusive in favour of Mr. Brougham.

Section 6. To ascertain this, it is of importance to advert to a principle laid down by Newton, and admitted by Brougham, p. 250. (or *Philos. Journal*, i. 561), namely, that the force, whatever it may be, that produces reflection, acts in the direction of a line, perpendicular to the reflecting surface.

Section 7. From this principle it follows, that the reflection operated by a plane surface, must be made according to the law hitherto admitted by all opticians (Newton's *Princip.* l. i. p. 96). And this is true, whatever may be the intensity of the repulsive force, and the inclination of the incident ray ; provided the ray be really incident, and do not move parallel to the repulsive surface.

Section 8. This consequence, and the whole of the Newtonian demonstration, supposes that the surface acts on the ray during its whole course through the sphere of its activity ; and equally at equal distances. Mr. Brougham alleges nothing against this hypothesis, but even seems to admit it expressly (p. 269, or *Ph. Journal*, i. 593) ; and indeed how can it be denied?

Section 9. Therefore, from a principle which is not contested, it appears that reflection cannot decompose white light, when it is made totally from a plane surface.

This is perfectly conformable to the observation of Mr. Brougham, that there is no mean
of

of effecting this decomposition by employing plane surfaces, nor curve surfaces, which have not a very minute, and, as it were, evanescent radius. It may, in fact, be conceived, that a small portion of a curved surface of a greater radius is a plane with regard to a particle of light. The author indeed, explains this phenomenon in another manner; but the fact, independent of all explanation, is not the less certain and acknowledged.

Section 10. Let H H H, fig. 3, now represent a very small bright polished cylinder (a metallic fibre), and B R V K the cylinder on the same axis, which is the sphere of activity of this small body: each of them being represented by its circular section. (These two circles, though very unequal, are confounded together in actual observation.)

A B represents a white ray, incident at the point B, on the cylinder, on its sphere of activity.

Suppose the homogeneous rays to be unequally repulsive, and that the red be more strongly repelled than the violet: Mr. Brougham admits this supposition (p. 267, or *Philos. Journal*, i. 592).

On this hypothesis, the violet ray must penetrate farther into the sphere of repulsion.

The course described by an homogeneous ray, within the sphere of repulsive activity, must be formed of two equal and similar branches: and its axis must pass through the centre of the sphere or section. This follows from the principle before laid down (§ 6).

And it is an immediate consequence of this remark, that this homogeneous ray will issue from the sphere of activity, under an angle of reflection equal to the angle of incidence.

So that all the homogeneous rays forming, in B, the same angle of incidence, will be reflected under equal angles.

But since some penetrate farther into the sphere of activity than others, they will come out divergent; for this is the only condition by which the equality of the angles of reflection can be preserved.

Fig. 3 is intended to shew this effect. The red ray penetrating less into the sphere or cylinder of activity B R V K, describes the curve B O R, the axis of which passes through the centre C; and it emerges through R G, making the angle of reflection E R G = A B D, the angle of incidence. The violet ray, penetrating deeper, describes the curve B Q V, of which the axis likewise passes through C. This ray emerges through V L, and the three angles F V L, E R G, A B D, are equal.

But the observer seeing the arc B R V like a point, and knowing that the angles which he measures are the sum of the angle of incidence and of reflection for the rays of each kind, will be induced to think, that, under the same angle of incidence, the angles of reflection vary: for he will find that the right line A B, forms unequal angles with the right lines R G, V L; and, in a word, he will have all the same appearances which presented themselves to Mr. Brougham (§ 5).

It is of importance to remark here, that though this philosopher affirms that the rays on the confine of blue and green are reflected under an angle of reflection equal to the angle of incidence, he did not, nor could not, ascertain this equality by any direct experiment: it

was simply the most natural supposition he could make. And if it had been pretended, that all the angles of reflection are smaller, or all greater than those of incidence; or that the limit of equality falls on any other division of the spectrum, the supposition would have been gratuitous, but against which the observer could oppose no direct fact. For there is no conceivable means by which the angle of incidence can be measured in these experiments, directly and apart from that of reflection.

Section 11. Since, therefore, by supposing (with Mr. Brougham), that the red ray is more repulsive than the violet, we perfectly reconcile the phenomenon observed with the law of reflection acknowledged to prevail in plane surfaces, there is no reason to depart from it in the present case.

And I conclude, from all that has been said, *that the homogeneous rays are not unequally reflexible* in the Broughamian sense; or, in other terms, that the law of reflection, admitted by Newton, is the true law of nature.

(*To be concluded in our next.*)

X.

*On the Manufacture of Beech Oil, by AARON HILL, and other Projects of Improvement.
By a Correspondent.*

SIR,

WE have an old English proverb, which affirms, that it is much easier to ask questions than to answer them. I have lately met with a pamphlet, which has tempted me to propose a number to your correspondents; but have been somewhat intimidated by the above consideration, which might, perhaps, prevent your favouring me with their insertion in your excellent work. It has, however, occurred to me, that a kind of middle way may be devised, to render the business more fair and reasonable.—If the account I now send you should be thought to contain information enough to render it acceptable to your readers, it will be but fair to place it in the balance against the questions I may hope to have resolved.

The pamphlet I allude to, is *Aaron Hill's Account of the Rise and Progress of the Beech Oil Invention* (8vo. 112 pages), which bears date in the year 1715: and, as it appears, was given away. This ingenious man, equally known for his works of imagination, his projects, and the tried rectitude of his principles, passed the latter part of an active life, of sixty-five years, in a state of poverty. His project of making oil from the beech-nut was, therefore, either unsuccessful, or, what is more probable, it failed from circumstances dependant either on himself personally, or those who were engaged with him. The elucidation of this object, which is of no small importance to our woollen and other manufactures, cannot surely be foreign to the purposes of your Journal; and even if any of your correspondents should favour us with historical facts, tending to shew the inutility of the scheme, the public would, no doubt,

receive it with gratification on the mere inducement of curiosity. The pamphlet before me contains also notices of inventions of some curiosity and apparent value, though related almost as concisely as the century of inventions of the Marquis of Worcester: upon the whole, they shew that when a man of ability leaves the beaten path of the current affairs of life, to pursue the remote analogies of improvement, new results spring up around him; to the practical accomplishment of which the life of man is totally inadequate, and in the pursuit of only a few of which, the utmost endeavours of an unfortunate individual may be exerted without effect, until at length he sinks under the extent and variety of his plans and occupations.

To your discretion, sir, I submit an abridged account of this pamphlet. If from the promise of utility it should appear fit for the public eye, I shall receive pleasure from your decision; if the contrary, I shall feel no pain from the free operation of a power which I think you worthy to exercise.

The treatise consists of a general introduction on the nature and advantages of new inventions, and the prejudices they have to contend with: with an account of the particular invention which forms the chief subject of the address. The author complains of his sufferings from the misrepresentations of ignorance and malice, and after relating the ancient story of Thales, the Milefian, who enriched himself by buying up oil previous to a scarce year of olives, which he foresaw, he gives an account of the discovery of the stocking engine, as follows:

“It is not out of some mens’ remembrance, that a young gentleman of no fortune, a student in Oxford, fell in love with an innkeeper’s daughter of that town, whose circumstances were very narrow. He had philosophy enough to despise superfluous wealth, and judgment to foresee the necessity of a competence, but love was headstrong, and too hard for reason, so that after a year or two’s ineffectual delay, they bid defiance to their stars, and had courage enough to marry. The scholar gained a wife, and lost a fellowship: the only small subsistence he before depended on.”

The narrative proceeds to acquaint us, that the father-in-law, who during his life-time despised the unproductive acquisitions of his daughter’s husband, died miserably poor, and left this couple in a state of extreme indigence; their whole support being derived from the industry of the wife in knitting stockings.

In this situation, rendered more anxious by the certainty of an event which promised to increase their family, and to suspend that labour which afforded their subsistence, the husband could only waste his hours between his books and his sighs, with ineffectual meditations on schemes for relief. In these reflections, his eyes were often involuntarily fixed on the motion of his wife’s fingers at her work. This attention was at last more steadily directed, and his imagination set to work on the enquiry, whether the same operation could not be performed by machinery. Both parties directed their ingenuity to this research, and at length succeeded in making a stocking loom, to their own great relief and comfort, and lasting benefit to the public.—*Qu.* Who were the parties here spoken of?—The author then mentions other speculations, and the numerous inventions which distinguish cultivated nations; from whence he deduces the evident consequence, that improvements ought to be encouraged.

couraged. He refers to *Stow*, and other authors, for accounts of the calumnies and opposition which were made to the project of the New-River, and occasioned the ruin of the projector of that noble work. The burning of bricks with pit-coal, which was first done at the beginning of the seventeenth century; the improvement of glass, introduced by the Duke of Buckingham; the project of James the First, to introduce the growth of silk; the establishment of the woollen manufacture from Flanders; and the manufacture of rape oil, are among the instances he offers in favour of projects: and to these he adds three speculations of discoveries of his own, which are little more than hinted at. Speaking of the first, he affirms, that there is a common vegetable, almost the growth of every hedge, which yields a wax, finer than that used for candles, and at an expence so inconsiderable, that he is very sure, that in the practice of the thing, a pound of such wax candles would not cost three halfpence. I will not annex a formal query what this vegetable may be: but simply observe, that an enquiry into the combustibility and other properties of vegetable products, with regard to this great object of affording light, appears to deserve the attention of curious men.

Another discovery, which he says he has lately bestowed upon an honest gentleman, is that by the charge of a single penny, exclusive of the price of coals, he can produce a gallon of aqua-vitæ, much fitter for all common uses than the thrice-rectified spirit of malt. Query: What is the date of the general practice of distilling spirit from molasses? Much earlier, I suppose than this period. I am disposed to think, that the project of Aaron Hill may have consisted in the use of some saccharine product, which was cheap, but not plentiful; and, consequently, could not be procured in sufficient quantity, or suddenly rose in price as soon as a demand was felt.

Another object of information is, that acorns, dried and ground, are a much better and cheaper tanning material than oak-bark, and that it is used in Italy, and all over the Archipelago, by the name of *velania*. Is this true in point of fact?

The last project is a scheme of finance, which is related, or alluded to, in terms of the highest promise; but as there is no hint of the nature of the plan, and as political arithmetic is a science in which mistakes are not only possible, but very likely to be made, I shall pass it over without further remark.

The rest of the book contains an history of the discovery and practice of the art of making oil from beech-mast. The author first states, that the raw material is very plentiful, and that every bushel is found to yield two gallons of much better oil than that of olives, called Seville or Gallipoli oil. He informs us, that he made the discovery at Naples, in 1699; but being at that time only fifteen years of age, he thought no more of it till 1712, when he was induced to enquire into the demand for oil, by the soap makers, clothiers and others, and also from the dealers and the entrances at the Custom-house. He was surprised at the extent of the consumption, and immediately undertook to ascertain the practicability of procuring the beech-nut in sufficient plenty, as well as the sources from which he might derive the necessary assistance for establishing the business. He met with no encouragement in his guarded applications to men of capital, till after he had taken out a patent, at which period he found

no difficulty in obtaining a subscription of twenty thousand pounds, which he raised by engaging to grant annuities of 50 per cent. on the sum subscribed. He afterwards raised one hundred thousand pounds, upon an engagement to receive beech-mast from the subscribers, at 100 per cent profit to them, for two years. With this stock, he proceeded to establish his works and procure orders; the latter of which, amounted to upwards of two and twenty thousand tuns of oil, at 40l. a tun, or near a million sterling. The reports concerning the beech must appear to shew, that the quantity in plentiful years is prodigiously great; but that this happens about every third year. It also appears in his foreign correspondence, not only that the beech-nut is very plentiful on the continent, but that its application to the purpose of manufacturing oil, had been known and practised for near a century in the northern parts of France. One of his correspondents points out, that this oil is preferred by many to the best olive oil; that it had long before been mentioned with commendation by Evelyn, in his *Discourse on Trees*; and that in Bretagne, the millers convert the beech-mast into oil, for the price of one sous per bushel, and the oil-cake which is used for feeding cattle.

After this mass of apparently good evidence, I should be glad to know, if it be my ignorance only which leads me to suppose that there is little, if any, beech oil in the market? If a bushel of beech-nuts be worth less than sixpence, and will afford, by mere mechanical pressure, two gallons of oil, equal in quality, or even much worse than olive oil, how happens it that the consumer now pays five shillings a gallon for whale oil for lamps, and sixteen shillings a gallon for olive oil, in flasks?

To return to my history.—It appears on the whole, that Aaron Hill did manufacture this oil, and that the samples were highly approved by the British clothiers, who gave him large orders; that the year immediately subsequent to the formation of his subscription company, proved very unfavourable for the beech-mast, which was lighter and less oily than in good seasons; that his subscribers became alarmed after having paid their first instalment, which was subject to forfeiture in case of failure in the last payment; that the patentee, instead of insisting upon this bargain, had the honourable spirit to offer them a repayment of their money, with a profit of twenty-five per cent.—that this offer was accepted, and actually performed to the whole of the second class of subscribers, and one-third part of the original annuitants; the remainder, to the amount of about thirteen thousand pounds, choosing to retain their annuities.

Here I must remark, that the scheme seems to have realized some considerable profit, since the advantage allowed to the subscribers over and above their money returned, must have been near eight thousand pounds; besides which, there were undoubtedly many expenses and disbursements made. Is it to be supposed, that these were all paid out of the thirteen thousand pounds which were not withdrawn?

The object of the pamphlet was to circulate proposals for establishing a new company, to which the whole patent, subject to the annuities, was to be assigned for twenty-five thousand guineas, and one-twentieth part of the profits, under certain general regulations, expressed in a deed, enrolled in Chancery for that purpose. The shares were five thousand, at forty pounds a-piece: besides the premium of five guineas on each share. What became of it afterwards, I know not.

I remain, Sir,

Your constant reader, X. X.

London, July 6, 1799.

XI.

Letter from Mr. TROUGHTON respecting a Balance of his Construction.

To Mr. NICHOLSON.

SIR,

Fleet-street, July 9, 1799.

YOU will not, I apprehend, think yourself at all obliged to the person who communicated the information expressed in your note, page 101, vol. III. of your Journal, when I assure you, that the beam which I made for Sir Geo. Shuckburgh Evelyn, had circular rings fastened inside the cones; without which necessary precaution, it would most certainly have been good for nothing. The other difference is most trifling; however, I will tell you, that the sensibility of his balance is not perceptibly diminished, when the beam deviates three degrees from the horizontal position; and that the crystal planes were set and levelled within half that number of minutes, by other means than those pointed out in your note; and your informant may, perhaps, be able to shew the mighty difference between the axis being inclined to the planes, and those planes being inclined to the axis. It is most true, that the conical form of this beam is borrowed from that of the Royal Society: but the former being designed to weigh twelve times as much as the latter, I saw ample cause, in other respects, to deviate from that model.

After all, those who are acquainted with the simple principles of the balance, and ease of constructing it, will hardly think the maker of either instrument in question, entitled to any higher credit than what is due to good workmanship: and more especially so, was it generally known, that there is an instrument in the assay-office, Tower of London, and which was used there by Sir Isaac Newton, when he was assay-master, to which the Royal Society's balance bears full as great an affinity both in form and properties, as Sir George's does to the latter.

I am, sir, relying on your candour, and thankful for the information and pleasure which I receive from the constant perusal of your Journal,

Your most humble servant,

EDWARD TROUGHTON.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

AN important work of Mr. Kirwan is now in the press, and nearly ready for publication, under the title of "Geological Essays; comprehending, the origin and constitution of mountains, seas, lakes, coal-mines, beds of salt, and of metallic ores, and evincing the accuracy of the details of the formation of the earth and of the deluge, delivered by Moses, from the laws of nature, and the appearances it exhibits in its present state. By Richard Kirwan, Esq. F.R.S."

Proposals

Proposals have been circulated by Mr. A. Q. Buée, a French clergyman at Bath, for publishing, by subscription, a work, entitled, *Recherches Mathématiques sur la Texture intime des Corps*; or, Mathematical Enquiries concerning the intimate Texture of Bodies; of which he is the author. It will be printed on fine paper, and illustrated with six copperplates. The manuscript is in the hands of the printer, and the work will be put to press as soon as one hundred and fifty subscribers shall be obtained at half a guinea each: the price will be greater to non-subscribers. Messrs. Dulau and Co. Cox, White, and Phillips, are authorised to receive subscriptions.

The author of the above treatise has drawn up and printed (in French) an outline of its contents, in twenty-three octavo pages. From the perusal of this, I gather that it is a work of considerable novelty and importance. Whether any philosopher has before undertaken to solve the phenomena of nature, by the universal combination of projectile forces with the attractive power, in the particles of matter, is to me unknown; and it is evidently impossible for me to speak of the manner in which he has treated this curious subject. I am aware also, of the difficulties and probability of mistake attendant on an endeavour to give an outline of an outline. In fact, there must be a large part of the author's sketch which will be unintelligible, without reference to the treatise itself; notwithstanding which considerations, I am persuaded that my readers will be pleased to know something more of this object.

The author begins his sketch, by stating that we are acquainted with two facts concerning the intimate texture of bodies: namely, their crystallization, which shews that their elements are disposed in right lines; and their dilatation by heat, which shews that those elements are not in contact. From the two grand laws of attraction, following the inverse ratio of the squares of the distances, and that of inertia, the mutual action of the elements upon each other may be expressed by an algebraic equation: this may be called the equation of the material universe. The author could not enter upon it in his sketch, and therefore only observes, that, according to this equation, each element describes a line, which if there were but three elements present, would be the same as is well known in physical astronomy in the *problem of the three bodies*, but universally is the result of as many small arcs of conic sections as there are other elements.

As some of the conic sections return into themselves, and others do not, the elements will be some planetary, and some cometary; the latter being distinguished from the former, by a greater initial velocity. But the cometary elements arising in the vicinity of other elements are disturbed, and made to circulate round a certain number of elements; the planetary elements oscillate.

Absolute repose or equilibrium has, therefore, no place in bodies, except eventually and for minute portions of time; but apparent repose is produced by the rapidity of oscillation in the planetary elements, and the constancy of their greatest and least distances: this apparent repose implies symmetrical arrangement, and the great agents of this symmetry are the cometary elements.

The doctrine of symmetry is applied to the explanation of chemical facts. Four kinds of aggregation include all the possible systems of elements: 1. Igniform aggregations; containing
only

only cometary elements: 2. Aeriform; containing more comets than planets: 3. Liquidiform; in which the planets exceed the comets: 4. Solidiform; containing planets only. These states may not, perhaps, exist purely and distinct from each other in nature. Another fifth state is, that in which no aggregation takes place. The latter cometary elements are the particles of light, of which the colours, the reflection, refraction, diffraction, absorption, double refraction, the Newtonian fits, &c. are explained by analysis; together with those results, in which light is said to enter into combination. The formulæ, which relate to the igneous aggregation, are applied to caloric, electricity, and magnetism. The aeriform aggregation exhibits the phenomena of fluidity, compressibility, hydrostatics, and sound; and under the article of the solidiform aggregations, some observations are made respecting impulse, elasticity, mechanical division, and re-union: and the causes which produce crystallization, vegetation, and animalization. When two distinct bodies approach each other as nearly as possible, without ceasing to be distinct, and by the influence of the elements of the one upon the elements of the other, their centres of gravity acquire a new motion; this operation is called impulse. Mechanical division is the separation of the parts of a body, by contrary impulses given to those parts: if the direct contrary operation could be performed, mechanical union would take place; this is performed to a certain extent when polished surfaces adhere by application to each other.

With regard to the other doctrines of crystallization, vegetation, and animalization, in which, I presume, there must be some principles assumed as data, which may require farther experiment; it would answer no useful purpose to enumerate the results. For the development of these, we must wait till the work shall appear.

Death of Galvani.

The celebrated philosopher, Galvani, died lately at Bologna, at the age of fifty-five. His name has been given to the discovery of the influence of the contact of two metals on the animal economy. The circumstance which occasioned this discovery, is not, perhaps, generally known. The wife of Galvani took soup of frogs on account of her health; her husband, who was much attached to her, had skinned several frogs, and on touching them by chance, he unintentionally made a communication, which produced the singular phenomenon, since known by the name of Galvanism. The account to be published by the commissaries of the institute, and the memoir of Baron Humboldt, in the *Journal de Physique*, for the month of Prarial, an 6, give an ample account of this curious discovery. Dr. Aldini, nephew and co-operator with Galvani, is employed on a continuation of experiments calculated to elucidate this phenomenon still farther.

Lalande, in the *Magaz. Encycl.* V. 551.

On

On the Acoustic Experiments of Chladni and Jacquin.

M. E. Perrole, some of whose experiments on sound are inserted in our Journal, I. 411. has written to Citizen Delametherie* a letter, in which he expresses his doubts respecting the truth or accuracy of the experiments of Chladni and Jacquin, announced at p. 43 of our present volume. His arguments are, in short, that Dr. Priestley, who made experiments with a bell under a jar, successively filled with the several gases, as well as himself, did not perceive any such difference of tone. He candidly takes notice, that these last experiments are different in their circumstances from those, in which a flute was used; but does not seem disposed to consider the difference as of much importance. On this head it may be sufficient to remark, that the difference, if theoretically considered, is indeed very great: and that the practical results may most conveniently be established, or refuted, by the test of experiment.

Effect of Hydrogen Gas on the Voice.

Otier, in the *Bibliothèque Britannique* †, informs us that Mauvoir, at Geneva, amusing himself by respiring pure hydrogen gas, found it attended with no sensible effect, either at the inspiration, or when expired; but when after having inspired a considerable quantity (une forte dose) he attempted to speak, he was strangely surprized that the sound of his voice had become shockingly hoarse and shrill. M. Paul, at whose house this trial was made, repeated it upon himself, and experienced the same effect.

* Journal de Physique, V. 455.

† Quoted by Delametherie, J. de Phys. V. 459.

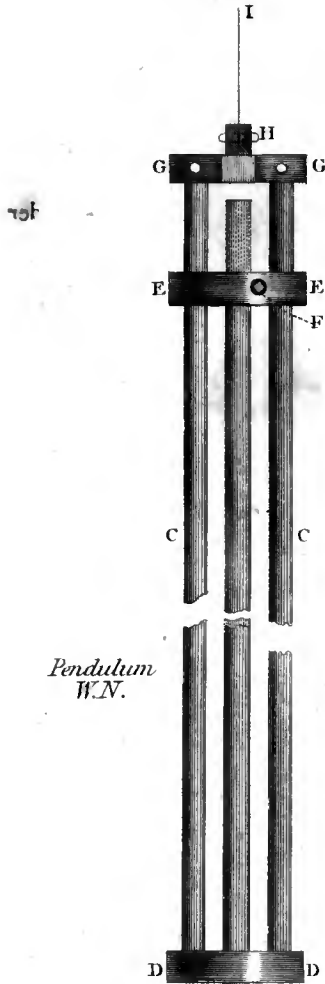


Fig. 2.

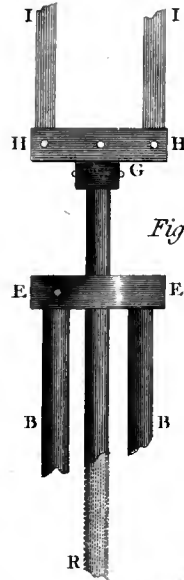


Fig. 3.

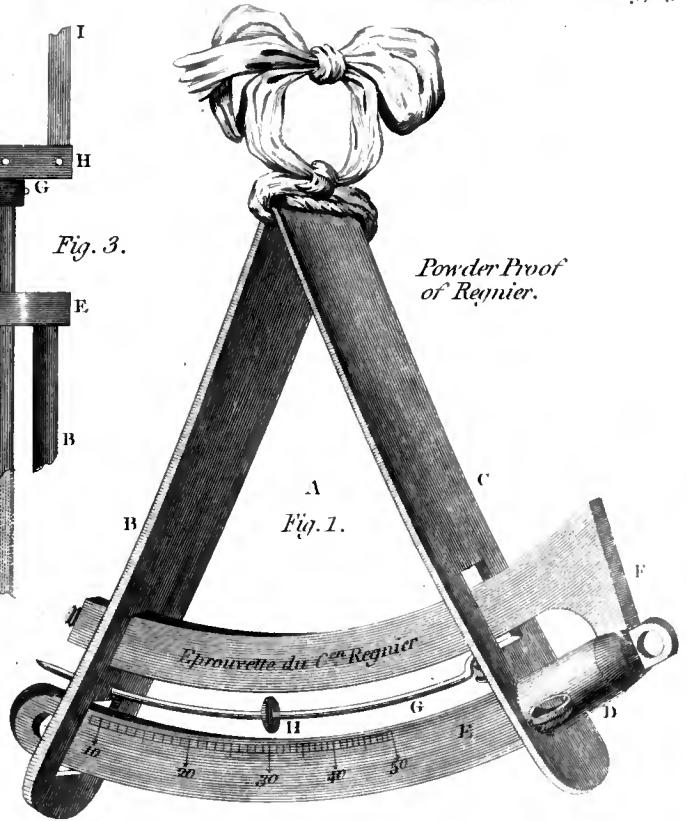


Fig. 1.

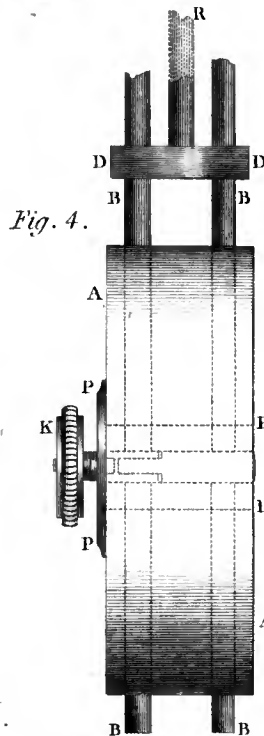


Fig. 4.

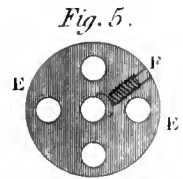


Fig. 5.

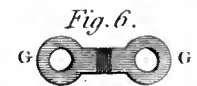


Fig. 6.



Fig. 7.



Fig. 8.



Fig. 9.





A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

SEPTEMBER 1799.

ARTICLE I.

A circumstantial Description of the Method of cultivating the White Beet (Runkelrübe), in order to obtain the greatest Quantity of Saccharine Matter, and to prepare it for the Manufacture of Sugar. By F. C. ACHARD, Director of the Physical Clafs in the Royal Academy of Sciences.*

IN the course of several years past I have made experiments to ascertain how far various native plants might be fit for making sugar. In these enquiries I had the opportunity of observing, that the quantity of saccharine matter, and its proportion to the other constituent parts of the same species of plants, may be increased or diminished by the manner of cultivation.

2. Among the various plants which I examined, for the purpose of making sugar in this country, I paid particular attention to the several species of *beta vulgaris Linnæi*; one of which, peculiarly proper for manufacturing sugar, is known to the economist in this country by the name of *runkelrübe*†, and to the gardener more especially by the name of *mangoldrübe*. On

* Translated from "Ausführliche Beschreibung der Methode nach welcher bei der Kultur der Runkelrübe," &c. von F. C. Achard, Director, &c. Berlin, 1799. Octavo, 63 pages.

† As no botanist has given the characters of this Runkelrübe, Beckmann describes it in the following manner: *Beta altissima, floribus ternis vel quaternis, foliis calycis inermibus, carinatis, caule crassissimo fasciato, radice maxima, rubro et albo intus variegata, foliis maximis rubentibus.*—Note of the translator, from Nöldecker's treatise on the culture of this plant.

comparing the several varieties of this kind of plants, I was convinced, that the particular variety, possessed of a *long conical root, red rind, and the interior part white*, was the most abundant in sugar, and that the saccharine contents of this variety of the *beta vulgaris*, called *runkelrübe*, may be greatly augmented or diminished, according to the various methods of cultivating it.

3. Having treated this root by various methods of culture, I obtained sugar from it, with more or less profit; in some instances with loss, or even no sugar at all; but frequently a mere extract, in the form of a pulp, smelling like turnips; in which, from the excess of extractive matter, no sugar could crystallize, unless some expensive artificial expedients, not applicable in the large way, had been employed. These observations have convinced me of the great influence of cultivation on the saccharine contents; and I spared no industry in order to discover the management by which this root might be cultivated of the greatest richness in sugar. I have not only raised them under various situations on my estate, *French Buckholtz*, but have procured such roots from various other territories, as from *Magdeburgh, Halberstadt, Brunswick, Blankenburg, Ciesar, and Nauen*, with accounts of the methods by which they were cultivated.

4. I then compared the results of the experiments which I had instituted for the purpose of obtaining sugar, partly from roots of my own in different circumstances of growth, and partly from those of other soils, which were likewise differently cultivated.—By this means I have found that the saccharine matter of this root may be considerably increased, and the extractive matter considerably diminished. The conditions are: (a). That it should be cultivated in a rich soil, which will be best adapted to it, if it be rather compact.

(b). The seed is not to be sown in one bed, and the plants afterwards removed to another, as is commonly done; but, on the contrary, they must be suffered to ripen on the spot in which they germinate from their seeds.

(c). The roots must not be too distant from each other. In the best soil, their mutual distance should be one foot: in a poorer soil, still nearer,—nine inches at most.

(d). After they have sprouted, they must be cleared of the weeds, either by the hoe or by pulling: taking care, when the hoe is used, that the earth be not removed from the plant; but rather, though slightly, brought nearer to it. This is not necessary, when the weeds are pulled up. It is usual to remove the earth from the plant, when it is cultivated to serve as food for cattle; for its upper part is, by this management, greatly enlarged, and it acquires a greater mass in the whole: but such management is highly detrimental to the *runkelrübe* intended for making sugar.

(e). The leaves must not be taken off from the plant, as is the custom, for the purpose of feeding cattle. This treatment diminishes the saccharine matter of the root, at the same time that it increases its mucilaginous, earthy, and farinaceous parts; and, consequently, is very injurious to the quality of the root, if appropriated for making sugar.

5. On these five positions, which are grounded on repeated experiments and observations, I can with justice and truth insist; and that, with respect to the manufactory of native sugar, if
carried

carried on with profit from the *runkelrübe*, every thing depends on its proper culture. For it is by this means only that the increase of its saccharine contents can be promoted; and it is only from the quantity of this last product that sugar can be made with profit from that root in the large way.

That this root contains sugar, has been long since proved by my celebrated predecessor in the Royal Academy of Sciences, the late Director *Margraaf*. But it was then unknown and unsuspected that it could be obtained from it in the large way, and so cheap as 2 *groshes* (about three pence English) for the pound of crystalline raw sugar*, and in some trials still cheaper, as I have demonstrated to be practicable, by the experiments made in the presence of the committee, selected for that purpose, by the king's command. This result is different from all trials hitherto made in this respect, by the most able chemists. The cause is simply, that the great influence which the culture of the *runkelrübe* has, with regard to the increase of its sugar, has not been suspected, and that the different modifications of that culture were unknown; though, in fact, the quantity of saccharine matter may, on the one hand, be highly augmented, while, on the other hand, the proportion of those constituent parts, which prevent the separation of the sugar, are greatly diminished.

6. From the method before described, of producing the *runkelrübe* abundant in sugar by means of a proper cultivation, and from my other observations on the most profitable management of this root, the following instructions may be taken for its cultivation:

A soil upon which wheat has grown is to be chosen, and kept in good condition. A low situation, not exposed to great or lasting drought, yet without being moist or swampy, is to be preferred. It is better if manured the year before than recently; which, however, must be done, if the former manuring has been omitted. This ground is to be ploughed thrice over, and as deep as the nature of the soil will admit. It is also very advantageous to perform, if possible, the first tillage in Autumn. Immediately after the third ploughing, which should be done in the middle of April, or, at latest, about the middle of May, the ground is to be smoothed by the harrow, as much as possible; and by means of a rake, whose teeth are distant from 9 to 12 inches; lines are to be traced along the surface, and by drawing the rake in lines across these the ground becomes divided into squares, measured by the distance of the rake's teeth†.

* In this computation the labourer's wages are taken at 8 *groshes* per day, and the fuel at the price of the royal Berlin wood-market. Besides, in my present experiments, a circumstance occurs which greatly enhances the expences of particular operations: namely, that the processes cannot be *concatenated*, or connected with each other. Notwithstanding this, the pound of raw sugar will cost, at the highest, only 2 *groshes*, and by some methods still less; because the manufacturer will not pay the day wages at highest rate, nor chuse Berlin, the dearest place, for establishing his manufactory. Thus, also, he will not buy his fuel at the greatest or retail price, but will take all the advantages resulting from the connection of the several processes, which do not take place in single trials, as they must in carrying on the business at large.

† As I have not spoken with precision of this distance, but have only given it from 9 to 12 inches, I must here remark, that it must be regulated by the goodness and richness of the soil. On the best ground the distance of 1 foot is most suitable: on a leaner soil, less manured, it is better to contract it to 9 inches.

7. Into each intersecting point of the lines delineated by the rake, one single seed capsule, if you are convinced of its good quality, is to be stuck in. But if not, then two such capsules are to be put in; and, in either case, to the depth of an inch. This operation may be done by children, or inferior labourers. When the plants have germinated out of the ground, and six or eight leaves are formed, the weeds must be destroyed or pulled up; but, as I have already observed, the removal of the earth from the plant is to be very carefully avoided. It answers better to push the earth nearer to the plant, though this may be neglected without any bad consequence. At this period of the culture there is another operation to be performed. If the plants be too much accumulated on a particular spot, which is often the case when very good seed has been used, because one capsule contains several seed grains, and produces more plants than one on the same spot; in this case the superabundant plants are to be pulled out.

8. There is no occasion for this operation, if the seed has not been quite fresh, or not quite ripe. But in case some empty places should be found, where nothing has grown up, two fresh grains should be inserted. After the ground has been once cleared of the weeds, the plants grow up so speedily, that their leaves soon completely cover the ground; and thus absolutely prevent the growing of any more weeds. In consequence of this, and to the great advantage to the farmer, an acre of ground cultivated with *runkelrübe*, occasions no more trouble till the time of gathering: which circumstance greatly facilitates their cultivation; because the time of the cultivator, who is then busied in his corn harvest, is not required to be at all employed on this object. For the gathering of these roots begins only towards the end of September, and may be continued to the end of October, if no early frost sets in.

9. At this gathering nothing particularly remarkable occurs, except that the root must be as little injured as possible; partly to prevent the loss of its juice, and partly to prevent the decay to which the wounded parts are more exposed than the sound ones. The verdure must then be cut off, in such a manner, that the *heart* may be also separated, in order to prevent the germination of the root. Too much should not, however, be lopped off the head; because the juice would, in that case, too plentifully exude. These leaves and hearts are exceedingly valuable to the farmer at this time, when other green food for his cattle is wanting.

10. The roots may be kept for use in ditches dug in the earth, where the depth, the dryness, and the loose nature of the soil, admits of it. Where this is not practicable, on account of the moisture and firmness of the soil, they may be preserved and secured against the frost in cellars. But the heart must not be taken out nor injured in such roots as are kept during the winter, in order to obtain seed from them by transplantation in the spring. The leaves are merely to be broken off. The roots must be well covered during winter, and sheltered against frost. In general, in the production of the seed from the *runkelrübe*, the same method is to be used as with other biennial roots and species of cole. As this procedure is known to every economist and gardener, I shall lose no time by describing it.

11. When large districts of ground are to be cultivated with this plant, the seed capsules cannot, for want of time, be singly put into the ground. The seed must, therefore, be sown with

with the greatest possible uniformity. The Magdeburg* acre will require from 3 to 4 pounds of seed, according to the quality of the soil. If the sowing has been well performed, the plants will be distant 9 inches, or a foot at most.—In case the roots grow nearer to each other, they do not contain less sugar on this account, but they remain small. If too far asunder, they grow larger, but abound less in sugar. It is, therefore, less detrimental to sow too thick, than too sparingly.

12. It has before been remarked, that the practice of pulling the leaves from the plant ought to be carefully avoided; but this observation relates only to the green vegetating leaves. The under leaves frequently turn yellow and die, and, in these circumstances of decay, they may be taken off, and will afford the farmer some assistance in a scarcity of food, without injury to the culture of our root.

13. Respecting the choice of the seed, besides its early and perfect ripening, regard must be had that it be not obtained from roots which, after their germination, have been transplanted on seed-beds; but from such as remained on the spot where they grew from the capsules, till autumn, and which likewise have produced the true oblong, thin, conical roots†. This is necessary, because such a seed from untransplanted plants produces roots more partaking of the spindle-form; whereas the seed from the transplanted roots forms thicker, and, at the same time, shorter, and on the lower parts roundly terminated roots.—The *Art of Gardening* affords numerous instances of the effect of this management of seed plants, different from those that are to be produced from their seed. The seeds obtained from untransplanted lettuce yield, on being sown, plants which but extremely seldom form any heads, and never obtain any firmness. The seed of a loose, and not transplanted, cabbage, never produces white cabbage, but a loose cole, not shooting into a head. The seed of fellery, if not procured from a plant, which by transplantation has been formed into a knob or nodule; but from fellery which, for want of transplantation, has produced rather fibrous roots, yields, on being sown, only herb, and no nodules. I am convinced of the truth of these assertions from my own experiments, and appeal to what *Luder* and *Germerhausen* have written on this subject, as men whose science and accuracy will not be disputed.

14. Among the spindle-shaped runkelrübes there exists a variety as to their colour. Some have a pale red rind, and are internally quite white; others with a rind usually of a more deep red, are internally striped reddish; others again of a more or less deep red have red circles; and, lastly, there are some, which, with an almost white rind, have the internal part yellow.—Those which are white, with a light red rind, deserve the preference beyond all others. For they yield much sugar, and an agreeable sweet syrup; which, if well prepared, has no taste of the root.

* The author has not said whether he means the large or the small Magdeburg or Berlin acre.—The former contains 53771, and the second 24197 French square feet. Probably he understands the large, in the common use of the language.—Translator.

† It is in autumn, as noticed before, that the runkelrübes must be gathered, and kept, during the winter, defended from the frost; because they are biennial plants.

15. The red-striped or circled roots, whose rind also is always of a darker colour, afford sugar indeed; but the syrup is bad, on account of its taste of the root, which cannot be removed but by expensive chemical processes. The *runkelrübes* of a white rind and yellow internal part do certainly afford most sugar, which shoots very readily into large crystals; but their syrup being of an extremely disgusting taste, is of no use, when raw sugar only is made. Even the sugar itself, prepared from these roots, is not easily, but with difficulty, cleared of that taste in the condition of raw sugar, though it certainly disappears in refining. For this reason, this last variety of the *runkelrübe*, that contains so much sugar, is not to be totally rejected, but is rather profitable in the manufactory of sugar. More especially, if not intended to be employed as raw sugar, and if the acquisition of the syrup be disregarded.

16. It is sufficiently proved, from the physiology of plants, that the *matter of light* has a great share in the formation of some of their constituent parts, as to quality, and consequently on their mutual proportions. I shall here mention one or two instances.

While asparagus is defended against the light, it becomes sweet, and of a pleasant taste; but if light has acted upon it, merely for a short time, it loses its sweetness and turns bitter.

Endive likewise has a tough, harsh tasted leaf, provided it be exposed to the free action of light. On the contrary, when the inner leaves are defended, by tying the outer ones together, they change their colour, which passes from green to yellow; the firmness of their texture is weakened; they become tender, soft, brittle, and full of juice; and their taste, which was almost disgusting, becomes mild and agreeable. All these changes produced in the external appearance and flavour of plants, by the presence or absence of the matter of light, can only originate from the *different modifications*, which this matter of light effects either in the nature of their constituent parts, or in their mutual proportions.

17. These observations, depending on results generally known, led me several years ago to make experiments, in order to discover whether the matter of light contributes in general to the *increase* or *decrease* of any certain constituent part of plants, *principally* and *exclusively*? whether this action have the same efficacy upon all the parts of plants? or whether the matter of light does *increase* or *diminish* the same constituent part of a plant, in its various parts, as *roots, leaves, fruit, &c.*? To avoid prolixity, I shall not describe the several experiments I have made for the purpose of answering these questions, either with plants growing in open air, or cultivated in hot-houses, and more especially those made with the sugar-cane in the hot-house. I shall only mention those results which bear any relation to the present subject, adding such observations, or well-known facts, as tend to confirm those results.

The following are the results of my researches:

(a) That the absence of light augments the saccharine liquor in almost all roots, or in the germs arising from them; that its presence diminishes it; and that to shade the whole surface of a piece of ground on which such roots are raised, adds very much to the increase of their saccharine matter.

(b) That the matter of light has not the same efficacy, but rather one of an opposite kind, on the other parts of plants; for instance, the fruits, the saccharine matter of which is augmented

mented by the presence of the matter of light, and, on the contrary, by its absence the formation of sugar in them not only *retarded*, but in every case *diminished*.

(*The conclusion in our next.*)

II.

*Appendix to Sir GEORGE SHUCKBURG EVELYN'S Paper * on the Means of ascertaining a Standard of Weight and Measure.*

(§. 44.) **SINCE** the writing of the preceding Memoir, I have had an opportunity of examining three other scales, divided into inches, or equal parts, of considerable authority in this country, having been executed by the late Mr. J. BIRD. I have also compared the old standard in the Exchequer, of the time of Henry VII. and which is considered to be the most ancient authority of this sort now subsisting: these observations, I flatter myself, the Royal Society will be desirous of possessing.

(§. 45.) The first of the abovementioned scales belonged to the late General ROY, and was purchased by him at Mr. Short's sale, the celebrated optician; it was used by him in his operations of measuring a base line on Hounslow-heath. (See Phil. Trans. vol. LXXV.) It was originally the property of Mr. G. GRAHAM, has the name of JONATHAN SISSON engraved upon it, but is known to have been divided by Mr. BIRD, who then worked with old Mr. SISSON. It is 42 inches long, divided into tenths, with a vernier of 100 at one end, and of 50 at the other, giving the subdivisions of 300ths, and 1000ths, of an inch.

(§. 46.) The second is in the possession of ALEXANDER AUBERT, Esq. and formerly belonged to Mr. HARRIS, of the Tower; contains 60 inches, divided into tenths, with a vernier, like that of the preceding. It is one inch broad, and 0,2 thick.

(§. 47.) The third was presented by Alexander Aubert, Esq. and the late Admiral Campbell, Mr. Bird's executors, to the Royal Society, in whose custody it now remains. It consists of a brass rod, 92,4 inches long, 0,57 inch broad, and 0,3 inch thick; bearing a scale of 90 inches, or equal parts, each subdivided into 10, with a vernier at the commencement, being a scale of 100 divisions to 101 tenths. *This* has been called Mr. Bird's own scale, *viz.* made for his own use; and was the instrument with which he is said to have laid off the divisions of his 8-foot mural quadrants. It is probable that Mr. Bird made many more of these scales, now in the hands of private persons (one of which, indeed, I saw at the President de Saron's, many years ago, at Paris), but those have not come to my knowledge.

(§. 48.) In comparing General Roy's (Bird's) scale with Mr. Troughton's, I found 42 inches of the former were = 42,00010 inches on Troughton's; (the thermometer 51°, 7;) 36 inches were consequently = 36,00008.

* Which was concluded at p. 205.

And 12 inches on the 1st foot were equal to the 12 inches	}	— ,0003 =	inches. 11,9997
from 12 to 24 on Troughton's scale			
The 2d foot	-	+ ,0006	12,0006
The 3d foot	-	— ,0004	11,9996
The last foot	-	+ ,0006	12,0006

The mean foot, therefore, in General Roy's scale, taken from four different feet compared with Troughton's between the 12th and 24th inch, is as 12 to

That is, general Roy's scale is longest on 1 foot by so much, and longer on 3 feet by

And the greatest probable error from the inequality in the divisions is about

And the mean probable error about

(§. 49.) Mr. Aubert's scale, compared with Mr. Troughton's, was as follows: 58 inches were equal to 57,9982 inches on Troughton's; (thermometer at 51°,0;) viz. Mr. Bird's measure was shortest, .0018; or, shortest on 36 inches = .0012,

And 12 inches, or 1st foot, on Mr. Aubert's	= 11,9999	} on Mr. Troughton's scale from 6 inches to 18 inches; the thermome- ter being at 50°,0.
2d foot,	= 12,0005	
3d foot,	= 11,9996	
4th foot	= 12,0019	
5th foot	= 12,0006	

Therefore the mean foot is

The greatest error in this scale appears to be about = .0012

And the mean probable error = .0006

(§. 50.) The Royal Society's scale, compared, was as follows: 58 inches on Mr. Bird's were equal to 57,99912 inches on Mr. Troughton's; (thermometer 50°,5;)

viz. Mr. Bird's measure was shortest

Or shorter on 36 inches

32 inches on the same were equal to

viz. Mr. Bird's was shortest by

Or, on 36 inches, by

The mean of these two comparisons is

And, by so much, is Mr. Bird's scale shorter, in three feet, than Troughton's.

And 12 inches, or 1st foot, of the Royal Society	} on Troughton's scale; the thermometer at 51°.
scale, is	
2d foot of ditto	
3d foot of ditto	
4th foot of ditto	
5th foot of ditto	
6th foot of ditto	
7th foot of ditto	

The mean of these seven feet is - = 11,99982

And the greatest error in these divisions - = ,0008.

And the mean probable error - - = ,0004.

(§. 51.) Left, however, it should be suspected, that Mr. Troughton's scale, with which I have made these comparisons, is not sufficiently correct for this apparent preference, I will now give the result of my examination of that scale, from one end to the other. I set the microscopes to an interval of nearly 6 inches, correctly speaking it was 6,00013 inches, taken from a mean of the whole scale; and, comparing this interval successively, I found as follows:


inches. inches.		inches.					Error, or difference from the mean.
viz. from	0 to 6	-	-	=	6,00025*	-	+ ,00012
	6 to 12	-	-	=	6,00013	-	,00000
	12 to 18	-	-	=	6,00020	-	+ ,00007
	18 to 24	-	-	=	6,00000	-	- ,00013
	24 to 30	-	-	=	6,00007	-	- ,00006
	30 to 36	-	-	=	6,00033	-	+ ,00020
	36 to 42	-	-	=	5,99980	-	- ,00033
	42 to 48	-	-	=	6,00020	-	+ ,00007
	48 to 54	-	-	=	6,00010	-	- ,00003
	54 to 60	-	-	=	6,00023	-	+ ,00010
Mean of all		-	-	=	6,00013		

From whence it appears that the greatest probable error, without a palpable mistake, in Mr. Troughton's divisions, is = ,00033 inch; against which the chance is 9 to 1; and the mean probable error = ,00016; and that it is 4 to 1 the error doth not exceed $\frac{1}{10000}$ inch.

This accuracy is about three times as great as that of Mr. Bird's scales, and about equal to that of the divisions of my equatorial instrument, made by Ramsden, in 1791. See Phil. Transf. for 1793.

(§. 52.) I now proceed to the examination of the standard rod of Henry VII. which is an octangular brass bar, of about $\frac{1}{2}$ an inch in diameter, with one of the sides rudely divided, into halves, thirds, quarter, eighths, and sixteenths; and the first foot into inches. Each end



is sealed with a crowned old English H () and from hence is concluded to be of the time of King Henry VII. viz. about 1490, but is now become wholly obsolete, since the

* It is not pretended, that in this and the foregoing observations, the quantity of any interval can be determined to the precision of the one-hundred-thousandth part of an inch; but it is presumed, that with the assistance of the microscopes, the ten-thousandth part of an inch becomes visible; and, as a mean is taken from 3 or 4 times reading off the micrometer at each trial, it has been deemed not unreasonable to set down the quantities to five places of decimals.

introduction of the standard of Queen Elizabeth; but such as it is, I have thought proper to examine it, and find it as follows: inches.

On this rod, $\frac{1}{3}$ or the 1st foot, is equal to 11,973 on Troughton's.

the 2d foot is	-	11,948			
the 3d foot is	-	12,047			
The mean foot is		11,989	Difference.	Error, of difference of 3 feet.	
$\frac{1}{2}$ yard, or 18 inches	-	= 17,946	— ,011	— ,033	
$\frac{2}{3}$ yard, or 24 inches	-	= 23,921	— ,054	— ,108	
$\frac{3}{4}$ yard, or 27 inches	-	= 26,937	— ,079	— ,118	
$\frac{7}{8}$ yard, or $31\frac{1}{2}$ inches	-	= 31,443	— ,063	— ,084	
$\frac{15}{16}$ yard, or $33\frac{3}{4}$ inches	-	= 33,665	— ,057	— ,065	
Entire yard, or 36 inches	-	= 35,966	— ,085	— ,091	
			— ,034	— ,034	

And the mean yard - - = 35,924 Mean — ,076

And, by so much, Mr. Troughton's measure is longest.

And the probable error, in the divisions of this old standard, is about $\frac{3}{1000}$ inch.

(§. 53.) It may now be desirable to see the comparative lengths of these various standards and scales, reduced to one and the same measure, viz. Mr. Troughton's.

36 inches, on a mean, of Henry VII. standard of 1490, are			Inches on Troughton's.	Difference.	Probable error in divisions.
equal to	-	-	35,924	— ,076	,03
— of standard yard of Elizabeth, of 1588	-	-	36,015	+ ,015	,04
— of standard ell of ditto, of 1588	-	-	36,016	+ ,016	,04
— * of yard-bed of Guildhall, about 1660	-	-	36,032	+ ,032	
— * of ell-bed of ditto, about 1660	-	-	36,014	+ ,014	
— * of standard of clock-makers' company, 1671	-	-	35,972	— ,028	
— * of the Tower standard, by Mr. Rowley, about 1720	-	-	36,004	+ ,004	
— of Graham's standard, by Siffon, of 1742, viz.					
line E	-	-	= 36,0013	+ ,0013	
— of ditto, ditto, viz. line Exch.	-	-	= 35,9933	— ,0067	
— of Gen. Roy's (Bird's)	all made probably between the years 1745 and 1760.	scale	= 36,00036 + ,00036 ,0003		
— of Mr. Aubert's, ditto,			= 35,99880 + ,00120 ,0006		
ditto			= 35,99955 + ,00045 ,0004		
— of Royal Society's,					
ditto, ditto					
— of Mr. Bird's parliamentary standard, of 1758			= 36,00023	+ ,00023	
— of Mr. Troughton's scale, in 1796			= 36,00000	,00000 ,0001	

* These Four quantities are taken from Mr. Graham's account, in the Phil. Transf. vol. XLII.

From

From hence it appears, that the mean length of the standard yard, taken from the seven first instances in this table, agrees with the quantity assumed by Mr. Bird, or Mr. Troughton, to within $\frac{1}{800}$ inch, but that the latter is the longest.

III.

Observations on the Manners, Habits, and Natural History of the Elephant. By JOHN CORSE, Esq. (Concluded from p. 197.)

DURING the time they were kept together, the male never shewed signs of his passions being excited, by any exudation from the ducts of the glands near his temples; which is generally considered as the sign of a male elephant being peculiarly ready for the female. This, however, I am inclined to believe is a vulgar error; as not one of the male elephants I have seen cover, in a domestic state, nor any of the males which were caught singly, or rather entrapped, by their desires to have connexion with the tame females, had, at those times, the smallest appearance of such an exudation. Had this happened, in any one instance, during my residence in Tiperah, I think I must have known it; for when this exudation takes place, the elephant has a dull heavy look, and it is dangerous for strangers to go near him. I have seen elephants in this situation after they had been many years caught; but though they were then said to have their passions excited, I have never known one to cover during the continuance of this exudation; nor have elephants, so far as I have been able to observe, any particular seasons of love, like horses and cattle. Of five instances of elephants covered at Tiperah, one received the male in February, another in April, a third in June, a fourth in September, and the fifth in October. Besides these, an attempt was made by a tame male to cover in the month of January a wild female, then in heat*. When the female is in heat, the parts of generation shew it, by an unusual fullness of the labia: and, if she is placed near a male, she endeavours, by caresses, to excite his desires†.

After the female had been covered by the male, as has been just related, there being then no other female ready, he was placed with an elephant which had had a young one about four years before this, and some months ago was reported to have been in heat. It was thought, after some trial, that she was likely to permit him to cover, as she caressed him occasionally, and roused his passions; but she would not allow him to gratify his desire.

* Many pregnant females are taken every year at Tiperah, and produce young ones in the different months: this clearly shows, that there are no particular seasons during which the females are in heat.

† It may be proper to observe, that the penis of a full-grown elephant is from two feet four to two feet six inches in length, and from fourteen to sixteen inches in circumference. I caused the penis of two males to be measured, after their passions were excited, in order to ascertain the real size. On some occasions, I have seen the penis absolutely touch the ground, when the elephant has been walking; but it must be recollected, that the hind legs of an elephant are very short, in proportion to his size.

The drivers, tired of this coyness, and stimulated perhaps by the hopes of another gratuity, were so brutal as to tie her, and let the male make an attempt upon her, while tied. His attempt, however, was to no purpose; though he continued his efforts till he appeared to be quite exhausted. This being told me, I severely reprimanded the people; and ordered the female to be left at full liberty to reject or receive the male, as she might think proper.

Here, however, was positive proof, that the male would have effected his purpose by force, when he found he could not obtain it any other way. He remained at Comillah till October, 1793, without my being able to procure a female that was in heat; he was then sent to Calcutta.

I now became extremely solicitous about the health of the female which was covered in June; and gave particular directions not to overheat her, but merely to give her as much food and exercise as were likely to keep her in the best condition, as she was now known to be pregnant. In three months after she was covered, she became fuller, her flesh felt softer, and her breasts began to swell. These marks of her being with young were so evident to the driver, that he mentioned them of his own accord; which convinced me, that an elephant, three months after conception, may be known by the keepers to be pregnant.

She had always been a favourite, from having been the gift of my worthy and respected friend Mr. JOHN BULLER*, as well as from her gentle and docile disposition; and I now had hopes of her going her full time.

She was seven feet three inches high, when covered; but after this increased so fast, not in bulk only, but also in height, as to exceed seven feet eight inches, before she brought forth. On the 16th of March, 1795, she produced a fine male; just twenty months and eighteen days after she was first covered.

The young one was thirty-five inches and a half high; and had every appearance of having arrived at its full time, being the largest I had known produced in Tipperah.

We have many young produced every year, by the females which are taken while breeding, and these seldom exceed thirty-four inches; this, however, may be owing to the weak and reduced state the mothers are brought to, while breaking in.

The young of the elephant, at least all those I have seen, begin to nibble and suck the breast soon after birth; pressing it with the trunk, which, by natural instinct, they know will make the milk flow more readily into the mouth, while sucking. Elephants never lie down to give their young ones suck; and it often happens, when the dam is tall, that she is obliged, for some time, to bend her body towards her young, to enable him to reach the nipple with his mouth: consequently, if ever the trunk was used to lay hold of the nipple, it would be at this period, when he is making laborious efforts to reach it with his mouth, but which he could always easily do with his trunk, if it answered the purpose. In sucking, the young elephant always grasps the nipple (which projects horizontally from the breast) with the side of his mouth.

* Now one of the Members of the Board of Revenue, at Calcutta.

I have very often observed this; and so sensible are the attendants of it, that, with them, it is a common practice to raise a small mound of earth, about six or eight inches high, for the young one to stand on, and thus save the mother the trouble of bending her body every time she gives suck, which she cannot readily do when tied to her picket.

Tame elephants are never suffered to remain loose; as instances occur of the mother leaving even her young, and escaping into the woods.

Another circumstance deserves notice: if a wild elephant happens to be separated from her young, for only two days, though giving suck, she never afterwards recognizes or acknowledges it. This separation sometimes happened unavoidably, when they were enticed separately into the outlet of the *keddah*. I have been much mortified at such unnatural conduct in the mother; particularly when it was evident the young elephant knew its dam, and, by its plaintive cries and submissive approaches, solicited her assistance.

Here it may be observed, that a female was believed to have gone twenty-one months and three days; being supposed to have been covered on the 13th of January, 1788, some days before she was driven into the inclosure. When I made particular inquiry as to the real time she was taken, the superintendant * of the hunters said it was in January; but the principal hunters † declared she was among the herd taken in February following, and was probably the same elephant Mr. BULLER, Captain HAWKINS, and many others, saw covered on the 9th and 10th of that month. Perhaps, some days prior to this, she might have been covered in the woods, before she was brought into the inclosure; but as a herd was taken in each of those months, and not kept separate, and two years had nearly elapsed before I thought of making any inquiry, it was impossible for me to determine in which of those months she was really taken; and the only motive I then had for endeavouring to ascertain this point, was to form some probable conjecture as to the period of an elephant's gestation, which has now been ascertained, in the instance before related.

Early in September, 1795, the female that had been covered, and had been bred under my own observation, was known to be in heat; this was less than six months after bringing forth. Learning, at the same time, that the Rajah of Cudwah, a principal Zemindar of the province, had a very large male that had been in the family near twenty years, from the time he was about five years old, I sent a messenger, requesting the elephant might be sent to Comillah, which request the Rajah immediately complied with.

To prevent any interruption from the number of spectators, the elephants were put into a small inclosure, on the 17th of September; the female was picketed by one leg, and the young one, to which she was giving suck, was tied to a tree at some distance, fearing if permitted to run about he might receive some injury.

After a few caresses from the female, the male at length effected his purpose, and covered her twice the same evening. As the intention of the male elephant's visit was known in the district, and a few days had elapsed since the two elephants were brought together, in

* The *Déogab*.

† The *Dydars*.

order to make them acquainted, the number of spectators was greater than on any other similar occasion.

She was afterwards covered, several times, on the 20th of the same month; the male, in this case, being admitted after an interval of three days, although formerly, in June, 1793, she refused him when only two had elapsed. She again proved with young; and in November, 1796, being myself in a bad state of health, and under the necessity of returning to Europe, I sent her to Lucknow, together with her young one, at the request of my friend Captain DAVID LUMSDEN: though she was then very big, she was still giving suck.

About a month before that period, I got my friend, Mr. STEPHEN HARRIS, to permit a female of his to be covered; the same which had, in 1793, rejected the attempts of the male to cover her contrary to her inclination. Another messenger was dispatched to Cudwah, for the Rajah's elephant, which was again sent to Comillah. He covered her repeatedly, on the 14th, 15th, and 16th of October, 1796, before many Europeans, as well as natives; and the last time he covered her, it was evidently contrary to her inclination; so that, in fact, he used force to effect his purpose, and held her so firmly, that the marks of the nails of his forefeet were deeply imprinted on her shoulders.

Having mentioned a sufficient number of instances, to prove the ability, as well as the inclination of the elephant, to propagate his species in a domestic state, and that without any signs of modesty, and having ascertained the period of gestation to be twenty months and eighteen days, it may be necessary to observe, that it is a difficult matter to bring a male which has been taken about the prime of life, into good condition to act as a stallion; for, being naturally bolder, and of a more ungovernable disposition, than the female, he is not in general easily tamed, till reduced very low; and it requires considerable time, as well as much expence and attention, before he can be brought into such high order as is requisite. He must also be of a gentle temper, and disposed to put confidence in his keeper; for he will not readily have connexion with a female, whilst under the influence of fear or distrust. Of this I have seen many instances; nor do I recollect one male elephant in ten, which had been taken after having attained his full growth, much disposed to have connexion with a female. This is a most convincing proof, that those males which are taken early in life, and have been domesticated for many years, more readily procreate their species than elephants taken at a later period. In their wild state, however, they shew no reluctance; for, besides all the males that are entrapped, from their desire to have connexion with the trained females which, though not in heat, are carried out to seduce them, several instances have occurred of wild elephants covering, immediately after being taken, in the *keddab*.

On the third of April, 1795, a very fine male elephant covered a female twice, in the midst of the herd, and before all the hunters. On the 4th I saw him attempting to cover a third time, when he was suddenly disturbed, by the noise the hunters made to drive away some of the herd which had come too near the palisade. In consequence of this interruption, he threw down first one and then another small elephant, and gored them terribly with his tusks, though they came between him and the female only for their protection: he had, before this, killed

killed four, and wounded many others. When the poor animals were thrown down, conscious of their impending fate, they roared most piteously; but, notwithstanding their prostrate situation, and submissive cries, he unfeelingly and deliberately drove his tusks through, and transfixed them to the ground; yet none of the large elephants, not even the dams of the sufferers, came near to relieve them, or seemed to be sensibly affected. This savage animal had been then confined four days in the inclosure, along with the herd, upon a very scanty allowance of food, and could have but very little hope of escaping; yet here his passions were stronger than his fears. It was on account of this savage disposition, the hunters had asked permission to shoot him, before I had either seen him or the herd, and thence judged he was a *goondab**, that had lately joined. Having never before known any elephant killed wilfully, in the *keddab*, by the larger males, and having no idea that he would commit such terrible havock, I unluckily refused to grant their request, being desirous to save so stately an elephant. When the palisade was finished, I got him tied, and led out; but, not brooking restraint, he languished about forty days, after he was secured, and then died.

In the course of this narrative, I have, in general, related only such particulars concerning the elephant as came within my own knowledge, and which were either not known, or not published. To enter into a particular history of the elephant was not my intention; and, although the procreation of tame elephants has been proved, yet the expence incurred by breeding them, may deter others from making attempts of this kind. But it opens a field of curious inquiry to the naturalists; and, now that the facility with which it may be done is ascertained, it suggests itself as a mode by which the breed of elephants may be improved, in size, strength, and activity. In this way, any expence which might be incurred, would more than repay itself, in the future benefits to be derived from a superior breed of elephants.

* From this instance, as well as many concurring circumstances, I am convinced that these *goondabs* generally leave the herd of their own accord, and join it when they think proper, or are induced to it from a female being in heat; yet it has been supposed, that they are driven from the herd, at an early period of life, by their seniors. This appears improbable, as it is not often that very large males are taken with a herd of elephants; for, depending on their own strength, they stray singly, or in small parties, from the woods into the plains, and even to the villages; and it is in these excursions they are taken, by means of the trained females. As these *goondabs* are much larger, and stronger, than the males generally taken with the herd, it is not probable they would submit to be driven from it, unless at an early period. I have seldom seen, in a herd of elephants, a male so large as we commonly meet with among two or three *goondabs*; but, if these last were driven from the herd when young, the very reverse would be observed.

IV.

Extract of a Letter on the Art of Bleaching, and the Effect of Friction in Water-courses for Mills. By a Correspondent.

AFTER giving a short statement of Mr. Higgins's process for making and applying the sulphuret of lime to the bleaching process, of which the entire account is given in the following article, from the work itself, sent by the same friend,* the writer proceeds as follows:

"The advanced price of alkaline ashes makes an economical use of them indispensable. The above process renders them unnecessary altogether. How it may succeed in the large way is yet to be tried. There is another way to obtain the advantage of reducing the expence of bleaching with alkaline ashes; that is, to apply to the liquor, when saturated with the colouring matter of the cloth, some cheap (if any such there is) substance having a greater affinity for the colouring matter than the alkali. The writer of the article Bleaching, in the supplement to the Encyclopædia Britannica, No. 5, says, 'that if unto the alkaline solution of the colouring matter of lime or lime water be poured, a copious precipitate falls down, which consists of the colouring matter and lime combined; lime, therefore, has a greater affinity for the colouring matter than the alkali.'

"This is surely an error. I have not been able, by adding lime water to the alkaline solution of the colouring matter, to obtain any considerable precipitate. The brown colour of the precipitate was not more changed than it would be by being so much more diluted; any precipitate which there was, appeared rather to be occasioned by the carbonic acid decomposing the lime water.

"The writer of the article, if he has succeeded as he has stated, has made a valuable discovery.

"Kirwan, in his experiments on the alkaline substances used in bleaching, &c.* says lime dissolved very little of the colouring matter.

"Mr. Nicholson's thoughts on this subject will gratify a friend to his Journal.

"On Water Courses for Mills.

"Allow a water-wheel 8 feet in breadth; it is required to know what width the canal must be to bring on the water to the wheel with full effect; supposing the length of canal to be 100 yards, and as the friction of the water in the canal is in proportion to its length, what allowance in width must be given in any given length to compensate for the obstruction arising from that cause? The form most advantageous is taken to be that described by three sides of a hexagon.

"Londonderry.

"A."

* Irish Transactions for 1789.

It would give me much pleasure if I could state any thing on the subject of enquiry respecting the method of disengaging the colouring matter of piece-goods from its combination with alkali. But the affinities of the former must be nearly unknown to most chemists, because, in general, they can have few opportunities of subjecting it to experiment. There can be little doubt, however, of its total destructibility by heat with access of air. If, therefore, the saturated lyes were evaporated to dryness, and then incinerated, with or without the contact of vegetable fuel, I conclude, of course, that the alkali might be profitably recovered. I am aware that the skill and researches of my correspondent render it unnecessary for me to remind him, that there are some contrivances for doing this, with little expence of fuel, in the treatise of Pajot des Charmes, of which a translation was lately published.

With regard to the question concerning water-courses, it cannot be treated in general, without including the quantity of water delivered in a given time as one of the data. I do not know that practical men have paid any considerable attention to the loss which is sustained by the friction of water against its channel or bed; but shall take a future opportunity of resuming this subject.

W. N.

V.

*On Sulphuret of Lime, to be used as a Substitute for Potash in Bleaching with the Oxygenated Muriatic Acid. By MR. WILLIAM HIGGINS, M.R.I.A. and Professor of Chemistry and Mineralogy at the Repository of the Dublin Society.**

SINCE I had the honour of being appointed chemist to the linen board, which is now more than three years, I have allotted a considerable portion of my time and attention to the investigation of the principles of that science, applicable to the art in which I am thus more particularly interested. It appeared, that, until potash could be dispensed with, we must for ever remain in the power of foreign nations as to our staple commodity. Observing also, that all the late improvements in bleaching were exclusively confined to one object—that of imparting oxygen to the cloth, in a safe and expeditious manner, but that there had been no effort made to supersede the necessity of potash, by far the most expensive and uncertain article employed by the bleacher, and for which he is entirely dependent upon foreign markets; I directed my attention chiefly to discover a substitute for potash; which, provided it should be of Irish production, though it might be equally expensive, I conceived would be of the utmost national importance. Impressed with these ideas, I undertook a series of experiments with that view.

To enumerate the many disappointments and failures I experienced during my investigation would be endless, and an unnecessary intrusion upon my reader. Knowing, from

* From his "Essay on the Theory and Practice of Bleaching," just published at Dublin, and forwarded to me by the respectable writer of the preceding article.

an important observation of Mr. Kirwan, that saline hepars, or the combination of an alkali with sulphur, might, from its detergent properties, be advantageously employed in bleaching as a substitute for mere alkali, by an obvious analogy I was led to expect a similar effect from calcareous hepar, or, more properly speaking, sulphuret of lime, being a combination of lime and sulphur.

In these expectations I was not disappointed; but at that time (about three years since) I contented myself (rather through necessity; for large cities are very unfavourable to experiments of bleaching, by exposure to atmosphere) with pointing it out to some of the principal bleachers from the north then in town, earnestly recommending it to them to give it a fair trial with and without potash. Since that time, alkaline salts have become progressively dearer, and in consequence of a late proposal of substituting lime for potash, in condensing the oxymuriated gas, I was instigated to resume the subject, and make further and more varied trials. The result of which has been, that the use of the sulphuret of lime may be most advantageously combined with that of the oxymuriated lime, and that thus cloth may be perfectly whitened without the use of a particle of alkali. This, then, alone would seem to give it a decided preference over the methods at present in use, while at the same time it possesses peculiar advantages, and is exempt from the principal objections to which other *substitutes* are liable; for first, quicklime and sulphur, the materials of which the *calcareous hepar* consists, are both articles of trivial expence, especially as the latter enters but sparingly into the composition; 2dly, their combination is effected in the easiest and most expeditious manner possible, and perfectly level with the capacity of the meanest workman; 3dly, as the manner of its application is by steeping the cloth in it cold, the saving of fuel is a matter of great magnitude; and lastly, there is no danger to be apprehended in the use of it from the unskilfulness or negligence of the workmen, as it appears to be incapable of injuring the texture of the cloth.

The *sulphuret of lime* is prepared in the manner following: Sulphur or brimstone, in fine powder, four pounds; lime, well slacked and sifted, twenty pounds; water, sixteen gallons: these are all to be well mixed and boiled for about half an hour in an iron vessel, stirring them briskly from time to time. Soon after the agitation of boiling is over, the solution of the sulphuret of lime clears, and may be drawn off free from the insoluble matter, which is considerable, and which rests upon the bottom of the boiler*. The liquor in this state is pretty nearly of the colour of small-beer, but not quite so transparent.

Sixteen gallons of fresh water are afterwards to be poured upon the insoluble dregs in the boiler, in order to separate the whole of the sulphuret from them. When this clears (being previously well agitated), it is also to be drawn off, and mixed with the first liquor: to these again thirty-three gallons more of water may be added, which will reduce the liquor to a proper standard for steeping the cloth.

* Although *lime* is one of the constituent principles of the *sulphuret*, yet being so intimately united to the sulphur, it has no longer the property of lime; upon the same principle that *sulphuric acid* in sulphate of potash has not the property of that acid.

Here we have (an allowance being made for evaporation, and for the quantity retained in the dregs) sixty gallons of liquor from four pounds of brimstone.

Although sulphur itself is not in any sensible degree soluble in water, and lime but very sparingly so, water dissolving but about one seven hundredth part of its weight of lime, yet the sulphuret of lime is highly soluble*.

When the linen is freed from the weaver's dressing, it is to be steeped in the solution of sulphuret of lime (prepared as above) for about twelve or eighteen hours, then taken out and very well washed; when dry, it is to be steeped in the oxymuriate of lime for twelve or fourteen hours, and then washed and dried. This process is to be repeated six times; that is, six alternate immersions in each liquor, which I found sufficient to whiten the linen.

When I submitted the linen to six boilings in potash, and to six immersions in the oxygenated liquor, it was not better bleached than the above.

The three first boilings in potash, it is true, produced a somewhat better effect than as many steeps in the sulphuret; but towards the conclusion, that is, when the linen was bleached, the smallest difference was not observable as to colour. The linen bleached with the potash was thinner, or more impoverished, than that treated with sulphuret; and the latter stood the test of boiling with soap much better than the former, although it did acquire a slight yellowish tinge, which I should suppose a week's, or at most a fortnight's, grass, as they term it, would remove.

I contrasted the effects of hot and cold sulphuret in various temperatures, and although the difference appeared in favour of the hot liquor, yet it was so trifling as not to deserve consideration, or the expenditure of the smallest quantity of fuel.

When I steeped the linen in the sulphuret first, and afterwards boiled it in potash, and then immersed it once in the oxygenated liquor, a better effect was produced than from two previous boilings in potash, or from two steeps in the sulphuret; so that the two substances seem to co-operate with each other.

Indeed, from what I have seen, two successive steeps in fresh sulphuret previous to the immersion in the oxygenated liquor, seemed to afford very little better effect than a single one, which is not the case with respect to potash.

It was observable, that the cloth was invariably thicker, or more swelled, coming out of the sulphuret, than after being boiled in potash, and remained so when even washed and dried.

It appears to me that the sulphuret opens the fibres of the linen more speedily and better than the latter, by softening and swelling, rather than by dissolving the resinous or colouring matter. This accounts for the better effect of potash upon the linen when previously steeped in the sulphuret, than when used by itself.

* When the above proportion of lime and sulphur is boiled with only twelve gallons of water, the sulphuret partly crystallizes upon cooling, and when once crystallized, it is not easy of solution.

Probably those bleachers who do not at present find it convenient to use the oxygenated liquor, but continue to bleach by exposure to air, may derive some advantage from this, by using the sulphuret and potash conjointly or alternately.

Mr John Duffy of Ball's-bridge (who, from his knowledge of chemistry, is very well acquainted with the principles of bleaching), was kind enough to repeat the above experiments, and his report to me corresponded with my own observations.

It is almost impossible to ascertain to the full extent, more especially by small experiments in an laboratory, the many advantages any substance, not hitherto used in bleaching, will afford by varying the mode of application.

The experimenter does a great deal by discovering the efficacy, proving the practicability, and ascertaining the safest and most economical method of directly using it, and also the best proportion of it. Before he can arrive at one of these, many a round of changes are necessary; indeed a greater number than any man, who is not used to experiments, can be aware of. But I should hope that the bleacher need not hesitate to use it in the state in which I present it to him, more especially as he runs no risk of injuring the cloth with it. If he can make more of it hereafter, I shall feel happy upon the occasion; no discovery was ever brought to perfection at once.

How gradually, and yet how progressively, the steam-engine, from its first invention by the Marquis of Worcester, was brought to its present degree of perfection! Undoubtedly it was just so with respect to alkalies, the substances now used by the bleachers; it must have taken a considerable time, after their first application in bleaching, before they could be made the most of.

I will now conclude, by pointing out the advantage likely to accrue from the use of the sulphuret to the nation, and also the saving to the individual.

By the information I have had from the Custom-house, it appears that the average importation of potash and barilha the last twelve years amounts to about 5066 tons annually; about one half of this (2533 tons) is barilha. The average price of barilha, the last three years, has been 40 l. a ton; so that the value of the quantity imported is 101,323 l.; of this only half, or thereabout, I understand, is used in bleaching, the remainder being converted into soap.

Most of the pot and pearl-ash is consumed by the bleachers, and the average price of it the last three years has been 65 l. a ton, consequently the value of 2,533 tons is 164,645 l.

Hence it seems that the quantity of foreign alkalies imported into the kingdom every year amounts to 265,968 l.; and that the quantity used in bleaching alone amounts to about 215,307 l. annually.

The average price of brimstone for the last three years is about 25 l. a ton, which is at the rate nearly of two pence farthing a pound; four pounds of brimstone, and twenty pounds of lime, as already observed, will produce sixty gallons of liquor. In this country, twenty pounds of lime may be valued at about four pence, so that the bleacher may have the sixty gallons at the expence of 1s. 1d.

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By what I could learn from different bleachers, the common allowance of alkali for sixty gallons of water is six pounds of barilha, or four pounds of potash, at the very least, and most bleachers use more than this. The price of four pounds of potash, at the rate of 65 l. a ton, is about 2s. 4d.; which is 2d. more than double the price of the sulphuret: but as the brimstone must be ground, an allowance should be made for it, and being easy of pulverization, a farthing per pound is an ample consideration for the expence attending it.

The saving of fuel only now remains to be taken into consideration; and as this cannot be calculated with any degree of accuracy, I shall content myself by particularizing the facts. In the first place, but sixteen gallons of liquid are to be boiled in preparing sixty gallons of the sulphuret, while the whole sixty gallons must be boiled when the alkali is used; hence it might appear that two thirds of the fuel are saved in the quantity of liquor, but it is not quite so much; suppose we estimate it at one half, which is rather under-rating it. Let us add to this the time necessary to boil the different liquors; the sulphuret requires but about half an hour, and the alkaline lixivium at the very least seven hours, to boil the linen in it, which is in the proportion of one to fourteen.

The saving altogether to the bleacher from this statement is obviously very considerable; and as the Wicklow copper mines are sufficient to supply the whole kingdom, or indeed two such kingdoms, with abundance of sulphur, let the consumption be ever so great, the entire of the alkali, or 215,307 l. must be annually saved to the nation.

But suppose two thirds only of the quantity of alkali generally consumed in bleaching were dispensed with by the use of the sulphuret (which is a supposition not warranted by any experiment), still the saving to the nation, and to the individual, must evidently be great indeed.

VI.

*On the Decomposition of the Acid of Borax, or Sedative Salt. By LAURENCE DE CRELL, M.D. F.R.S. London and Edinburgh, and M.R.I.A. Translated from the German.**

THE salt called Borax, so useful in various manufactures and arts, and hitherto imported only from Thibet and Persia, or in small quantities from Tranquebar†, has ever excited the attention of natural philosophers. This attention was principally directed to the acid (called the sedative salt) contained in it; its other component part, the alkaline salt (soda or natron), being better known, and found in many other natural productions, either alone in conjunction with other acids. The acid above mentioned has hitherto been discovered only by Hofer in the Lagone of Castelnuevo; by Martinovich in the petroleum of Galicia‡,

* Phil. Transf. 1799.

† Demachy in Laborant in Großen, part II. p. 89.

‡ Crell's Annales. 1791. t. I. p. 162.

mixed with alkaline earth; and by Mr Westrumb near Luneburg. The scarcity of this acid, and its being found only in the substances and situations above mentioned, occasioned a supposition in the minds of those who minutely observe and examine the course of nature, that it is not a simple substance, but is formed afresh from a variety of other substances previously decomposed by a singular coincidence of operative causes, and consequently that it belongs to compounds.

Numerous have been the experiments made by chemists who supposed they had formed this salt by composition. Some described experiments, which they declared to have succeeded with them, though they always failed when attempted by others *; from which Leonhardi concludes, that nothing more can be expected from any similar attempts to produce sedative salt †.

I was surprized that these chemists had never (so far as I knew) examined the subject by the way of analysis, and endeavoured to decompose the sedative salt already formed by nature. Indeed no great hopes of success could be entertained; as daily experience shows, that though this salt be kept fluid in the hottest fire for many hours together, till it becomes a vitrified substance, yet when it is afterwards dissolved in distilled water, the solution is complete without any residuum, and it then shoots into crystals of precisely the same salt as before. Notwithstanding all this, when I reflected that borax is generated only in certain climates of the east, and that its acid is found only in particular substances and situations, as has been already mentioned, I could not but suppose the latter to be the produce of a new formation. This being premised, I considered maturely in what manner the decomposition of this new and extraordinary compound might be attempted. Admitting the composition to be formed by the coalition of a number of different substances, it seemed not improbable but that an acid, penetrating into and dissolving the whole mass, would rather associate with some than with others of its various component parts, and thus produce a separation or change of the latter. Besides, as the sedative salt, strong as its operation is (in a high degree of heat) upon almost all neutral salts, has but a faint taste of acid, it might be supposed that its acid is contained within some unknown species of earth intimately combined, or within some sort of inflammable matters; or, according to a phrase used in the new system, there might be a deficiency of oxygen; that therefore some more powerful acid would probably separate and dissolve the earthy particles, destroy or change the inflammable matter, or impart the oxygen it might be supposed to want.

My choice among the different acids was fixed upon that particular one, which, though not always quick in its operation, never fails to penetrate deep into all soluble substances, has a strong affinity to all inflammable bodies, and possesses abundance of oxygen; I mean the oxygenated muriatic acid prepared with manganese. In the application of this menstruum, I resolved to follow the practice established by the constant experience of both ancient and

* See Fuchs Geschichte des Boraxis.

† Macquer's Dictionary translated by Leonhardi, second edition, vol. V. p: 588.

modern chemists; which has taught us, that difficult decompositions of parts closely united, are more easily effected by a gentle long-continued digestive heat, and repeated distillation of the same menstruum, than by a heat which is more violent, and operates more quickly.

I first made some preliminary experiments, in order to judge what probability there might be of success.

Experiment I. I poured an ounce and a half of the abovementioned acid upon two drams of sedative salt in a retort, to which I adapted a proper receiver, and then placed the mixture in a gentle digestive heat of from 140° to 200° of Fahrenheit. The fluid was distilled over very slowly, and the salt was dry on the third day. The salt in the retort seemed unchanged; nor had the marine acid lost any thing of its usual smell.

Exper. II. I poured the distilled fluid out of the receiver upon the same salt, and exposed them to the same degree of heat as before. The salt again became dry on the third day, but there was yet no appearance of any change.

Exper. III. I repeated the same process a third time. I now perceived, during the distillatory digestion, several bright yellow spots upon the salt as it ascended the sides of the retort, resembling well-formed ammoniacal flowers of iron; more of which I discovered after the entire exhalation of the fluid.

Exper. IV. The above change induced me to repeat the distillation; and I then perceived, not only as many, but a much greater number of bright yellow spots, some of which were even much darker in colour, and approaching to brown. A change had now evidently taken place, which change increased upon every repetition of the process; I therefore judged I might follow this indication with confidence. But with a view to use the greatest accuracy and precaution in my proceedings and observations, I resolved to begin my work over again.

First, I procured some ounces of sedative salt, which had been obtained from borax by means of vitriolic acid, and then prepared two quarts of the abovementioned oxygenated muriatic acid, by distilling three parts of muriatic acid with one part of the purest manganese in the usual manner; this I preserved in a cool dark place. Thus the substances used in the following experiments were always of the same nature.

Exper. V. I poured three ounces of the oxygenated muriatic acid upon half an ounce of the sedative salt in a white glass-tubulated retort. I used such a retort that (in frequently pouring back the distilled fluid) I might not have to lute afresh the several vessels after every distillatory digestion. For the same reason also I chose a tubulated receiver, the tube of which gradually terminated in a point in shape of a funnel. This tube passed into a phial placed in such a manner, that all the fluid passing into the receiver dropped immediately into the phial, the joinings of which were closed with bladder. To close the tube of the retort I did not think it right to use a waxed cork (though it closes very tight), because it might be corroded, and also because the vapours dropping from the cork might carry some fat and oily matter back into the retort. For the same reason I would not use any greasy lute; but closed the joints of the glass stopper (which fitted remarkably close) with a ring of fine sealing-wax, closely pressed upon it, but which could be easily disengaged after
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my work was done, while the retort was still warm: and as I was even afraid of an oily lute about the joints of the receiver, I closed them up with a ring of very fine white clay, which I fitted to them as exactly as possible by pressure, letting it stand several days to dry, and then carefully filling up all the cracks. Having made this previous arrangement, and put the abovementioned ingredients together, I suffered them to remain cold for twenty-four hours; at the end of which the salt was not entirely dissolved, but upon the application of heat the whole became a clear fluid *. The degree of heat in the sand was from 180 to 240, by which the fluid evaporated very slowly. During this operation there ascended, or rather crept up the sides of the retort, a considerable quantity of salt in very loose flowers, rising pretty high above the fluid, increasing by degrees, and chiefly occupying that half of the retort which received a greater degree of heat than the other, but never the opposite or colder half. In four days, the fire being extinguished, towards the evening of the last the fluid had evaporated so as to leave the salt apparently dry. After cooling for some time the bladder upon the phial was moistened by water, and the vessels were separated; the sealing-wax also having been removed, and the stopper taken out, the distilled fluid was poured back through a glass funnel upon the salt, without disturbing the lute.

Exper. VI. As soon as the fluid was added, the salt at the bottom began by degrees to dissolve; that on the sides of the retort did the same after it was heated, but soon began to form again: the solution appeared of a yellowish hue. In general, however, the whole experiment took the same course as in *exper. V.* and the smell both of the salt and the fluid seemed to be unchanged. The only difference was, that the former did not appear like salt, the crystallization on the sides excepted, and in single detached crystals, but something like a white uniform spongy, and, as it were, earthy mass. The fluid was now again taken from the phial, as in *exper. V.* and poured back upon the salt.

Exper. VII. VIII. and IX. During the third distillation, bright yellow spots began to appear upon the white flowers; and after the salt at the bottom had become dry, similar spots appeared upon it, particularly upon the lower surface. The fluid was again for the fourth time poured upon the salt, and distilled, when the yellow spots and flowers increased in number. This was also the case in the fifth distillation.

Exper. X. The fluid obtained by the last experiment, which had changed a little in smell and had acquired a particular scent, almost as if some sebatic acid had combined with the muriatic, was poured upon the salt as before. The number of yellow spots, which had also become of a darker hue, was considerably increased. The salt had now been exposed ever since the fifth *exper.* for thirty-two days, to the digestive distillation, and the intermediate time between each distillation had been longer or shorter in proportion to the degree of heat

* This appeared to me so striking, that I endeavoured to obtain a confirmation of it. I made a similar mixture in the same proportions, which was not dissolved so long as it remained cold, but was dissolved by heat. When the solution cooled, a small part of the salt (and a larger as the cold increased) precipitated, which was dissolved again by a fresh application of heat. But with the degree of heat I employed, no more than one part of salt would dissolve in six parts of acid.

and to the time of kindling and extinguishing the fire. I now found that business of importance would prevent me from continuing my labours for some months; I poured two other ounces of the muriatic acid upon the salt, besides the fluid so often drawn off by distillation, and left the mixture at rest.

Exper. XI. XII. XIII. XIV. When my business was finished, I again undertook the distilling of the mixture, which had been so long digesting in the cold, for the seventh time, and obtained the same results as in *Exper. X*. Nor was there much difference observed in the XII. XIII. and XIV. experiments.

Exper. XV. I now poured the fluid obtained by the XIV. experiment upon the salt (which had acquired here and there yellow spots brighter in hue), and then proceeded, as before, till the salt became dry; upon which, when the retort was cool, I poured one ounce and three drams of the muriatic acid in addition, and allowed the mixture to digest gently for some days.

Exper. XVI. In this twelfth distillation there appeared a large quantity of flocculent sublimate, looking almost like branches hanging down, and in many places of a yellow colour; it extended even into the neck of the retort, and almost covered the interior aperture of the tube.

Exper. XVII. The thirteenth distillation produced the same phenomena. Upon the lowermost surface of the mass of salt, many light-brown spots appeared as soon as the fluid was so much evaporated that no more of it could be seen upon the salt.

From all these circumstances I now believed the mass of salt, by a digestion of twenty-two days, and seven distillations, from experiment XI. to XVII. (that is, by a digestion of fifty-four days, and thirteen distillations, in the whole), to be so far decomposed as to admit of a separation of some of its constituent parts. I therefore supposed I might leave off applying only a digestive warmth, and proceed to a greater degree of heat.

Exper. XVIII. Having poured out the fluid obtained by experiment XVII. and replaced the phial, I increased the degree of heat. By this the retort became quite obscured, first by fumes, and afterwards by a quantity of white sublimate, attaching itself to all its sides, which however had not the appearance of common sedative salt. As I increased the heat, the sublimate grew darker in colour, afterwards became black and frothy, and at length ran down the sides of the retort, being almost wholly blackened by it.

Exper. XIX. While the retort was still warm, I poured into it the fluid obtained by experiment XVII. having first watered it a little; when almost in the same instant a very agreeable phenomenon took place: crystals perfectly white shot forth suddenly, and all at once, from every side of the black mass, covering the sides of the retort. The distillation being continued, these crystals were at length dissolved and entirely removed. The supernatant fluid was as usual almost colourless. When the mass of salt appeared dry, the fire was increased, as in experiment XVIII. and the same appearances, as above related, took place; first, the sublimate appeared white; then black frothy, and flowing down the sides.

Exper. XX. I proceeded as in experiment XIX. to pour back the distilled fluid. Instantly a

number of the whitest crystals shot forth from the black ground, forming small groups; but the retort was cracked.

Exper. XXI. I therefore took all the vessels asunder, and stook the retort well, till whatever hung upon its sides was dissolved; then distilled the fluid in another retort, till the mass of salt appeared quite dry. I now put the retort into a crucible, surrounded it with sand, fitted another receiver to it, and placed the crucible in an open fire. First some sublimate was produced towards the neck of the retort, (but which vanished as the heat increased), and then a small portion of fluid (hardly more than a dram, or a dram and a half), which appeared to smell a little of the sebatic acid. At the bottom of the retort was a blackish mass *a*, and likewise some sublimate *b*, which by its varied appearance seemed to be of a two-fold nature.

Exper. XXII. The residuum taken out of the broken retort had a spongy appearance, and swam upon water. It had a blackish colour, and weighed three drams and ten grains. Being exposed to the air, the blackish colour became lighter, and inclining to grey. When digested in sixteen parts of distilled water in the usual temperature for two days and a half, it did not at all sink to the bottom; and after being digested with heat for twenty hours, it was not entirely dissolved: that part which sunk was of a blackish-brown. More water was then added, and it was made to boil for two hours; it was afterwards placed upon a paper filter (the weight of which was previously ascertained), andedulcorated with boiling distilled water, till at last a proportion of twenty-six parts of water to the substance had been used. After all the fluid *a* had passed through, and the filter with the residuum had been dried in a heat of 212° for an hour and a half, the residuum *β* weighed, exclusive of the filter, nineteen grains.

Exper. XXIII. The fluid *a* obtained by exper. XXI. was suffered to evaporate gradually, and yielded three drams and ten grains of a white transparent salt.

Exper. XXIV. This salt (obtained by exper. XXIII.) was put into a small retort, and exposed in a crucible filled with sand to an open fire. It became of a blackish brown colour, yielded some sublimate *a* (about five grains), a small portion of fluid *b*, and a blackish brown residuum *c*, which grew lighter in colour on being exposed to the air.

Exper. XXV. The fluid *b* (of exper. XXIV.) smelt like marine acid, and precipitated nitrate of lead.

Exper. XXVI. The residuum *c* (of exper. XXIV.), by the addition of some water, became whiter, and was dissolved; more water having been added, it was digested with heat, by which the matter was dissolved. The solution being afterwards filtered, I obtained two drams and four grains of white salt: the residuum upon the filter weighed four grains.

Exper. XXVII. This salt (exper. XXVI.) I again exposed to the fire, when it yielded from twenty to thirty drops of acid liquor, four grains of sublimate, and a residuum, which being dissolved, yielded one dram and thirty-three grains of salt, and left two grains and a half *c* upon the filter.

The same salt (obtained by exper. XXVI.) being distilled, became of a brownish-grey colour, and besides a few drops of fluid yielded not quite two grains of sublimate. On heating

ing the residuum with water, it yielded sixty-eight grains of salt, and there were not quite two grains left upon the filter.

Exper. XXVIII. On treating these sixty-eight grains of salt in the same manner, they yielded a few drops of fluid, and two grains of sublimate; after filtration, there remained forty-eight grains of salt, and a residuum of hardly one grain and a half.

Exper. XXIX. The same salt, treated in the same manner, yielded a few drops and a little sublimate; and after filtration thirty-five grains of salt and a residuum of hardly one grain.

Exper. XXX. On treating these thirty-five grains of salt in the same way, they yielded, besides a very small quantity of fluid and of sublimate, twenty-four grains of salt, and about three quarters of a grain of residuum.

As I now discovered that the quantity of salt was continually decreasing, and some coal separating from it, I thought it superfluous to endeavour to decompose the above twenty-four grains any farther.

Exper. XXXI. The residuum β of *exper. XXII.* was light, blackish, and like coal. I now poured common concentrated muriatic acid upon three grains of it, and digested the mixture for forty-two hours in a considerable degree of heat, but no dissolution was apparent. I then added smoking nitrous acid, and digested it for twenty-four hours, till it boiled without any apparent dissolution. I added some sugar (about two grains), but without effect, except that its colour grew yellowish. I now boiled the fluid till it all evaporated in reddish-yellow vapours; there remained a very black, thick, glutinous mass, smelling like burnt sugar. Having added three ounces of water, the greatest part of the blackish matter rose to the surface, and the water appeared only a little tinged: the fluid part, indeed, became brown by boiling, but after rest and subsidence, it again grew clear. I filtered it *a*; then poured two ounces more distilled water upon the residuum, and after digesting, boiling, and filtering, added the filtered fluid *b* to the former *a*. After this treatment there remained two grains of residuum *c*.

Exper. XXXII. Having caused the fluid *a b* of *exper. XXXI.* to evaporate, it yielded a salt greyish yellow mass, which very quickly attracted the moisture of the air. Being again dissolved in water, and saturated with potash, a considerable quantity of whitish earth was precipitated very much resembling talc.

Exper. XXXIII. The residuum *c* of *exper. XXXI.* which, besides its insolubility and lightness, had much of the external appearance of coal, was now thrown upon melted nitre, and it deflagrated. I placed a second crucible with melted nitre close to it, and after having at the same moment thrown into one the abovementioned residuum, and into the other a quantity of common charcoal pulverised, I could not observe the smallest difference in effect. Very little difference was also apparent as to the residuum β of *exper. XXII.* *c* of *exper. XXIV.* and that of the following experiments.

(To be continued.)

VII.

Reflections on the Qualities of Pottery, with the Result of some Analysis of Earths, and of Common Pottery. By CIT. VAUQUELIN.*

THE quality of pottery may be influenced by four things ; 1. The nature or composition of the material ; 2d, The preparation ; 3d, The dimensions of the vessels ; and, 4th, The heat to which it is subjected in baking.

By the composition of the material, the author understands the nature and proportions of its elementary parts ; and these elements in most potteries, whether valuable or common, are siliceous, alumine, lime, and sometimes a small quantity of the oxide of iron. Hence it is evident that good pottery differs from bad, less in the diversity of its elements, than in their proportions.

Siliceous, or quartz, constantly forms at least two thirds of most potteries ; alumine from a fifth to one third ; lime from five hundredth parts to twenty hundredth parts ; and iron from the minutest quantity, or none, to twelve or fifteen per cent.

Siliceous gives hardness, infusibility, and unchangeableness ; alumine communicates tenacity, and ductility to the paste, so that it may be kneaded, moulded, and turned at pleasure. It likewise undergoes a commencement of fusion, by the heat which connects its parts with those of the siliceous ; but its quantity must not be too abundant, because it would render the pottery too infusible, and disposed to break by heat.

Experiment has not yet shown that lime is a necessary ingredient in pottery ; the traces which are constantly found of its existence in this manufacture arise from its forming part of the ingredients from which it has not been washed or properly separated. When this earth does not exceed five or six per cent, it appears at least to do no harm to the quality of the pottery ; but when more abundant, it communicates too great a degree of fusibility.

The oxide of iron possesses the inconvenience of affording a red or brown colour according to the degree of heat, and has besides the property of being very fusible ; more so even than lime.

Among the various kinds of pottery, some of them being applied to contain very penetrating substances, such as salts, metallic oxides, glasses, &c. in a state of fusion, require to be formed of a fine paste, which is only to be obtained by mechanical division of the earths ; others intended for the fusion of metals and other matters which act very little upon them, and being required to support sudden and extreme variations of temperature without breaking, require to be formed of a mixture of cement, or burned clay, with the raw clay. By this means a kind of pottery is obtained, the coarse texture of which, in some measure, resembles the breccias or pudding-stones, which easily undergoes those rapid changes of temperature.

The baking of pottery is likewise an object of extreme importance. It is necessary that

* Communicated to the Philomathic Society at Paris, and inserted in their bulletin, No. 26, in the year vii. the

the heat should be such as to expel the moisture, and agglutinate the parts which enter into the composition of the paste; but incapable of effecting the fusion, which if too far advanced, will render the ware of so homogeneous a texture, as to become brittle. The same effect happens with regard to the fine pottery, because the extreme division of the earth puts them nearly in the same state as if the materials had been fused. Hence it is that porcelains which have been strongly baked are more or less brittle, and cannot well be subjected to changes of temperature: and hence also it is that coarse porcelains, into which a certain quantity of baked clay is mixed, retorts, crucibles, porcelain tubes, and common coarse pottery, are much less brittle than plates or dishes formed of the same materials in a more divided state.

The general and relative dimensions of the different parts of pottery are also of great consequence to the manner in which they support the fire. In some cases the glaze, particularly when too thick, and of a very different nature from the body of the pottery, likewise occasions it to break.

It is therefore essential in all cases in the fabrication of pottery, 1st, To observe the best proportions of the ingredients; 2d, To divide the materials more or less finely, according to the use to which the ware is to be applied, and to give all the parts as nearly as possible the same dimensions; 3d, To bake with as strong a heat as can be given without fusion; and 4th, To apply a thin coat of glaze, the fusibility of which ought to be as nearly as possible approaching to that of the body, in order that the combination may be more intimate.

From a conviction that the goodness of pottery principally depends on the due proportions of earths which enter into their composition, Citizen Vauquelin has thought it might prove interesting to those who are employed in this important manufacture, to be acquainted with the analysis of the different natural clays employed for this purpose, and of the pottery made from some of them, in order that when any new clay is discovered, it may be known by simple analysis which kind of pottery it may the nearest resemble.

	Hessian crucibles.		Clay of Dreux.		Capsules of porcelain.		Pyrometers of Wedgwood.
Silex,	69	-	43,5	-	61	-	64,2
Alumine,	21,5	-	33,2	-	28	-	25
Charcoal,	1	-	3,5	-	6	-	6
Oxide of iron,	8	-	1	-	0,5	-	0,2
Water,	-	-	18	-	-	-	6,2

Crude kaolin 104 parts contain,—silex 74,—alumine 16,5,—lime 2,—and water 7.—One hundred parts of this earth afforded eight of alum, after treatment with sulphuric acid.

Washed kaolin 100 parts afforded,—silex 55,—alumine 27,—lime 2,—iron 0,5,—water 14.—This kaolin, treated with the sulphuric acid, afforded between 45 and 50 per cent of alum.

Petuntze afforded silex 74,—alumine 14,5,—lime 5,5;—loss 6.—One hundred parts of this substance treated with the sulphuric acid, afforded 7 or 8 parts of alum. But this quantity does not account for the loss which was sustained.

Porcelain retorts, silex 64,—alumine 28,8,—lime 4,55,—iron 0,50;—loss 2,77.—When treated by the sulphuric acid, this porcelain did not afford alum.

Description

VIII.

*Description of a Thermometer, which marks the greatest Degree of Heat and Cold from one Time of Observation to another, and may also register its own Height at every Instant. By ALEXANDER KEITH, Esq. F.R.S. and F.A.S. Edinburgh. **

THERMOMETERS have hitherto been defective for meteorological purposes, in so far as they only point out the degree of heat at the moment of inspecting them, but do not show what the difference of temperature has been from the time of one observation to that of another. Nor has any instrument been yet constructed, so far as I have been able to learn, which will record the intermediate degrees of heat.

The ingenious Robert Hooke, in the end of the last century, mentions his intention of making a thermometer for the same purpose; but it does not appear that it ever was executed; neither does he explain how it was to have been done.

The thermometer invented by Mr. James Six, as described in the 72d volume of the *Philosophical Transactions* of the royal society of London, is made to show its greatest rise or fall from one period of observation to another. This is done by means of two small pieces of black glass which float on two different surfaces of mercury, within two glass tubes hermetically sealed. These floats, when raised to their greatest height, adhere to the side of the tube by means of a spring of glass, and become stationary, although the mercury falls.

After the observer has taken a note of the temperature, he, by a magnet held in his hand, draws down the float to the surface of the mercury, in consequence of a small bit of steel wire inclosed in the float, and the instrument is prepared for another observation. This is an ingenious invention, but requires too delicate workmanship to be fit for common use; besides it cannot be made to record the degrees of heat at intermediate periods. The thermometer lately invented by Dr. Rutherford of Balilish, and described in the third volume of the *Transactions* of this society, is also an ingenious contrivance; but has the same defect of marking only the extreme points to which the liquor has risen or fallen in two separate glass tubes.

Several years ago it occurred to me, that an air thermometer might be used for the purposes required, provided the weight of the atmosphere could be excluded, or a counter balance formed to it; and as the whole instrument could be made to rise or fall by the temperature of the atmosphere alone, it might be adapted to a piece of clock-work, which would record the degrees of heat at every instant through the year; and accordingly I read to this society a description of the instrument. But having formed another instrument of a more simple construction to answer the same purpose, I beg leave to give a description of it.

A B is a tube about fourteen inches long (pl. XII.) and three fourths of an inch caliber, of thin glass, sealed or closed at top. To the bottom, which is bent upwards, there is joined a glass tube seven inches long, and four tenths of an inch caliber, open at top. The tube

* Edin. Transf. vol. IV.

A B is filled with the strongest spirit of wine or alcohol, and from B to E is filled with mercury.

It will be evident from inspection, that if the spirit of wine is expanded by heat, the mercury in the smaller tube will rise; and if the spirit of wine is contracted by cold, the mercury will fall; and although they are both subjected to the pressure of the atmosphere, yet as liquids are incompressible by weight in any perceptible degree, neither the spirit of wine nor mercury will be altered in bulk by the different weights of the atmosphere.

F D is a scale of brass or ivory about six and a half inches long, divided in the usual way.

E is a small conical piece of ivory or glass of a proper weight, made to float on the surface of the mercury in the smaller tube; to which float is joined a wire reaching to H having a knee bent at a right angle, which raises one index, and depresses another index, according as the mercury rises or falls, which wire shall be termed the *float wire*.

I I is a glass tube seven inches and a half long, closed at top, and open at bottom, so wide as to slide easily over the scale; and by means of a brass rim cemented to it is made to fit exactly to the circular base of the scale, so that when this tube is put on, it covers the whole scale, and hinders and defends them from wind or rain. This cover need not be taken off, except when the instrument is to be prepared for an observation.

The operation of the float and indexes will be better understood from fig. 2, which represents them of the full size.

F G is the scale fixed to a circular piece of wood or brass through which the top of the small tube is made to pass.

From G to K is a piece of the smallest harpsichord wire, or rather of the smallest gold wire, stretched along the scale, fixed at the ends by two brass pins.

L, L, are two indexes formed of thin black oiled silk, pierced by the small wire in such a manner as to slide upwards and downwards with a very small force, not more than two grains.

H, the knee of the float wire, before described, is made to encompass the small wire between the two indexes, so that when the float rises, the upper index is moved upwards, and when it descends, it leaves the upper index stationary, and pushes down the lower index, which is also left stationary when the float rises.

When the instrument is to be prepared for an observation, the one index is to be pulled down, and the other raised, by means of a bit of wire bent for the purpose, until both indexes touch the knee of the float wire; and when it is again observed, the upper index will point out the greatest degree of heat, and the lower the greatest degree of cold.

If this thermometer is to be adapted to a piece of clock-work in order to record the degrees of heat at each hour and minute of time, it ought to be made of larger dimensions. The large tube may be forty inches long, and not increased in diameter, but the small tube ought to be enlarged in diameter, and not in length. By enlarging the tube which contains the spirit of wine in length only, it will be affected by heat and cold in as short a time as that before described.

It is unnecessary at present to explain the clock-work. It is sufficient to say, that a hollow cylinder,

cylinder, of any light substance, seven inches long, and five inches diameter, is made to revolve upon a vertical axis once in thirty-one days, or a month; a piece of smooth or vellum paper is put round this cylinder, pasted only at the joining, but so as to make it adhere close to the cylinder; on this paper are drawn thirty-one equal perpendicular divisions, numbered at the top 1, 2, 3, &c. to correspond to the thirty-one days of the month, each of which is subdivided into six parts to answer to four hours. The length of this cylinder is divided by lines surrounding it, or zones in such number as correspond to the scale of Fahrenheit's thermometer, namely, from 0 to 100 degrees. These divisions ought to be engraved on copper-plate, and a great number of impressions thrown off on smooth or vellum paper, in order that one may be ready to put on each month.

Fig. 3, M N represents the cylinder covered with one of these impressions. P P is the scale fixed to the frame on which the cylinder turns. This scale is divided into 100 of Fahrenheit's degrees, exactly corresponding to the divisions of the cylinder.

Q is a piece of black-lead pencil joined to the end of the float-wire in the place of the knee before mentioned. This pencil is made to press lightly on the cylinder by means of the small weight R. And as the pencil rises or falls by heat and cold, it will mark the degrees on the scale of the cylinder; and the cylinder being constantly revolving, the division for each day and parts of a day will successively be marked by the pencil, which will leave a trace describing an undulated line, distinctly delineating the temperature of each day through the month. These papers, when taken off and bound together, will make a complete register of the temperature for the year; or, if they are pasted to one another, they will form a thermometrical chart, by which the variations of heat and cold during the year may all be seen and compared by one glance of the eye.

By inspecting fig. 3, the effect of the instrument may be seen. It appears that the papers had been put on the cylinder the first day of the month at mid-day, when the thermometer stood at 45° ; that it fell gradually till midnight to 25° ; thereafter it rose till the 2 at 1 P. M. when it stood at 42° ; then it descended at midnight to 35° ; then on the fourth at mid-day it was to 50° ; and at noon the tenth of the month it stands at 40° .

If three inches be added to the length of the cylinder, it may be made to delineate the variations of the barometer as well as the thermometer, and thereby to form a complete chart or view of the progress of both of them. And if instruments of this kind were kept in different parts of the country, and their charts compared together, it would afford much information with regard to meteorology.

IX.

On the Effects of apparent Attraction, or Repulsion, between Bodies floating upon or immersed in Fluids. By M. MONGE.*

THE motions of apparent attraction or repulsion which take place between floating bodies on the surface of water or other fluids, are sufficiently remarkable to excite the curiosity of the most common observers. Men of considerable eminence in the sciences have ascribed them to direct forces acting between the bodies themselves, and have overlooked the agency of the fluid, which, indeed, performs the whole effect; while others have pointed out the true cause. Our author is among the former, and I have no doubt but his clear and perspicuous explanation will be acceptable to the reader.

If two light bodies, to which water is disposed to adhere, be suffered to float on the surface of that fluid, and left to themselves at the distance of several inches from each other, they will either remain at rest or obey the impulse of the wind or other accidental forces; but if they be placed at a small distance from each other, such, for example, as half an inch or less, they will rush together with an accelerated motion. A very perceptible force will be required to separate them, and whenever they are left to themselves they will rush together as before. Or otherwise the experiment may be made with a vessel containing water, which, by its adhesion to the sides, forms a kind of eminence or ring all round: if in this vessel a globe of cork be placed, this floating body will remain at rest while at a distance from the sides; but when brought within a certain small distance of the side, it will rush to it, and adhere in the same manner as one of the small bodies in the former experiment.

That these effects are not produced by the immediate operation of an attractive force in the bodies themselves, is proved by suspending them to long slender strings; in which situation they are not found to be affected by their vicinity to each other, or to the sides of the vessel, provided they do not touch the fluid. Or in the last experiment, if water be poured in while the floating body lies in contact with the side, it will be found that the apparent attraction will continue no longer than till the surface is perfectly level to the very border. And when, by the continuation of pouring, the fluid becomes convex towards the side, the floating body will be so far from adhering, that it will be repelled.

Again, if two bodies be suffered to float on the surface of water, one of which only is capable of being wetted; as for example, a ball of cork and another ball, of the same material, but blackened over by holding in a candle; these bodies will avoid each other, and if urged together they will fly asunder as soon as they are left at liberty. If the ball with the blackened surface be suffered to float alone, it will be repelled by the sides of the vessel, while the fluid remains elevated in contact with those sides; but, on the contrary, it will be attracted, when, by the act of filling, the border of the fluid is rendered convex.

* Abridged from the Memoirs of the Parisian Academy for 1789, p. 506.

If these balls be also suspended by strings, it will be found that the effects do not arise from any immediate attractive or repulsive force.

Lastly, If two bodies be suffered to float on the surface of the liquid, neither of which are capable of being wetted, an apparent attraction will take place between them; they will be attracted by the sides of the vessel when the fluid is convex, or repelled when its termination is concave.

These circumstances may be summed up by observing, 1. That two bodies susceptible of being wetted will appear to attract each other when floating on the surface of water. 2. When two bodies, not capable of being wetted, are either floated or immersed in a fluid, they will still have the same apparent attraction: and, 3. When the two bodies, either immersed or floating, are one of them capable of being wetted, and the other not, they will exert an action productive of the same effect as if a mutual repulsion were exerted between them.

As the prejudice the two drops of water mutually attract each other is generally received, the author gives a number of instances which tend to overthrow it. He observes, that two such drops are very far from exhibiting any attraction; insomuch that they may even in a dry state of the air be struck against each other, so as to alter their figure without uniting together. These separate drops may be shewn to advantage by causing the fluid to fall in drops through a small tube, from the height of a few lines, upon the surface of a mass of the same fluid. The tube must be slightly inclined, in order that the drop may run away in the horizontal direction. Spirit of wine is better adapted to this experiment than water. The spherical drops move along the surface with the utmost ease and rapidity; sometimes striking each other so as to change their figure without uniting, and it is a considerable time before they confound themselves with the general mass. M. M. thinks the evaporability of the spirit and the mutual attraction of its parts are the causes why this fluid exhibits the phenomenon in a more striking manner than water; in which last fluid it is, however, often seen when agitated by the dashing of oars, the fall of violent rain, and the like. It is very easy to distinguish these solid globules from the vesicles or hemispherical bubbles filled with air, because the former are much more moveable.

This appearance of drops rolling on the surface of a liquid is more durable the smaller the drops. When any heated liquor of a brown or dark colour, coffee for example, is exposed to the air, the stratum of heated air in contact with the surface becomes loaded with vapour, and rises to an elevation, where it cools and deposits part of the water in the form of a smoke or vapour, which condenses into drops that fall on the surface of the coloured liquid, and constitute a kind of scum. These globules being small retain their situation a long time, and are very distinguishable from bubbles by their great mobility, as the slightest breath of wind is sufficient to disperse and arrange them along the sides of the vessel.

Our author adopts the explanation of Mariotte for the second general fact or law, which is, that floating bodies not capable of being wetted will apparently attract each other. In fig 1. plate XI. the globes A and A' float upon the fluid, in which each makes a cavity of considerable

considerable extent, very easily observable, by attending to the reflection of a strait body, such as the frame of a window from the surface. In fig. 2, where the bodies are brought nearer, the cavities do not communicate, and, consequently, the reaction of the water all round each ball is the same, and they have no tendency to move in one direction more than another. But when they are brought still nearer, as in fig. 3, the effect of the repulsive powers of both depresses the surface between them, and the reaction of the fluid becomes less in proportion to its diminished height. The consequence is, that the bodies move towards each other from the external pressure or reaction which continues undiminished.

When two bodies of this kind are submerged, as in fig. 4, the fluid will be displaced from between them, if the air can have access, and the bodies will be pressed together by the force on the opposite parts. If the air cannot enter between them, the liquid will not be excluded unless the force of repulsion be equal to that of the pressure of the atmosphere; but the tendency to separation is productive of the same effect. Philosophers who operate with the pneumatic apparatus over mercury, have frequent occasions to notice this fact, when they plunge their hands in that dense fluid. For, upon attempting to separate the fingers in this situation, a resistance is felt, which does not arise from any difficulty of moving the fluid, but simply from this cause. It is a perception as if the fingers attracted each other, and is not felt except when they are near each other.

Fig. 5 represents two bodies, A' and A, in the situation of the third general fact or law. One of them being wetted has a mass of the fluid hanging round it, and the other forms a surrounding cavity by its repulsion. At the distance here expressed the bodies do not affect each other. In fig. 6 the general surface B is still unaltered: but at fig. 7 the curves interfere. The body A may be considered as if on a moveable inclined plane of water, down which it will slide, while the plane itself, with the body A', to which it is attached, will be moved in the contrary direction. The bodies will seem to repel each other.

As this effect also takes place beneath the fluid, that is to say, that the body which is disposed to be wetted, or which attracts the fluid, will exert a kind of repulsion upon any other body which repels the fluid, our author infers, that it may be considered as a pretty accurate representation of what happens in chemical precipitations. This, however, seems scarcely to apply to the case, as both principles in chemical solution appear to have an attraction to the solvent.

The first law or general fact, where apparent attraction is produced in two bodies capable of being wetted, employs a large portion of his research. If (fig. 8) a slip of glass be suspended at the extremity of a thread fixed to the point K, so that its lower end shall be plunged in the water, represented by MN, it is well known that the fluid will attach itself to the sides of the glass, so as to form two concave surfaces, EDC, HGF, higher than the common surface. As we are not acquainted with the laws of the force by which these effects are produced, and no observations have been made on the dimensions of the curve itself, little can be said

concerning its figure or dimensions. It is only known that it rises according to the nature and temperature of the fluid, and the material of which the slip itself is composed.

The two masses of water raised by the plate of glass, tend to give it motion downwards by adding to its weight, and sideways by the action of the water, which tends to move in the horizontal direction. M. Monge compares this to the action of a chain, having the figure of the curve, but variously loaded in its different parts. In the present figure the horizontal actions are precisely alike, and consequently equal; but it is easily to conceive that they may become unequal, and also to shew the same by experiment. FBA, fig. 9, represents a drop of water lying on a smooth plane: to this the plate CA, supposed to be moveable on the centre D, is applied. The effect of the action, which is now either wholly or chiefly on the side EF, tends to carry the extremity A towards B, and would cause the plate to place itself in the position represented in fig. 10, if gravity or other circumstances did not interfere.

But the attraction is more particularly seen by suspending two slips or plates of glass, as in fig. 11. These will retain the vertical position as long as they are more distant from each other than the limits of this capillary attraction; but when, as in fig. 12, the curves interfere, other consequences must follow. Suppose the water to acquire the figure AHB at its surface, and the quantity which the two plates suspended before will be greater by the portion CHD than it is at present. The power of suspension is not, therefore, saturated, and the water will rise, for example, to IGK, and this elevated water, independent of any increase of attraction from the diminished distance, is considered by the author as a weight suspended to the chain which connects the two plates, and producing a greater reaction on the plates themselves. The plates will not, therefore, continue in equilibrio, but will come together and strongly adhere.

This effect of fluids upon the sides of capillary vacuities, whether it be well accounted for or not by the comparative argument of the weight, is of considerable moment in the phenomena of nature. M. Monge observes, that as these plates come together and are retained, not by any direct attractive force, but indirectly by the operation of the fluid; so likewise by extending the argument to submerged bodies we may account for the strong adhesion of the parts of the crystals of salts in their separation from water, such as the sulphate of lime, which loses its water of crystallization and its tenacity by heat, sulphate of soda, and many other hard crystals, which fall into powder by drying, &c.

The laws of attraction, when two plates are plunged in mercury, as in fig. 13, is also considered by our author. This pressure will be equal to the weight of a column of the fluid, whose base is the surface DH of the glass, and height is half the depth between D and H. If the plates be placed horizontally beneath the fluid, as in fig. 14, the pressure will be twice as great, and constant, provided the depth AB be at least equal to the limit of repulsion IH. It may also be remarked, that if a small hole M, fig. 15, be made at less depth, the mercury will not flow out, though it would issue from the hole M, fig. 14, below that depth. Hence

it

it is seen why a certain degree of pressure is required to force mercury through a small aperture, and a small quantity of this fluid may be retained in a cloth without passing through.

A few experiments were made by the author to determine the elevations and reactions when the plates were separated by the interposition of a wire of known thickness. When the distance was four thirty-third parts of a line, the elevation of the water was 15 lines and a half.—When it was four forty-ninth parts the elevation was 33 lines and a quarter; and when it was one twenty-eighth part the elevation was 74 lines. But, from some difficulties which presented themselves, it was not practicable to measure the adhesion farther than to determine that they were much less than the pressure of a column of water, having the surface of the plate for its base, and the column of liquid for its height.

X.

Observations on the Proportion of real Acid in the three ancient known mineral Acids, and on the Ingredients in various neutral Salts and other Compounds. By RICHARD KIRWAN, Esq. LL.D. F.R.S. and M.R.I.A. (Continued from page 215.)

Of the Use of these Tables. (Without abridgement.)

PROB. 1. **A**N extratabular specific gravity being given, but intermediate between some of those in the table, to find the quantity of real acid in 100 parts of such acid liquor.

1st. Find the difference betwixt the next higher and lower tabular densities = D ; and also the difference betwixt their acid contents = D' .

2d. Find the difference betwixt the extratabular sp. gravity and the next upper or next lower, whichever it is nearest to, = d , and let the difference betwixt its acid contents (or quantity of real acid) and those of the next upper or lower = d' , which is the quantity sought; then as $D. D' :: d. d'$, then $d' = \frac{D'}{D} d$, consequently d' added to the acid contents of the lower tabular sp. grav. or subtracted from the upper, is the quantity sought.

Note. In general when d , that is, the difference between the extratabular sp. grav. and any tabular sp. grav. does not exceed $\frac{2}{10000}$ it is insensible, and the acid contents of the lower or upper, whichever is nearest, may be ascribed to it.

PROB. 2. The quantity of real acid in 100 parts of an acid liquor being given but extratabular, being intermediate between some of the quantities in the tables, to find the sp. grav. of such acid liquor.

Find D, D' and d' as in the foregoing problem; then $d = \frac{D}{D'} d'$; then d , added to the lower tabular sp. grav. or subtracted from the upper, gives the sp. grav. sought.

But

But with regard to the *marine acid* its sp. grav. is to be investigated according to the ordinary mathematical rules.

PROB. 3. To find how much water must be added to 100 parts of an acid liquor of a given sp. grav. to bring it down to another lower given sp. grav.

1st. Find by the table the quantities of acid and water in 100 parts of each of the acid liquors respectively, each being supposed to be in the table, let the quantity of water in the denser be W , and the quantity of acid $= A$, let the quantity of water in the less dense $= w$, and the quantity of acid $= a$, and the quantity of water to be added to 100 parts of the denser $= m$: then $W + m$ must be to A as w to a . And $W a + a m = A w$. And $a m = A w - W a$. And $m = \frac{A w - W a}{a}$.

PROB. 4. Given weights of two or more acid liquors of different sp. gravities being mixed, to find the quantity of real acid in 100 parts of the mixt liquor and its sp. grav.

Find the sum of the quantities of real acid in 100 parts of the mixture, then find the resulting sp. grav. by the 2d problem; if the given sp. gravities be extratabular, the operation must be more tedious, as the acid contents of each must be found.

PROB. 5. The quantity of an acid liquor requisite to saturate 100 parts of any basis being found, to find the sp. grav. of that acid liquor.

1st. Find by the 4th table the quantity of real acid requisite to saturate 100 parts of the given basis, it is then plain that the *given* quantity of acid liquor contains the requisite quantity of *real acid*, since it is supposed to saturate 100 parts of the basis, and hence we may see how much 100 parts of such acid liquor contains of *real acid*, and if this last found quantity be in the table, its sp. grav. will be seen, but if extratabular, its sp. grav. must be sought by the 2d problem.

PROB. 6. The quantity of real acid requisite to saturate 100 parts of any basis being known, to find how much of one acid liquor of any given sp. grav. is requisite to saturate that, and consequently any other given quantity of such basis.

If the given sp. grav. of the acid liquor be *tabular* the quantity of real acid in 100 parts of it is apparent, and consequently the quantity of such acid liquor containing the required quantity of real acid is easily found by the rule of proportion. But if the given sp. grav. is *extratabular*, the quantity of real acid in 100 parts of the acid liquor must be sought by the first problem.

PROB. 7. The quantity of real acid in a given quantity of an acid liquor being known, and also the quantity requisite to saturate 100 parts of any given basis. To discover the quantity of such a basis contained in any solution, or in any powder, by which the given quantity of acid liquor is saturated.

If the basis be single (that is unmixed with any other basis to which the acid may unite) or combined only with fixed air, the solution is easy, but if the given bases be mixed with other bases combinable with the same acid, the solution is more complex and varies according to the variety of cases.

PROB.

PROB. 8. To find how much of an acid liquor of *one sort* will hold as much *real acid* as is held by a given weight of an acid liquor of *another sort*, whose sp. grav. is also given:—for instance, how much vitriolic acid will contain the same quantity of real acid as is contained in 100 grains nitric acid, whose sp. grav. is 1,3925.

1st. First find by the table the quantity of real acid contained in the given quantity of the second acid, whose sp. grav. is given, or if not in the table it must be found by problem 1st.

2d. It is apparent that the quantity of the first acid liquor must vary with its sp. gr. thus, in the instance given, as 100 parts nitrous acid of the sp. grav. 1,392 contains 50 parts real nitrous acid, so 100 parts vitriolic acid, whose sp. grav. is 1,5202 contains by the table the same quantity of real acid, *v. z.* 50 parts, but of the vitriolic acid, whose sp. grav. is 1,800, only 64 parts are requisite to contain 50 parts of real acid, whereas 200 grains are requisite of the vitriolic acid, whose sp. grav. is 1,2320.

Note. The solution of this problem may hereafter be found of use in comparing the quantities and affinities of oxygen in different acids.

PROB. 9. To find the sp. grav. of such vitriolic acid as that 100 parts of it shall contain the same quantity of real acid as 100 parts of the nitrous.

This can be found only by *inspection* on consulting the tables; an example has been seen in the last problem; so also 100 parts vitriolic acid 1,3102 contain the same quantity of real acid as 100 parts of nitrous acid, whose sp. grav. is 1,2687. And 100 grains vitriolic acid, whose sp. grav. is 1,1746, contains the same quantity of real acid as 100 grains sp. salt, whose sp. grav. is 1,159.

And 100 grains nitrous acid 1,1963 contains the same quantity of real acid as 100 grains spirit of salt whose sp. grav. is 1,187.

Hence it should seem that the sp. grav. of the *real marine acid* is smaller than that of the *real nitrous*, and that of the real nitrous smaller than that of the *real vitriolic*, since when the weight of each acid, and also the weight of real acid in each is equal, the vitriolic acid is specifically heavier than the nitrous, and the nitrous than the marine; but this, perhaps, may arise from penetration.

PROB. 10. To find how much of a neutral salt of one sort holds as much real acid or basis as a given weight of the *same* neutral salt in another state, or as a given weight of *another* salt in any given state.

These questions are resolved by the 4th and 5th tables. Thus, if it be asked how much nitre contains as much acid as 20 grains of vitriolated tartarin? By the 4th table I see that 221,48 parts of vitriolated tartarin and 227,22 parts nitre contain equal quantities of acid since both contain 100 parts, then as $221,48 : 227,22 :: 20 : 20,5$.

Again, How much desiccated soda will hold as much alkali as 30 parts crystallized soda? In the 5th table I see that 541,1 parts of the crystallized hold as much alkali as 227,4 parts of the desiccated, then as $541,1 : 227,4 :: 30 : 12,6$.

PROB. 11. How much of a given basis will be requisite to saturate the acid contained in a given

given quantity of a given neutral salt; thus, how much desiccated soda will be requisite to saturate the acid contained in 50 parts crystallized Epfom?

By the 4th table I see that 100 parts real vitriolic acid are contained in 340 parts crystallized Epfom. Then if $340 : 100 :: 50 : 14,7$, then by the 3d table I see that 100 grains of real vitriolic acid saturate 78,32 of soda. Consequently if 100 saturate $78,32 :: 14,7$ would saturate 11,51 of soda.

Lastly, In the 6th table I find that 100 grains desiccated soda contain 60 of soda. Then if $100 : 60 :: x : 11,51$; then $x = 19,1$ parts desiccated soda. Then 19,1 parts desiccated soda will saturate the acid contained in 50 parts crystallized Epfom.

Note 1st. This problem is of use in determining the quantity of any precipitating substances to be employed in decompositions, operated either by a single or double affinity. But in most cases *more* of the precipitant must be employed than the exact quantity necessary for saturation, and particularly when decompositions are attempted in the dry way, as otherwise a complete contact with the substance to be decomposed will not be attained, or if volatile it may be sublimed before the decomposition takes place.

PROB. 12. Some analysts have denoted the strength of their acids by expressing the quantities of each necessary to saturate a *certain quantity* of alkaline liquor (and sometimes of another basis), without even telling whether the alkali was mild or caustic, or the quantity of it contained in the alkaline liquor. This problem is consequently indeterminate. However, a method of giving some solutions of it may be understood from the following example; and circumstances will generally shew whether the application to particular cases be just.

Link tells us that 240 grains of a vitriolic acid, which he employed, saturated 6,5 times its weight of tartarin (he must mean in a liquid state, as no vitriolic acid will saturate six times its weight of real alkali), and that 240 grains of the nitrous acid he employed saturated 2,5 times its weight of the same alkali. *Quere*, the spec. gravity of both acids?

1st. It is plain, that since 240 grs. of the nitrous acid saturated 2,5 times its weight of the alkali, 624 grs. of that acid would saturate 6,5 times its weight of the alkali; and since 624 grs. of the nitrous acid would saturate as much alkali as 240 of the vitriolic acid, then 260 grs. of it would saturate as much alkali as 100 grs. of the vitriolic acid could saturate. Therefore, supposing 100 of the vitriolic acid to contain 75 of real acid, since more real nitrous acid is required to saturate a given quantity of tartarin than of vitriolic acid, in the inverse ratio of 1214 to 1177 (as appears by the third table), then denoting the quantity of real nitrous acid in 260 grs. of the nitrous liquor by x , we have the following equation, as $1214 : 1177 :: x : 75$. and $x = 77,55$. Then 260 grs. of the nitrous acid contain 77,55 of real nitrous acid, consequently 100 grs. of it contained 29,82 real acid. And, therefore, its sp. grav. was nearly 1,234, and that of the vitriolic about 1,800.—The quantity of alkali in the alkaline liquor might also on this supposition be determined.

So if it be required to know how much common salt is requisite to decompose a solution of nitrated silver containing 176,25 grs. of silver.

1st. I find

1st. I find by the 6th table that 75 grs. silver take 16,54 of marine acid, consequently 176,25 grs. silver take up 38,87.

2d. By the 4th table, I find that 100 grs. muriatic acid are contained in 257,2 of common salt, consequently 38,87 are contained in 99,973, that is 100 grs. common salt, then 100 grs. of it are necessary to precipitate the silver.

XI.

Experiments on certain Principles obtained from Animal Substances treated with the Nitrous Acid.

*By CIT. WELTER.**

THE author having treated silk with the nitric acid in order to obtain the oxalic, was surprized to find none, and to obtain at the end of the operation a silky salt of a golden yellow colour, which acted in the same manner as gunpowder by the contact of an ignited coal. As he made this experiment only once, he has thought fit to describe it, that others may repeat it.

He poured upon one part of silk six parts of common nitrous acid, to which a small quantity of concentrated nitrous acid was added. After two days' repose, he distilled the mixture, and adding the contents of the retort and receiver together, he threw the whole on a filter. The oxalic acid having crystallized on the filter, he returned the whole into the retort, together with a considerable quantity of water which was used in washing the filter. A quantity of the water was then distilled off, but the residue not crystallizing, he returned the distilled liquor; and after repeating this operation several times, he obtained for a residue an acid liquor of the same weight as the silk, which contained small crystalline grains.

This fluid afforded no signs of oxalic acid. It was yellowish, and tinged the fingers and silk of the same colour, which was not weakened by washing with water.

Cit. Welter saturated this fluid with lime; and after having concentrated it, he poured alcohol to it, which separated a substance of a gummy appearance. The alcohol diluted with water was then evaporated, and left a yellow substance mixed with the solutions of calcareous nitrate and muriate. He decomposed these salts by the carbonate of potash, and the fluid separated from the carbonate of lime was submitted to evaporation. It afforded gold-coloured crystals as fine as silk, which detonated like gunpowder with a black smoke. These crystals are soluble in water and in alcohol, and crystallize by cooling. The oxygenated muriatic acid renders them colourless. The sulphuric acid disengages a smell of nitric acid. The muriatic acid occasions in these solutions a precipitate of small whitish micaceous crystals, which are volatile, and in the fire emit a bitter and inflammable fume.

* Abridged account of a memoir read before the French National Institute. Bulletin de la Société Philomatique, no. 25. Germinal vii.

This golden, yellow, detonating, and crySTALLIZABLE substance, is called the bitter principle by the author. Its crystals appear to be octahedrons.

As animal substances become yellow by the contact of nitrous acid, citizen Welter endeavoured to extract the latter principle from beef; but he found it combined with another substance likewise unchangeable by the nitric acid. This combination, which is soluble in the concentrated nitric acid, is separated by water in the form of a yellow powder, which does not change by exposure to the air, and may probably be useful in painting.

Cit. Welter was induced to suppose, that this powder is composed of the bitter principle, and another substance, from the circumstance of his having obtained the latter substance by treating sponge with the nitric acid. It is colourless, soluble in the concentrated nitric acid, and precipitable by water like the other powder.

These facts seem to show, that animal matters treated with the nitric acid, afford two substances as a residue, which are not changed by this acid, and are found either in a combined or separate state. It appears that silk affords the bitter principle in a state of purity. Sponge affords the second substance pure; and beef affords a combination of both. The bitter principle is yellow and soluble in water; the combination of both is also insoluble in water, but coloured.

Cit. Welter remarks, that having made his experiment only once, and not being able yet to determine with precision the circumstances upon which the production of the bitter principle depends, he has thought it necessary to relate the whole.

XII.

*A short Account of the Life and Writings of Defauffure. By A. P. DECANDOLE. **

HORACE Benedict Defauffure was born at Geneva in the year 1740; his father, an enlightened cultivator, to whom the public is indebted for some memoirs concerning rural economy, resided at Conches, a country-house situated on the banks of the river Arve, half a league from Geneva. This habitual residence in the country, together with an active education, was undoubtedly the cause which developed in Defauffure that natural strength of constitution so necessary to the practical cultivator of natural history. He went every day to the town, in order to profit by the advantage of public education. Residing at the foot of the Salève, a mountain he has since rendered famous by his researches, it was an entertainment to him to climb its rugged paths. Living thus surrounded by the phenomena of nature, and possessing the advantage of study, he became attached to natural history, without imitating those learned men who form theories without leaving their cabinets, nor those men of mere practice, who being continually surrounded by natural scenes, become incapable of admiring their beauty.

His first passion was for botany. A varied soil, producing numerous different plants, invited the inhabitant of the borders of the Lemane lake to cultivate this agreeable science. This taste of Defaussure led him to form a connection with the great Haller. He paid him a visit in 1764, during his retirement at Bex, and gives an account in his travels of his admiration for this surprising man, who excelled in all the natural sciences. Defaussure was still more excited to study the vegetable kingdom by his connections with Charles Bonnet, who had married his aunt, and who soon perceived the value of his nephew's increasing talents. Bonnet was then employed on the leaves. Defaussure also studied these organs of vegetables, and published the result of his enquiries under the title of "*Observations on the Bark of Leaves.*" This little work, which appeared soon after the year 1760, contains some new observations on the epidermis of leaves, and in particular on the milky glands which cover them *.

About this time the place of professor of philosophy became vacant. Defaussure, then just in his twenty-first year, obtained it. Experience proves, that if very early recompences extinguish the zeal of those who exert themselves merely for the sake of reward, on the contrary they increase the industry of those who are in search of truth. At that time the two professors of philosophy taught by turns natural philosophy and logic. Defaussure filled these two offices with equal success. He gave a practical, we may say an experimental, turn to the science of logic. His course, which began with the study of the senses, in order to arrive at those general laws of the understanding, shewed that he was even then a close observer of nature.

Natural philosophy being the object of his attachment, led him to study chemistry and mineralogy; and soon afterwards he recommenced his travels in the mountains, not only to examine the plants, but to observe the mountains themselves, whether he considered their composition or the disposition of their masses. Geology, a science then scarcely known, gave a charm to his numerous walks in the Alps. Here it was that he discovered himself to be a truly great philosopher. During the fifteen or twenty first years of his professorship he was employed in performing the duties of his office, and in surveying the mountains in the neighbourhood of Geneva. He extended his excursions on one side as far as the banks of the Rhine, and on the other to Piedmont. About this time he made a journey into Auvergne, to examine the extinct volcanoes; and another to Paris, Holland, and England, and afterwards to Sicily. These voyages were not merely excursions from one place to another. They had only one object, namely, the study of nature. He never travelled without being provided with every instrument that might be useful to him; and always before he set out, he sketched the plan of the experiments and observations he intended to make. He often mentions in his works, that he found this method of great utility to him.

In 1779 he published the first volume of his *Travels in the Alps*. We there find a complete description of the environs of Geneva, and an excursion to Chamouni, a village at the

* He resumed this subject eighteen months before his death.

foot of Mont Blanc. Natural philosophers will read with pleasure the description of his magnetometer. The more he observed the mountains, the more he perceived the importance of mineralogy. In order to study it to greater advantage, he learned the German language; and in the last volumes of his Travels, we may easily perceive how much new mineralogical knowledge he had acquired.

During his numerous excursions among the Alps, and even in the midst of the political troubles of Geneva in 1782, he found opportunities to make his experiments on hygrometry, which he published in 1783 under the title of "*An Essay on Hygrometry.*" This work, the best he ever wrote, completed his reputation as a natural philosopher. We are indebted to him for the invention of an hygrometer. Deluc had already invented an hygrometer of whalebone, on which subject a dispute was maintained between him and Defauffure, which was even attended with a considerable degree of earnestness.

In 1786 Defauffure resigned the place of professor, which he had held for nearly 25 years, to Pictet, his disciple and colleague, who performed with reputation to himself the difficult task of succeeding this great philosopher.

Defauffure being called upon by his office to attend to public education, made it a particular object of his attention. He presented a plan for reforming the course of education at Geneva. He proposed to teach children very early the natural sciences and mathematics; he was even attentive to their physical education; and, that it might not be neglected, proposed the adoption of gymnastic exercises. This plan excited great attention in a town where every one is aware of the importance of education. It found both admirers and censurers. The mediocrity of their pecuniary resources was a great obstacle to every important innovation. They were apprehensive that in changing the form they might lose sight of the principle, and that an alteration, even for the better, might destroy the good they possessed. The Genevese were attached to their form of education, and they had cause, for it had not only introduced general information among them, but had given the first spring to the talents of several distinguished mathematicians *, and natural philosophers †.

Public education did not alone claim the attention of Defauffure. He attended himself to the education of his two sons and his daughter, who have shown themselves worthy of such an instructor. His daughter unites to the accomplishments of her sex, an extensive knowledge in the natural sciences. His eldest son is already known by his works in natural philosophy and chemistry.

The second volume of his Travels was published in 1786. It contains a description of the Alps which surround Mont Blanc. The author considers them as a mineralogist, geologist, and natural philosopher. It contains, in particular, some very interesting experiments on electricity, and a description of his electrometer, which is one of the most complete we possess. We are likewise indebted to him for several instruments of measurement; his cyanometer, designed to measure the intensity of the blue of the heavens, which varies according to

* Abauzit, Cramer, P'Huiler, F. Trembley, &c.

† Jallabert, A. Trembley, Bonnet, Lefage, Deluc, Senebier, Prevost, Pictet, and Defauffure himself.

its elevation; his diaphanometer, or his method of measuring the diaphaneity of the air; and his anemometer, in which, by means of a kind of balance, he weighs the power of the wind.

Some years after the publication of his second volume, Defauffure was received as a foreign associate of the academy of sciences, and Geneva could boast of having two of its citizens in these seven eminent situations. Defauffure not only honoured, but was desirous of serving his country. He founded the society of arts, to which Geneva is indebted for that prosperity it has gained through its industry within the last thirty years. He presided in this society to the very last; and it was one of his principal objects to support that useful establishment.

He also showed his zeal to serve his country while he was member of the Council of Five Hundred, and of the National Assembly. It was from his assiduous labour in that assembly that his health first began to fail; and in 1794 a paralytic stroke deprived him of the use of one side of his body. However painful his situation might then be, he lost nothing of the activity of his mind; for it was after this accident that he drew up the two last volumes of his Travels, which appeared in 1796. They contain an account of his travels in the mountains of Piedmont, Switzerland, and in particular of his ascent to the summit of Mont Blanc. These two last volumes, so far from appearing to partake of the weakness of his condition, offer a considerable mass of important facts and observations in natural philosophy.

He gave the last proof of his attachment to science in publishing the Agenda, which completes the fourth volume. Here this great man has surpassed himself. He conducts the young naturalist amidst the mountains, and teaches him to observe them to advantage. This Agenda is a proof of his genius, and the strength of mind he preserved amidst all his sufferings. During his illness, he also published his observations *on the fusibility of stones with the blowpipe*; and he directed the *experiments on the height of the bed of the Arve*. When he was at the baths of Plombières for his health, he observed the mountains at a distance, and procured specimens of the strata he perceived in the most steep rocks. He had announced to the public, that he intended to complete his Travels by his ideas on the primitive state of the earth; but the more new facts he acquired, and the more he meditated on this subject, the less could he determine with regard to those great revolutions which have proceeded the present epoch. In general, his was a Neptunian, that is to say, he attributed to water the revolutions of this globe. He admitted it to be possible that elastic fluids, in disengaging themselves from the cavities, might raise mountains.

Though his health was gradually impaired by degrees, he still retained the hope of re-establishing it. The French government having appointed him professor of natural philosophy in the school of Paris, he did not despair of possessing that honourable office at some future day; but his strength failed him, and a general want of energy succeeded the activity he had formerly enjoyed. His slow and embarrassed pronunciation no longer displayed the activity of his mind, but formed a striking contrast with the agreeable vivacity which formerly distinguished him. It was an affecting sight to behold this great man so worn out at a time of life when the mind is most active in meditation, or at least when he should have enjoyed the fame and knowledge he had acquired.

It

It was in vain he tried all the remedies which medicine, assisted by the natural sciences, could offer. Life and strength abandoned him by slow and painful degrees, and towards the end of the 6th (republican) year, his decay became more evident; his memory failed; and at length, on the 3d of Pluvoise, in the 7th year, at the age of 59, he completed his brilliant career, much regretted by a family who loved him, a country to which he was an honour, and Europe, whose knowledge he had increased.

By his side, and at the same moment, a violent death robbed the sciences of a young man whose industry and talents had afforded the most flattering hopes. (Qu.?)

I must here conclude this short account; and it may easily be perceived that I am very far from making the eulogy of my illustrious countryman. I had neither the necessary materials, nor sufficient means; that interesting task is reserved for one who has been the companion of his travels and labours, and who, by living habitually with him, has had the advantage of observing his manner of acting and thinking.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

On Steam.

AN apparatus has been exhibited to the Linen Board in Dublin, which, added to the boilers used in bleach-greens, will collect the steam, and communicate it to the machinery for turning the mill-work in room of, or in aid of, water-mills.

The trustees of the linen manufactory are so well satisfied with this invention, that they give a considerable sum in aid of those who will first make trial of it in the large way.

There can be no doubt but that the machinery used in the linen manufacture may be wrought by the power of steam: and it is certain also, that a considerable quantity of steam is produced by the common mode of boiling the linen; which if collected, and applied to proper machinery, would do somewhat of work. Whether the power of this steam, as a first mover, is adequate to the cost of the machinery, is a point on which I have yet heard nothing conclusive. To determine this point, we require to know what quantum of steam is equal to any known power; and what quantum of steam a boiler of a certain size will generate, kept a given time in a boiling state? Or, in other words, how many cubic feet of water converted into steam, is equal to the power of one horse? In the state of boiling, what quantity of steam will be generated from a given quantity of water in a given time?

Mr. Nicholson will oblige a constant reader, by referring in his next Journal to where the above data are to be found.

A.

Londonderry, August, 1799.

On the theory of the dilation of steam and other elastic fluids, there is a valuable treatise by Prony, in the second cahier of the *Journal de l'Ecole Polytechnique*; and the same author has treated the subject very ably in his *Architecture Hydraulique*. Desaguliers has given some good documents of Beighton, in his *Course of Lectures*, with regard to the atmospheric steam-engine.

engine. Much practical information is to be found in *Curr's Coal-viewer, and Engine-builder's Practical Companion*, sold by Taylor, in Holborn. Dr. Hutton has also written on this subject, in his *Mathematical and Philosophical Dictionary*, art. *Steam*; and professor Robison more amply in the *Encyclopædia Britannica*, art. *Steam*, and *Steam-engine*.

W. N.

Pasigraphy, or Universal Writing.

A numerous meeting of friends to the arts and sciences was held the 25th of Prairial, at the Republican Lyceum at Paris, where C. Demainieux explained the twelve rules of Pasigraphy, and the three rules of Pafilaly, two arts which only make one, and of which he is the inventor. The former, as the term implies, is a method of universal writing, which can be read in all languages at once. That is to say, its characters, denoting ideas and not words, may be read at the same time, by six persons speaking each a different language, by communicating the very same ideas to each, who will express them to himself in his own terms. The second art is an universal language, of which that writing is the basis. The perspicuity of the exposition, the simplicity of the means, and the manifest utility of the results were much applauded. It is known that the National Institute has directed its attention to this discovery, and have appointed a commission to examine it. C. Ræderer, who is charged with this report, has already read it, in a private sitting of the class of moral and political sciences, of which he is a member. He treats the subject philosophically, not merely as an universal method of conveying our ideas in every language, but likewise in a still more important point of view, as a method of extending our researches, and carrying them to a higher degree of perfection.

Several foreign philosophers attended, and among them, M. Van Swinden, of Amsterdam, and M. d'Ossuna.

Mag. Encyclop.

French Weights and Measures.

I have deferred rectifying the tables of French weights and measures, as notified on the wrapper of Number 27, till I shall receive the new report of the commission appointed for that purpose. Dr. Delametherie informs us, *Journal de Phys.* V. 460. that the metre is settled at 3 French feet, 11 lines, and 0.296 of a line.

Royal Institution of Great Britain,

For diffusing the knowledge, and facilitating the general introduction of useful mechanical inventions and improvements, &c. is now incorporated by charter, under the patronage of his majesty, by the title of the Royal Institution of Great Britain. The present officers are:

President,

President, the Earl of Winchilsea.—Vice-presidents : the Earl of Morton; the Earl of Egremont; and the Right Honourable Sir J. Banks, Bart. K.B. P.R.S.—Committee of managers.—For three years: Earl Spencer; Count Rumford; and Richard Clarke, Esq. Chamberlain of the City of London.—For two years: the Earl of Egremont; the Right Honourable Sir J. Banks; and Richard Joseph Sullivan, Esq.—For one year: the Earl of Morton; the Right Honourable Thomas Pelham; and Sir J. C. Hippisly, Bart.—Treasurer, Thomas Barnard, Esq.—Secretary, the Rev. S. Glasie, D.D. F.R.S.—Committee of visitors.—For three years: the Duke of Bridgewater; the Lord Bishop of Durham; and Thomas Bernard, Esq.—For two years: the Lord Viscount Palmerston; Lord Teignmouth; and Rowland Burdon, Esq.—For one year: the Earl of Besborough; Lord Somerville; and Samuel Thornton, Esq.—State of the subscription lists, the 6th of July, 1799:—138 persons had given in their names as subscribers of 50 guineas each, and hereditary proprietors of the institution; 103 persons, as subscribers for life, at 10 guineas each; and 97 persons as annual subscribers, at 2 guineas each;—338 subscribers.

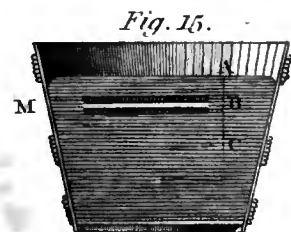
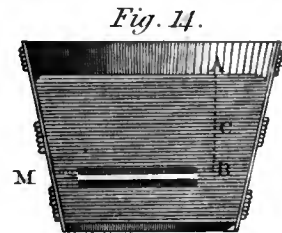
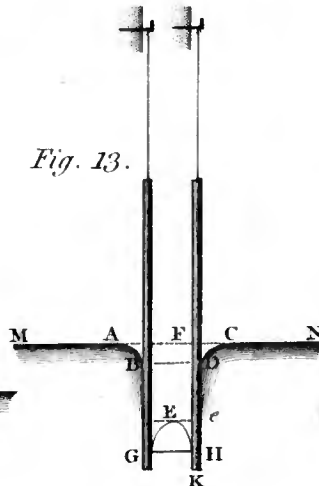
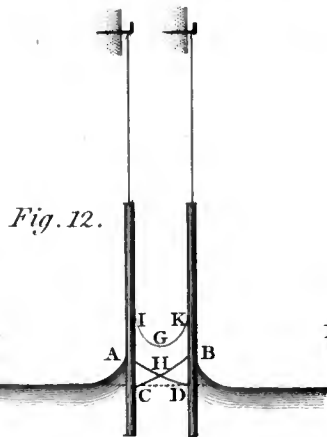
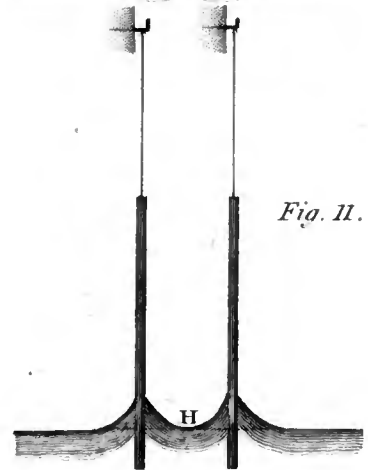
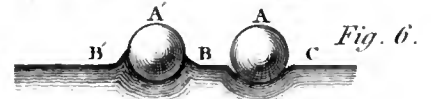
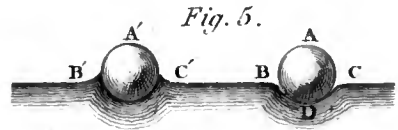
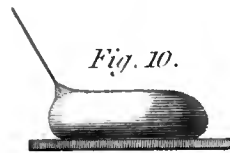
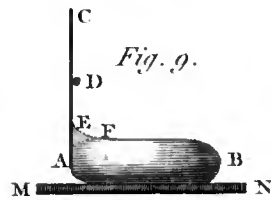
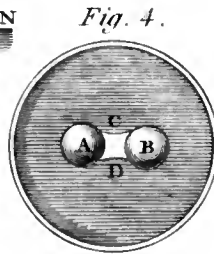
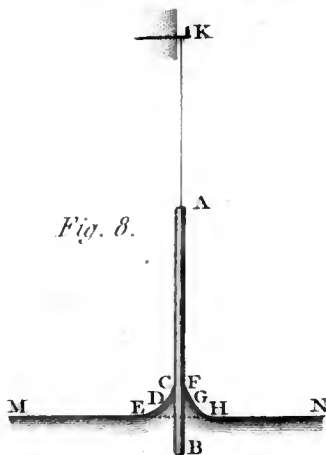
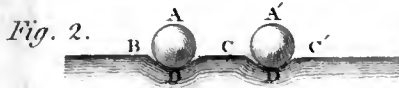
As many ingenious tradesmen and artificers may be desirous of having their inventions and their work exhibited in the repository of the institution, the managers, desirous of bringing forward into public view all such mechanical inventions and improvements as are worthy of the public notice, have resolved:

That the gratuitous assistance of such respectable and wealthy artists, tradesmen, and manufacturers, as deal in any of the articles that will be wanted in the arrangement of the institution, or as are proper to be exhibited in the repository, be accepted by the managers; and that the articles so furnished *gratis*, be marked with the names and places of abode of the donors. The managers have also resolved:

That the assistance of such ingenious and well-informed persons be accepted, as may offer their gratuitous services in the capacities of draughtsmen, artists, artificers, overseers of the work, &c. in the arrangement of the various details of the institution; and that persons so giving their voluntary assistance for a certain time, be permitted to have free access to the institution, and to attend, *gratis*, all the philosophical lectures during a certain period.

Persons who are desirous of availing themselves of either of these resolutions, are requested to send in their proposals in writing, to the managers of the institution, stating specifically, what they are willing to undertake.

Mr. Kirwan's Essay on the Analysis of Mineral Waters, and his Geological Essays, will both appear early this month (September).



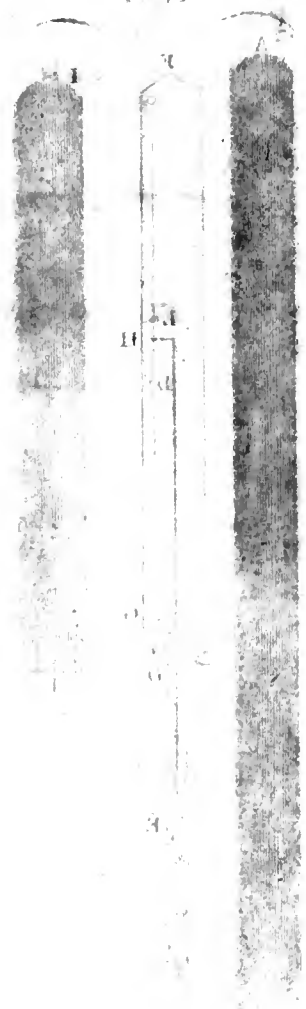


Fig. 1.

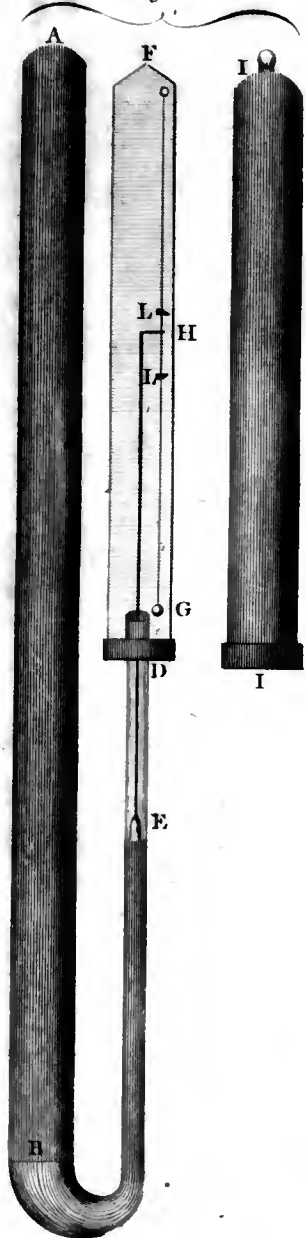


Fig. 2.

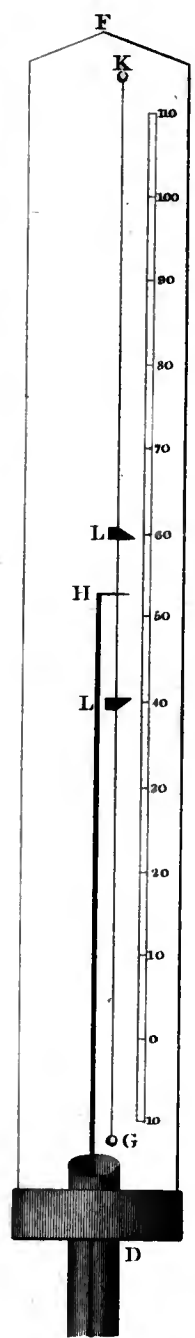
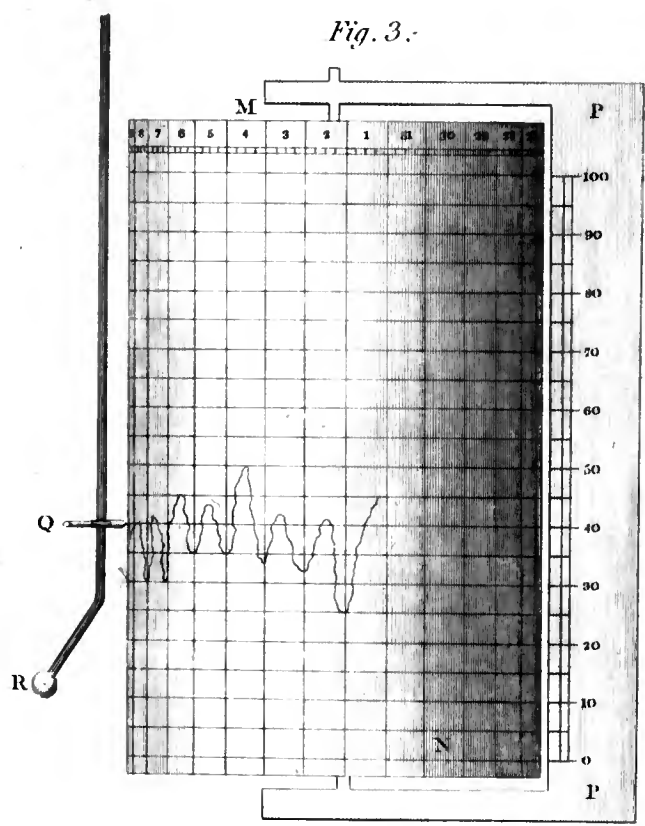


Fig. 3.





A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

OCTOBER 1799.

ARTICLE I.

*Geological Observations on the Vicinity of Hull and Beverly. By JOHN ALDERSON, M.D.**

TO MR. NICHOLSON.

SIR,

IN consequence of the appearance, in your truly valuable miscellany, of Dr. de Serra's account of a submarine forest forming islets on the coast of Lincolnshire, I have inclosed you a section of this part of Yorkshire, which I made some years ago, to explain the origin and situation of our springs, and have transcribed my description of the country as published in the Hull Advertiser, October, 1795.

This neighbourhood (by this neighbourhood I mean the environs of Hull and Beverly, including all Holderness) may be divided into two distinct regions, or districts—the high and the low lands. The high land is a ridge of hills, forming an arc or bow, extending from Flamborough-Head to the Humber, where it is divided; and is then continued down the Lincolnshire side of that river to the point opposite the Spurn at the mouth of the Humber.

The east side of the ridge is entirely composed of chalk and flint, in most places in regular horizontal strata, the flint being from six to eight inches thick, the chalk from two to three feet; in a few places the flint is in nodules, and here and there in the chalk rock are found nodules of iron ore and thin strata of fullers'-earth. Towards the base of the ridge, the particles of these earths are to be found washed down and forming gravel; this operation has

* Communicated by the author.

been evidently performed under water; for at a certain height the gravel ceases to be found, and the line is distinctly marked by the particles at that level being rounded, the consequence of agitation, as on the sea-beach.

The depth of the chalk rock is not known; it has been bored into 50 feet below the Humber; the height at Riplingham, or top of the wolds, is about 400 feet.

The low land, which stretches from the base of the hills to the sea, whose cliff forms the string of the bow, may again be divided into warp-land and alluvial: the former is uniformly level, and would be covered every high tide by the Humber, were it not restrained within proper limits by embankments. The latter, or alluvial, is uneven ground, and rises by a gentle ascent to the sea cliff. This ground is partly gravel, partly clay, variously mixed with shells, and with, here and there, particles of culm or powdered coal. The cliff, in some parts, is near 100 feet above the level of the sea.

The depth of the warp is nearly the difference of the tide, being about 22 feet; below this warp is found a stratum of vegetable mould, or moor earth, in which have grown trees of a great size: this stratum is about three feet deep, and may be found continued on the other side of the Humber, and may be seen at low ebb of spring-tides on both sides at the same level. Below this morass is found a bed of sand mixed with small particles of carbonated wood, chalk, clay, and salt water, sufficient to form what is called a * quick-sand, varying in depth according to the distance from the hills. Below the bed of sand is found a stratum of clay more or less thick and compact, and beneath this the chalk rock (a continuation of the lowest stratum of the hills), which declines towards the Humber and the sea about five yards in the mile; generally under this clay, and on the surface of the chalk rock, lies a reservoir of water kept down by the clay: when this clay is perforated, the water rises to its level, and, if well piped, will flow over the warp. This water breaks out at the base of the hills, at a place called Spring-Head, and is conveyed to Hull for the supply of the inhabitants.

The moor earth has been universally found at the same level wherever the ground has been bored, and may be found along the course of the Humber within low-water mark, and along the sea coast from the Spurn to Bridlington. Although I have no positive facts that the morass extends under the alluvial part of Holderness, yet I have some reason to conclude it does from the following circumstance: the morass at Seathorn or Withernsea, along the coast, which is only visible at the lowest ebb, is not one hundred yards from the cliff. Sixty yards of the cliff have gone into the sea within the memory of many now alive, and there are writings, &c. of many cliff closes being transferred or assigned, whose identity is beyond the memory of any one at present living, and therefore I conclude the cliff extended over this morass. It may be suggested that this bog is the bottom of a mere or mar, similar to that at Hornsea, where trees have been found: but there is no mention of any such mar

* When Mr. Dodd, in the year 1797, suggested to me the possibility of making subterraneous passages under rivers, I shewed him the impracticability of it here, from the annexed section, owing to the nature of this quick-sand, which defeats every attempt at well-making, by blowing up in the night the labour of the day.

near this place. From the regular falling of the cliff on the average of the last fifty years, this spot may have been uncovered within the present century.

The same may be presumed at various places along the coast, where this submarine wood, &c. are found within a short distance from the cliffs that have been for many years gradually wasting. In this submarine bog have been found the horns of the red deer, the head of an animal of the ox kind, oak, fir, and hazel; the nuts of the last quite perfect when first taken out. This wood is here called, not unaptly, Noah's Wood. A few years ago the trunk of a large tree was taken out, evidently cut off at both ends, and hollowed out so as to form a canoe capable of carrying a man; it was so perfect when found, that I am credibly informed it would have answered that purpose. I have also seen the head of an instrument resembling an axe, formed of one stone; round one end a groove has been hollowed out, in which the handle of the instrument has been twisted—in the same manner as the blacksmiths do at this day round some of their tools—and the other is wrought to a fine edge.

Another circumstance which inclines me to believe that this part of Holdernefs lies upon this moor earth is, that a mile from the sea, in sinking a well, the workmen met with a boggy earth, which emitted so noxious a stench that they escaped suffocation with difficulty: this was at the level of the sea. From the nature of the ground, which I have called alluvial, consisting of specimens of every kind of rock, from Flamborough-Head to the North of Scotland, I have been led to conclude the whole of that part has been brought there by some great irruption of the Atlantic Ocean, and precipitated upon the surface of this vegetable mould.

I have also been of opinion, that there was a time when this country was inhabited at the level of this morass, before this irruption of the sea, and that the Rhine and the Humber met, and mixed their waters, without the interference of the German Ocean. The manner in which the warp land has been formed from the Humber, since the ocean retired within its present limits, is very obvious: it is, at present, the practice in some parts of this country to suffer the Humber to pass freely over the land, in order that the proprietors may obtain a new soil at will. From certain data, which might easily be obtained, and the calculation of Wallerius, that level soil left to itself rises half an inch in a century, it would be no difficult matter to calculate the period of this deluge.

There are, as I mentioned before, traces of the water when it first broke in having risen up the sides of these hills, or wolds, something more than the height of the highest of the lands in Holdernefs; for in many parts, both in Yorkshire and Lincolnshire, I have found the rounded pebbles at the level of 110 or 120 feet above the sea; and I am of opinion, that, if searched for, at the same level there will be found * gravel, or other marks of the action of the sea, in almost every part of the island. Similar inferences seem to have been drawn by Linnæus, Wallerius, and Pallas, in their observations on the Baltic.

Many of the hills of gravel in Holdernefs evidently shew that they were formed by a periodic subsidence from water; some of them appear to have been conical, and every distinct

* What height is Kensington gravel pits?—A.

layer is conical : various sea shells are found in their natural state ; others evidently the product of some more ancient hill being agatized. We also find culm, or the washings from some stratum of coal in the north, and the alum schistus from Whitby, all blended and mixed in one heterogeneous mass.

I am, Sir,

Your most obedient and very humble servant,

Hull, Aug. 14, 1799.

JOHN ALDERSON, M.D.

II.

A circumstantial Description of the Method of cultivating the White Beet (Runkelrübe), in order to obtain the greatest Quantity of Saccharine Matter, and to prepare it for the Manufacture of Sugar. By F. C. ACHARD, Director of the Physical Class in the Royal Academy of Sciences. (Concluded from p. 243.)

18. **I**N support of these two positions, deduced from my own experiments, I wish to add a few well-known facts. With regard to the first, it is notorious, that not only the tender roots of asparagus, as well as those of hops, liquorice, and cucumber, possess a sweet taste so long as they are protected against the influence of light by being covered with earth ; and that they lose it as soon as, by sprouting out from the ground, they are exposed to the light and its action : in which case they acquire the peculiar sharp and acid taste of the plant.—In the same manner the uncovered part of the lower cole-rape has a sharp taste, in comparison with those of its parts which are covered by the soil. Carrots sown between hemp and poppies, if all other circumstances, such as soil and manure, be alike, become always sweeter than those which are cultivated on a soil not shaded by other overhanging vegetables.

19. Some may, perhaps, ascribe the decrease of sweetness in the root-germs pushing out from the earth to the *access of air* thus facilitated, and not to the *matter of light*. But to refute this assertion, I shall only mention, that the air undoubtedly penetrates, in every instance, through the uppermost stratum of the ground, unless the soil be thoroughly argillaceous, and of course very compact. To this I add the fact known to every gardener, that asparagus coming forth from the ground and covered with a *glass*, acquires, in a short time, a green head, and changes its agreeable sweetness for a bitter taste ; just as if it had not been covered. But if the asparagus be covered with an *opaque vessel*, such as a flower-pot, &c. it continues white in its subsequent growth, and retains all its sweetness. By this it is fully proved, that the agency of the matter of light, with an exclusion of the action of air, is the only cause of the loss of sweetness in asparagus. The heads of carrots, parsnips, beet-root, and perhaps of all vegetable root, which are not covered with the earth, lose the sweet taste naturally belonging to them. And hence the lower part of the root, on which the light cannot act,

act, is often very sweet, while the upper part, not covered by the earth, and thus exposed to action of light, is not in the least sweet, but even possesses a bitter taste.

20. The very reverse holds good with respect to fruits, more especially from trees, at least for the most part, if not with all of them. In this part of the plant the *matter of light* causes an *increase* of saccharine matter; and its influence is directly opposite to that which it exerts on the roots. I think it superfluous to offer proofs of this phenomenon, as every amateur of gardening knows that the pleasant and sweet taste of fruits from trees is most certainly produced by bringing them to a place where they are most exposed to the sun. Branches of one and the same tree yield fruits of different agreeable flavour and sweetness, according as they are in various degrees acted upon by light; so that the fruits from shaded branches are much inferior to those that are not shaded.

21. It is no less easy to prove, that by the absorption of light in the roots, the *extractive*, *sapaceous*, and *farinaceous* principles are increased, which give to the juice of the root the quality of leaving, on inspissation by fire, a tenacious, pulpy mass, which, on account of its consistence and other properties, throws great, and often insurmountable, difficulties in the way of the separation of the sugar, and likewise makes it impossible to produce a syrup of a pure sweetness.

22. To be thoroughly convinced of this, nothing more is required than to examine a runkelrübe, a carrot, a parsnip, a parsley-root, or any other root, in such a manner, that the part above the ground may be compared with the part that remained beneath. It will then be found, that the latter not only contains more juice than the former, but, in general, also that both these several parts are materially different as to texture, weight, compactness, and colour. The sweetness of the roots covered by earth may be diminished, only by causing the light to act on the upper uncovered part. By such procedure a considerable change in the saccharine matter, and other constituent parts, even in the lower part of the root, is effected.

23. Since the *chemical analysis*, as well as the *physiology of plants*, evinces that all this deviation in the nature of the parts of one and the same plant, accordingly as it stands above or below the ground, is imputable to the increase of those of its constituent parts which, taken together, impart to the inspissated juice of the root the property of resisting the separation of the sugar by crystallization; it follows from thence, if applied to the runkelrübe, that by preventing, in the best possible manner, the access of the matter of light to that part of the root which lies under the heart, or the place from which the leaves take their rise, the runkelrübe must be produced, not only richer in sugar, but also containing less of such other parts as impede the separation of the saccharine matter.

24. Upon the full proof of these assertions it is that my method of cultivating the runkelrübe is established. I found it perfectly confirmed and established by experiments, partly in my own culture, partly by comparing the saccharine contents of such roots as had grown in various places, and under different management. I therefore affirm that the runkelrübe only which is produced in the manner I have here indicated, will be so rich in sugar, and so
poor

poor in other parts, that render the separation of the sugar difficult, as to be employed for making sugar with extraordinary profit. On this account, I give to such economists as may cultivate the *runkelrübes* for that purpose the well-meant advice, to bestow the most careful attention on its culture, and to follow the method pointed out in this treatise; because they would otherwise be greatly disappointed by obtaining a sweet mucilage of a bad taste instead of crystals of sugar. I ground this assertion on my own experiments, as well as those of others acquainted with this subject. And for this reason, the proper culture of the *runkelrübe* may be considered as the basis of its useful application in the profitable manufacture of European sugar; since from roots of this kind whose saccharine matter has not been greatly increased by proper cultivation, no sugar can be obtained to advantage. Sugar may, indeed, be prepared from roots cultivated in a different manner from mine; but not with profit, though the loss may prove variable. This fabrication of sugar would thus continue, what it has long been, a subject of scientific research; in the same manner, as the preparation of sugar from green-peas, cabbage-leaves, melons, and many fruits, is, even now, and will probably remain, an object of such inquiries.

25. The assertions here deduced from experience will easily admit of a theoretical explanation. And, in particular, according to the theory given by the worthy counsellor of the mines, *Hombolt*, in his *Aphorisms*, which is founded on the most diligent observations, and a well-studied physiology of plants: this learned man proves that the separation of oxygen from plants is prevented by the absence of the matter of light. But oxygen is necessary to the formation of any acid, and the formation of acid is required to the production of saccharine matter. From this it is evident, that the influence of the matter of light, as a means of separating the oxygen, is, and must be, injurious to the formation of saccharine matter in superabundance. On the other hand, it follows from the same principle, that the oxygen retained in larger quantities in the root by all possible exclusion of the matter of light, must, of course, contribute towards increasing the saccharine matter*.

26. This application of the *runkelrübe* to the making of sugar in the large way, with variations in the products, according to its different culture, has already been perfectly confirmed by the experiments which I am now making, under the inspection of the royal committee. Of the *runkelrübes* from Magdeburg and Halberstadt, only those have yielded sugar which (1.) were of a small size, and consequently had grown close to one another; (2.) those had not large, rounded heads, but whose heads were buried under ground, and had been thoroughly covered with earth; (3.) those which had flat heads, and, therefore, from which the leaves had not been taken off; and (4.) those that possessed a conical or spindle shape;

* A contradiction seems to prevail in this place; as the matter of light rather promotes the increase of saccharine matter in the fruits of trees.—The reason of this must be sought for in the different organic structure of all the parts of plants, and in the consequent elaboration of their constituent parts; which, of course, must be different in the *esulent* fruits from that of the roots and their germs. Hence, the presence or absence of the matter of light must exert a different influence on the various parts of plants—namely, roots, leaves, fruits, &c. &c.—with regard to the formation of different parts in greater or smaller quantity.

which

which consequently had not been transplanted, but had received their full growth in the places where they germinated from seed.

On the contrary, those roots from Magdeburg and Halberstadt afforded only pulp and ill-tasted syrup, in which no crystals of sugar were formed. 1. These were strong and thick, and consequently grew at a greater distance from each other, by which they increased in bulk. 2. Their heads were large, and the marks where the leaves had been cut off were distinctly perceivable by the stalks which remained. 3. They possessed the round figure which is the effect of transplantation; and is very seldom obtained even by the use of seeds obtained from transplanted roots. This last event takes place only when the seed is procured through *several generations* from transplanted roots. For if this has been done only by *one* generation, the variety of a rounded root by that means produced immediately passes again into a root of a spindle or long conical form.

27. It remains now to recapitulate the contents of this paper in a few words, with such explanations as may be necessary.

In my method of cultivation, the ground is wholly covered with leaves, and consequently shaded, by reason of the nearness of the plants to each other; but, on the contrary, those roots which have been cultivated to feed cattle, are sown or planted at a much greater, and usually double that distance. This very necessary adumbration is maintained by taking care not to cut the leaves till the roots themselves are gathered. The access of light to the surface of the field, to the great injury of the formation and accumulation of the saccharine matter in the root, is not the only bad consequence; there is another noxious effect, namely, that it promotes the drying of the ground in hot seasons, which is always very detrimental. Moreover, the natural growth of the roots is, by this means, necessarily disturbed, and cannot be productive of good consequences. Again, if the earth be not removed from the plant, the action of light on the top of the root is checked; and the separation of the ground, which is done in many places, tends only to increase the size of the root. Lastly, by producing the roots from seed which has been sown on the spot where the plant is to remain, this advantage is obtained, that the root acquires a spindle shape, penetrates deeper into the ground, and therefore acquires more sweetness, for it is always sweeter in the lower than upper parts. To conclude, the projection out of one part of the root out of the ground, which obtains in the growth of almost all roots, and especially of the *runkelrübe*, is by this method prevented. The cause of this prominence consists in this: that the earth—which it is impossible to avoid—is loosened on the spot where the plant is inserted. It sinks again on becoming firm, and hence the upper part of the root becomes prominent. Another and the principal cause of this effect arises from the circumstance, that on transplanting the plant, either the point of the root is taken off, or, on account of its tenderness, unintentionally injured. Nor does it again acquire an upright position; so that, for all these reasons, it cannot proceed in growing downwards perpendicularly. Whence the root does not continue in the slender conical form which it had, and would have preserved, if it had not been transplanted; it then forms a more roundish and nodular root, which in the progress of its vegetation (not being able to spread

spread downwards from the too great resistance of the soil) rises upwards, and protuberates more or less from the earth, in proportion to its stronger or weaker growth. Thus circumstanced, the prominent part becomes so modified in its constituent parts by the action of light, that it not only yields less of sugar, but also adulterates the saccharine matter, copiously contained in the lower part, with so many noxious principles, that the preparation of sugar from the root is rendered very difficult, and sometimes even impossible.

28. From these considerations we may deduce the reason why the manufacturing of sugar in our country has not succeeded before, though it might be carried on in the large way with a real and lasting profit, notwithstanding the low prices of the raw sugar from India. The cause consists merely in the want of an acquaintance with the various modifications under which the *runkelrübe* ought to be cultivated, if intended for the manufactory of sugar to a decided advantage, as has been several times mentioned. Before this time, and before the discovery of the means of increasing the saccharine matter in this kind of root, enquirers could only prepare sugar from the *runkelrübe* and the various species of the *beta vulgaris* *Linnaei* with a scientific view, rather than for purposes of public utility.

To ascertain the proper method of culture, by which this root might be produced abounding in sugar, and deficient in mucilage, was therefore the only expedient to obtain the latter advantage to the public. And it is only from such a root, highly abundant in saccharine contents, that raw sugar can profitably be made, and freed from its mucilaginous and other parts, by means of a very simple, and not in the least expensive, process, of pressure and crystallization; which I shall hereafter communicate.

III.

On the Decomposition of the Acid of Borax, or Sedative Salt. By LAURENCE DE CRELL, M.D. F.R.S. London and Edinburgh, and M.R.I.A. Translated from the German.

(Concluded from page 257, Vol. III.)

EXPER. XXXIV. To obviate the objection, that sedative salt alone would perhaps deflagrate with melted nitre, I made that experiment also, but in vain. Not the smallest deflagration took place, even when both were melted together for many hours.

Exper. XXXV. Another objection may be made, namely, that in distilling the muriatic acid from manganese, part of the latter had passed over with the acid; and, in the frequent distillations of the sedative salt, had been deposited upon it, and thus deflagrated. But, on throwing fresh pulverized or solid manganese, either such as is usually sold, or quite pure, heated to redness, into melted nitre, not the smallest deflagration took place.

Exper. XXXVI. to L. Instead of the interrupted heat used in the foregoing experiments, I now exposed half an ounce of the salt, with three ounces of the oxygenated muriatic acid, to a continued heat, of between 200° and 300° of Fahrenheit. The fluid had nearly evaporated in

in twenty-four hours. I changed the phial, towards the close of the operation, for another, that the former might be gently heated, and the fluid, by that means, be poured back, with the greater safety, upon the warm salt, through the tube of the retort. In this manner, during an uninterrupted fire of fourteen days, the acid was fourteen times distilled, and returned upon the salt. On the third day, yellow spots appeared. On the fourth, some particles of oil or fat were discovered, swimming on the surface of the fluid in the phial; which particles, after cooling and emptying the phial, adhered to its sides, so as to obscure its transparency. More or less of these oily particles were discovered in every successive operation; and the oily matter, adhering to the inside of the glass, increased considerably.

Exper. LI. When the fluid was distilled, the receiver was changed, and the fire increased. A considerable quantity of sublimate was obtained, pretty white in colour, as was likewise the surface of the mass of salt at the bottom of the retort; but lower down, it was almost of a light ash-grey. After the sublimate ceased to arise, I diminished the fire.

Exper. LII. Upon the mass of the former experiment, I poured the fluid obtained by *Exper. XLIX.* and continued a gentle digestion. I very soon perceived something rising towards the surface, and swimming upon it: after some hours, it appeared to be a thick wrinkled skin, like fat, or a skin of mould, increasing in size, until it covered the whole surface. White spots of sublimate appeared upon it, but it did not sink. It assumed gradually a fine lemon colour; and some yellow matter, though not in large quantity, ascended the sides of the retort. The fluid having been gently distilled, and the receiver changed, I placed the retort in an open fire; on which more sublimate soon appeared; but, not long after, it all vanished, and the retort lost its transparency. The mass contained in it began to rise, first gently, and then violently, especially in the centre, in large frothy bubbles. The distillation was finished, after obtaining one dram of fluid, and when the frothy bubbling had ceased. The retort being broken, that part where the bubbling had been strongest was found to be black; the upper surface being covered with a thin greyish matter, under which a solid, compact, and almost vitrified substance appeared. Upon this I poured water, and dissolved it in the usual manner; filtered it, let it evaporate, and treated it as described above, *Exper. XXII.—XXX.*

Exper. LIII. I obtained a white salt, *a*, and some coal, *b* (which deflagrated briskly with nitre), in nearly the same proportions as throughout the series of experiments described from *Exper. XXII.* to *XXXIII.* which I will not repeat, on account of the little variety observed in them; one of them, however, deserves to be distinguished from the rest.

Exper. LIV. I put six grains of the coal, *b* (of *Exper. LIII.*), in three drams of common muriatic acid, and digested them for two days, till the acid had gradually evaporated. I then added one dram of the same acid, with one scruple of nitric acid, and, when they had evaporated, boiled the residuum full half an hour in distilled water. By this process I obtained a red solution; and having saturated it with mild alkali, a sort of skin rose to the surface, with

some small pieces of a fat slippery substance, *a*. A considerable quantity of loose earth was also precipitated, of a light brown colour.

Exper. LV. On throwing the floating pieces, *a* (*Exper. LIV.*), into a solution of caustic alkali, they dissolved; the solution had a reddish-brown colour.

Exper. LVI. With the same solution of caustic alkali I covered the light brown earth of *Exper. LIV.* As the solution changed its colour to a reddish brown, the earth gradually became perfectly white.

Exper. LVII. To observe the affinity of other acids to the sedative salt, I poured six drams of nitrous acid upon two drams of the salt, with ten drams of the forementioned oxygenated muriatic acid; digested the mixture, and distilled it, in twenty-four hours, with a gentle heat. Upon the fluid swam a white compact substance, and some small particles of the same kind lay at the bottom, which however rose on the application of heat, and swam about with the rest.

Exper. LVIII. to LXIII. I poured the whole distillation back upon the salt, and, by means of a digesting heat, again drew off a fluid, which appeared covered with a thin fat skin. I then poured the fluid back, distilled it again, and thus repeated the process three times more. No phenomenon particularly remarkable appeared, except that the thin fat skin grew more inconsiderable, and at last seemed almost to vanish.

Exper. LXIV. The salt separated from the fluid by the gentle distillation in *Exper. LXIII.* emitted, now, by the force of additional heat, dark red vapours, as is usual in strong nitrous acid. When the distillation was at an end, the retort was exposed to an open fire; but, during this operation, no black matter appeared; nor was any coal separated from the mass, upon dissolving it in distilled water*.

Exper. LXV. I now tried the effect of a mixture of four drams of strong vitriolic acid and twelve drams of the muriatic acid, repeating the usual digestion and distillation six times. I will pass over other circumstances, and only mention, that after the sixth distillation of the fluid, a stronger heat, and at length an open fire, was applied; but hardly any fluid was produced, though the fire was so violent, that the whole mass appeared to be melted down into one uniform compact substance.

Exper. LXVI. The vessels having cooled, the mass was of a light milky colour throughout, without the least mixture of brown or black, or any other indication of coal†. Being some time exposed to the air, it became moist, and for a long time attracted much water, which I caused to run off. At last it remained pretty dry; but the mass seemed to have diminished, by at least one-fourth part.

Here I will stop, for the present, in the description of my experiments, which sufficiently tend to prove, in a general way, the decomposition of sedative salt, and to show, that one of

* Here the nitrous acid seemed to destroy, and carry off, the inflammable matter, sooner than it could become coal; as it had before occasioned the oily and fat substance to vanish, in the beginning of this experiment.

† Perhaps here also the remark contained in the former note holds good: yet I am rather of opinion, that the vitriolic acid did not operate with sufficient strength to separate the component parts.

its component parts is inflammable matter, which may be converted into coal. I obtained of true coal (mixed with some earth, *Exper.* XXXII. and LIV.), according to the above-described experiments (*Exper.* XXII. XXVI.—XXX.), thirty grains and three quarters, in the whole; and by other experiments, often repeated, in general, one grain and a half, more or less. Every other substance liable to be changed into coal (as gum, tartar, sugar, &c.) suffers this change by a gentle heat, and deflagrates with nitre, in the degree of heat necessary to melt the former. But sedative salt can bear a red heat for many hours, without shewing any signs of becoming coal, of burning, or of deflagration. Astonishing phenomenon! What menstruum preserves it so securely against the assault of force, in a dissolved state, and yet suffers itself to be separated from it by more gentle means? What power exists here, to protect the inflammable particles (which afterwards turn to coal) so effectually against a degree of heat which nothing else can resist? Of what nature is the salt obtained in conjunction with the coal? These are all questions which excite great interest, but which are not easily answered. How far I have been successful in resolving them, some subsequent essays will show; which I shall have the honour to lay before the Royal Society, as soon as I shall have sufficiently repeated the experiments I have already made.

IV.

Observations on the Means by which the Mountains in the Cevennes are rendered fertile.

By CIT. CHAPTAL.

INDUSTRY is the child of want: this axiom, of which the truth is established on the experience of every age and climate, directs our search after prodigies of agriculture to those places only which appear to be absolutely deprived of natural advantages. In no place has this truth received a more striking degree of confirmation than in the dry and barren chain of mountains called the Cevennes. They are almost entirely formed of steep rocks; but the power of man has successively converted them into fertile lands; and this soil, which in past ages would not have afforded subsistence for one family of savages, does, at this moment, support two or three hundred thousand inhabitants. In this district, every thing is the product of art; and here it is that we may, with the greatest effect, study the creative powers of human industry. I shall here relate the means by which these changes have been made.

I shall confine myself at present to the description of two processes, which are still daily practised, and which may hereafter be adopted with advantage in many parts of France.

It is a well-known fact that the waters which flow down the sides of a mountain carry the earth along with them, and wear furrows or ravins of a greater or less depth, according to the hardness of the rock, and steepness of the descent. These two effects are constant and inevitable. By a series of these progressive degradations, the hardest rock is laid bare; deep

ravins are cut in its face, and every resource of which the cultivator might avail himself is utterly destroyed. The inhabitants of the Cevennes have found means to correct this double effect of the waters; and to restore, by processes no less simple than ingenious, the grounds which had been lost.

I shall first describe the method of filling the ravins, and converting them into fertile land; and afterwards speak of the method of covering the bare flanks of the mountains with vegetable mould.

The first Process.

To fill up a ravin, they begin by raising, at the very foot of the mountain, a wall of loose stones quite across the ravin, and two or three metres (or yards) in height, towards the middle, according to the depth of the ravin itself. This wall forms a kind of dyke, which opposes its flank to the current of the waters, and suffers them to pass through while they are clear; but when, after a storm or heavy shower of rain, they become turbid, and bring down earth and stones, these substances are deposited against the wall while the water escapes between the stones nearly pure. By the continuance of this process, the triangular space above the wall at length becomes filled.

At the other extremity of this plat of newformed ground, another wall is then built like the first, which in the same manner detains the earth and vegetable mould, and forms a second piece of ground. By a succession of similar operations, other platforms are produced; and by this ingenious process, the ravin is converted, to the very top of the mountains, into a number of platforms of good ground, forming steps one above the other. Under these circumstances, the waters no longer rush in destructive torrents down the sides of the mountains, but flow gently along the level ground, or are filtered through the porous earth deposited against the walls of support. The mountain, which formerly presented a scene of desolation in every point of view, is thus made to exhibit amphitheatres of vegetable ground capable of the richest cultivation.

Thus far the labours of the husbandman are employed in the conquest of natural difficulties; but this being accomplished, he directs his attention to obtain the products of the ground. In the former instance, he claims our admiration; in the latter, we feel an emotion of gratitude for benefits conferred. He plants the vine against the upper part of the wall, and causes it to creep along the external surface, that it may not uselessly employ a space destined to other purposes. On these small platforms he plants mulberry-trees; he cultivates Indian-corn, potatoes, every kind of grain; and varies his culture to the greatest advantage on this virgin soil, which is well watered, and in general of the highest fertility. These vines, these trees, and other vegetable products, give firmness to the ground, and bid defiance to the floods of subsequent times. Seldom indeed does it happen that this valuable work of human ability is considerably damaged by the fury of the tempest.

The

The second Process.

The industry of the inhabitant of the Cevennes is no less surprising, when his exertions are directed to give fertility to the slope of a calcareous mountain. Most of these mountains are formed by beds of stone about a demi-metre, or half a yard, in thickness. These different strata form shelves one above another, according to the inclination of the mountain. But the cultivator gives an equal width to all these stages by breaking away the rock, the fragments of which he employs in constructing a low wall on the edge of the platform itself. He fills this cavity with a bed of vegetable earth taken out of the clefts of the rock, or conveyed upon his back from the very foot of the mountain, where it has been gradually deposited by the waters. In this manner, after unremitted labour, the side of the mountain becomes covered with low walls parallel to each other, which confine beds of vegetable earth from one to three yards in width.

It often happens that these beds of earth are carried away, and the walls overset, in consequence of a violent wind, or storm of rain. On these occasions, the determined spirit of the cultivator is shewn in repairing the mischief. The life of the inhabitant of the Cevennes is one continued struggle against the elements, which seem to conspire against his efforts. I was acquainted at St. Jean de Gardonnenque, with Cit. Pestre, an industrious man, an enlightened cultivator, and skilful physician. This man, at the first threatenings of a storm, clothed himself in a long garment of oil-cloth, with an enormous hat of tinned iron, firmly fixed by means of straps. Thus defended, he hastened into the midst of his possessions, where alone, with a mattock in his hand, he directed the water to the feet of his trees, and collected the surplus in cavities which he had formed in the rock itself. By these laborious exertions he constantly prevented inundations, and procured water for his grounds at such times as the burning heats rendered them necessary. His neighbours, who, as usual in such cases, at first derided his extraordinary solicitude, were constrained to admire his industry, and envy his gains. They have all admitted to me, that by this labour, of which few of them are capable, he quadrupled the usual product of his grounds. Such instances of prodigious agricultural exertion are not unfrequent in the Cevennes; but my present object being to speak of general methods, it is sufficient that I have described the ingenious method of giving fertility to a mountain. We cannot avoid feeling a sentiment of admiration, and even personal vanity, when we consider one of these mountains converted by the hands of man from a state of absolute barrenness to a high degree of fertility, and covered from its base to its summit with trees, fruits, grain, and other useful productions.

He who doubts the extensive powers of labour and industry in the affairs of agriculture, needs only to visit the Cevennes to become convinced of his error*.

* This practice of converting the sides of mountains into platforms for the purposes of agriculture is common in China, and gives a singular aspect to the country around Canton. But it did not appear to me that the practice was directed to the same object, namely, that of giving fertility to barren mountains. On the contrary, these last were left in a state of nature.—N.

V.

Account of certain Experiments and Inferences respecting the Combustion of the Diamond, and the Nature of its Composition. By CITIZEN GUYTON.*

THAT the diamond is a combustible body was long ago conjectured by Newton, from its strong attraction to the rays of light, and this truth has since been verified by many experiments. With regard to the nature of this gem, the first experiments of Guyton, published in 1785†, on its total combustion in fused nitre, seemed to shew that it was similar to charcoal, because it left an effervescent alkali; this suspicion was rendered still more probable by the examination by Lavoisier of the gas remaining after burning it in close vessels, which proved to be carbonic; and very lately Mr. Tennant has given a new verification of this important fact, by repeating the combustion of the diamond in a crucible of gold, as Guyton had pointed out in order to obtain a residue perfectly exempt from foreign matter‡.

There were nevertheless many reasons for doubting the perfect identity of the diamond and carbone. Independent of the extreme difference of their external characters, the author of the present communication stated several facts to the institute, which shew that the chemical habitudes of the two bodies are not consistent with this identity. For if the diamond were pure carbone, why does it not detonate with the oxygenated muriate of potash, and disoxygenate sulphur, arsenic, phosphorus, and such metals as are sufficiently fixed to determine its combination with oxygen? Why does it not form carburets? or conduct the electric fluid? It is known that the power of aggregation sometimes counterbalances the power of chemical affinity; but this does not happen when the bodies are sufficiently fixed, and the temperature sufficiently elevated to give effect to the weakest attractions of composition.

It remained, therefore, that some discoveries should be made to reconcile and explain facts apparently so discordant§. Our author judged that the explanation was to be sought by a diligent research into the phenomenon of the combustion of this substance, and he hopes that the narrative of his experiments made at the Polytechnic school, in conjunction with the citizens Clouet and Hatchette, in the fifth and sixth republican years, will shew that his expectations were not ill founded.

The first experiment was made on the 9th Fructidor, in the year 5 (August 26, 1797). An inverted vessel of white glass, 18.3 centimetres in diameter, and 5580 cubic centimetres (A. fig. 1. plate 14.) in capacity, was placed on the shelf of a trough containing mercury. Near this trough was fixed an air-pump B, to extract the common air by means of a re-

* Abridged from the communication read at the sitting of the first class of the French National Institute, 26 Prairial, in the year 7 (June 14, 1799); inserted in the *Annales de Chimie*, XXXI. 72.

† French edition of Bergmann's Treatises, vol. XII. p. 124.

‡ *Philos. Transf.* 1797; or this Journal, I. 177, 199.

§ Cit. Berthollet, in his Lectures at the Normal School, considers it still as a matter of doubt whether the diamond be crystallized carbone, or that principle combined with some other matter.

curved tube. On the other side was a pneumatic trough C, containing water, in which was a large pneumatic vessel D, communicating with the interior of A, by means of a tube properly bended. The communication could be regulated at pleasure by the cock E. On one side of the trough of mercury was a bar F F, to which a clamp G could be fixed at pleasure; and through the upper part of this clamp was passed a rod of hard wood H, which passed beneath the mercury, and rose again to afford a support for the diamond at I. By altering the situation of the clamp G, by elevating or depressing the rod H, or by turning this last on its axis, it was possible to place the extremity I in any required situation within the glass vessel A. At I was placed a small porcelain crucible, in which the diamond was placed. This crucible was provided with a stopper, which could be put in and out by means of a wire.

The diamond which was the subject of this experiment was an incomplete octahedron, of a dull water, inclining to yellowish-grey, somewhat rounded on the edges. It weighed exactly 142 milligrammes. In this situation of the apparatus, the crucible being closed, the pump was worked until the mercury rose to within less than one millimetre from the orifices of the communicating tubes, which terminated quite at the upper part of the vessel at A. The cock E was then opened, and oxygen gas, obtained from the oxygenated muriate of potash, and previously put into D, was passed into A. The first portions of this gas which mixed with the minute residue of common air at A, were drawn off by the air-pump, in order to exhaust the common air as much as possible; and at last the vessel A was filled with the oxygen gas, which might be considered as nearly pure.

The solar heat was applied to the diamond by means of the great lens belonging to the Polytechnic school, the diameter of which is 40.59 centimetres, and its focus 135.3. The effect of the sudden heat upon the glass vessel was moderated by occasionally intercepting the rays with paper for a few instants, after which the diamond being exposed to the focus for twenty minutes did not take fire. It appeared at first farinaceous, and afterwards perceptibly blackened on its surface, when examined through coloured glasses, while the focus was upon it. When the focus was intercepted by an opaque body, in order to examine the diamond more accurately, the diamond appeared to have undergone no alteration but that of having acquired a yellowish shade, perfectly resembling clear amber. At this period the experiment was discontinued, on account of the weather becoming obscure.

On the following day the solar focus was thrown upon the diamond, and at the end of one hour and fourteen minutes, the cone of light being intercepted, the diamond was seen distinctly; transparent and surrounded by a weak radiation. When it was cold its edges appeared to have been softened. A black point was visible; but the diamond had recovered its white colour by the loss of the yellow tinge it had acquired the day before.

The experiment could not be resumed till the 15th. On this day the weather was unfavourable, but nevertheless a slight scintillation appeared.—To the astonishment of the operators, the diamond did not continue its own combustion, which even metallic substances do when sufficiently heated in oxygen gas. This seemed the more remarkable on account of the contrary
having.

having been announced by Landriani. To determine whether the aggregation of the stone were the cause of this, another diamond was subjected to experiment, scarcely exceeding the eighteenth part of the former; but this gave no sign of inflammation, nor underwent the least change.

On the 23d the diamonds were taken out and carefully examined, when the larger was found to have lost 54 milligrammes, or 0,38 of its weight, having preserved its original form, though the angles were blunted, and its surface tarnished and full of small inequalities, which under the magnifier presented either pointed projections, or the sections of parallel plates. In several of the cavities were seen a greyish kind of spot; and, in particular, in a cavity somewhat larger than the rest, where the focal spot appears to have been most strongly active, there was a blackish streak not terminated, as if drawn externally, but fused, and penetrating by a degradation of colour into the body of the stone.

This diamond was preserved for inspection, and another of a clearer water than the former, of an octahedral form, and the weight of 200,1 milligrammes, was exposed to the focus of the great lens of Tschirnhausen, which is 32 French inches in diameter, and 73 inches in its focal length: but the focal length was shortened by the interposition of another lens. The first experiment scarcely exhibited any signs of combustion, and failed by the breaking of the receiver. In the following year, 6, it was therefore found expedient to resume the process with a large globe, in order that its surface might be at a sufficient distance from the denser part of the luminous cave. The globe made use of measured 28,63 centimetres in diameter, and its capacity was 12325 cubic centimetres; and, in order to determine the quantity of space occupied by the rising mercury, there was a piece of paper pasted on the outside, having marks thereon to express every 100 cubic centimetres, determined by pouring in water by a measure of that magnitude.

As it was impracticable to exclude the air from so large a vessel of glass, by means of the vast quantity of mercury it could contain, our operators had recourse to an ingenious expedient derived from the consideration that the specific gravity of oxygen gas is about one ninth part greater than that of common atmospheric air. It consisted in admitting the gas by a tube which reached from the neck (then turned upwards) to the lower part of the inside, while the common air was suffered to escape through another short tube in the cemented cork by which the orifice was closed. It may easily be imagined, that the oxygen did not displace the common air without mixing with it and becoming more or less contaminated with azote; for which reason it was necessary that the contents should be thus displaced for several successive times. By this treatment the last portions of gas extruded were found to be pure, and the vessel continued perfectly clean and dry. The authors give a detail of the methods by which the oxygen gas was obtained, and its purity ascertained.

Fig. 2 represents the globular vessel inverted over an iron mortar, containing mercury, and secured in its place between two pieces of wood, which formed a cavity to receive it. The diamond was previously secured in a small vessel of baked clay, by means of a ribband, which was afterwards slipped off when the vessel was inverted. There was a small tube of communication

nication through the cemented cork to the mercury, and through this tube a portion of the gas was drawn in order that the mercury might stand a small elevation within the vessel.

The diamond which had, by the former treatment, lost a small part of its weight, now weighed 199,9 milligrammes, or one 32d part less than a jeweller's carat. On the 5th of Fructidor, in the 6th year, the experiment was re-commenced by directing the focus of the great lens of the Institute upon this diamond, the value of the oxygen gas having been previously determined in cubic centimetres, and reduced to a known temperature and pressure.

The diamond at first exhibited a black spot at the angle immediately struck by the solar rays, after which it became entirely black and of a coaly appearance. An instant afterwards brilliant and, as it were, boiling specks were distinctly perceived on the black ground. The solar rays being then intercepted for a moment, it appeared of a transparent red; and afterwards, when a cloud obscured the sun, the stone was seen of a much clearer colour than at the commencement of the operation.

When the sun resumed its force the surface of the diamond assumed the appearance of metallic brilliancy. It was then perceptibly diminished, scarcely one fourth part remaining; its form oblong, without marked angles or edges, but still very white and of a beautiful transparency.

A slight crack appeared at the bottom of the vessel which supported it, but without any separation of parts.

At the commencement of the combustion a purplish cone was thought to be perceived rising from the support in the cone of solar rays; but this phenomenon, according to the writer, was merely an optical effect, depending on the position of the observer.

The apparatus was left in its place, without alteration, till the seventh, when the solar focus being again thrown upon the diamond, the same appearances were observed; namely, blackness at the surface, and brilliant boiling points, which disappeared and re-appeared, according to the power of the solar light. An appearance of metallic brilliancy was also seen, or rather a leaden colour, according to the expression made use of by one of the assistants to characterize this phenomenon.

The diamond was entirely consumed after twenty minutes exposure this second time. After a careful examination of the support, which was withdrawn, a solution of barytes was introduced above the mercury. It immediately became milky, while the volume of the gas was diminished. This barytic water was then, for the most part, withdrawn by means of the inverted bottle (fig. 3.) filled with mercury, which was raised into the globe by means of an iron stem, consisting of several pieces screwed together. A portion of the distilled water was also introduced to wash out the remaining precipitate; and this was afterwards added to the barytic water. It was necessary to add more of the barytes, in order to saturate a portion of carbonic acid, which was more in quantity than the operators expected. The valuation and reduction of these quantities, and the precautions made use of, are given in the Memoir. The result was, that instead of 28 parts of combustible substance, with 72 of the acidifying principle,

as observed in the combustion of charcoal, the proportion in the combustion of diamond was 17,88 of carbone, and 82,12 of oxygen.

The learned reporter proceeds to offer his remarks upon the theory of these facts. Though he could not doubt the facts, he, at first, felt a degree of repugnance to admit so considerable a difference in this combustible and charcoal, the latter of which contains little more than half the quantity of oxidable matter, which is capable of being burned at a temperature prodigiously below that which is required by the diamond. But, upon reflection, he soon observed that this is not the first example of acidifiable base, the first degree of oxidation of which is effected with extreme difficulty, though the subsequent acidification is very easy. 2. That many substances of the same genus, likewise, present these two characters; namely, a greater abundance of true carbone, accompanied with a greater resistance to inflammation. On these heads, he remarks how difficult it is to form a commencement of union of azote and oxygen in the direct way, and the high temperature it requires; whereas nitrous gas cannot be brought in contact with oxygen, without immediately passing to the acid state. Charcoal will therefore be, with regard to the carbonic acid; what nitrous gas is to the nitric acid; and the diamond will be to charcoal, what azote is to nitrous gas. It will not, therefore, appear surprizing that more oxygen is required to combine with a substance which has yet none of that principle, than with a substance which has taken up the quantity which is necessary for its first point of saturation.

The second consideration rests on facts no less conclusive. Plumbago is a carbonic combustible which does not burn at a very elevated temperature, or in nitre in fusion. It produces carbonic acid, and, like the diamond, is richer in combustible than charcoal itself. The incombustible coal, which was described by Guyton in the Dijon Memoirs for 1783, is a substance of the same kind, as is also the anthracolite; and the celebrated Klaproth has likewise established by experiments, that a fossil described by Widenman, under the name of incombustible coal, is of the same nature. The Kilkenny coal, described by Kirwan as possessing the metallic brilliancy, and refusing to burn, unless in a state of ignition, is also capable of decomposing 9,6 of nitre. All these are considered as true oxides of carbone, which, like charcoal, possess the property of conducting the electric fluid, of cementing iron, of depriving certain acidifiable bases of oxygen, but are not sufficiently oxidized to exert this divellent affinity at a low temperature.

No good account has yet been given, why certain animal and vegetable matters afford coals of such difficult incineration? Why charred pit-coal, known by the name of coke, though half burned in its preparation, is nevertheless so powerful in combustion? Why turf, the weakest of all fuel, acquires, by a good carbonization, the property of welding large pieces of iron better than charcoal? and why, lastly, charcoal itself, after exposure to very strong fire in a vessel impenetrable to the air, becomes to a certain point incombustible? The answer to all these questions is referred, by our author, to the theory he has developed; that these are all carbones at the first degree of oxidation, having been deprived of part of their oxygen by the elevated temperature to which they have been exposed. Hence he concludes,

cludes, that practical consequences will, no doubt, be deduced for the reduction of metals; the cementation of steel, which probably takes up only the oxide of carbone; since it is separated in this state*; for the incineration of the coaly residues of our analyses; for the carbonization of wood, pit-coal, and turf; and lastly, for the useful application of mineral coals, which, though difficult to be burned, are capable of affording a strong and lasting heat when mixed with a due proportion of other materials which burn more readily.

The general recapitulation of facts and inferences, in the words of the author, are as follow:

1. It is not merely by its colour, weight, hardness, transparency, and other sensible qualities, that the diamond differs from charcoal, as has hitherto been thought.
2. Neither does the constitution of the diamond depend simply on its state of aggregation.
3. Nor do the distinctive properties of charcoal depend on the two hundredth part of residue, which it leaves in the form of ashes, nor the small quantity of hydrogen which it contains.
4. But the most essential difference consists in the chemical properties.
5. The diamond is the pure combustible substance of this genus.
6. The product of its combustion, or combination with oxygen, carried to the point of saturation, is carbonic acid without residue.
7. Charcoal burns at a temperature which may be estimated at 188° of the centigrade thermometer †. The diamond does not burn but about pyrometric degrees, which, in the system of Wedgwood's scale, affords a difference between 188 and 2765.
8. Charcoal set on fire in oxygen gas, does itself maintain the temperature necessary for its combustion; but the combustion of the diamond ceases, as soon as it is no longer maintained by the heat of the furnace, or the concentration of the solar rays.
9. The diamond requires, for its complete combustion, a much greater quantity of oxygen than charcoal does, and likewise produces more carbonic acid. For one part of charcoal absorbs 2,527 of oxygen, and produces 3,575 of carbonic acid. But one part of diamond absorbs somewhat more than four of oxygen, and really produces five of carbonic acid.
10. There are substances which exist in an intermediate state of composition between the diamond and charcoal. These are plumbago, or native carburet of iron; the incombustible fossil coal; the carburet of alumine of Dolomieu; the anthracolite of Werner; the black matter united to iron in the state of cast-iron and steel; the coaly residues of difficult incineration; and charcoal itself unburned by the action of a strong heat, without the contact of air.
11. These substances, mixed or weakly combined with three or four hundred parts of

* It is a strong fact in favour of this theory, that the charcoal which has been used for the cementing process is rendered incapable of being again used for the same purpose. See Duhamel, in the *Encyclopédie Méthodique*, art. *Acier*.—N.

† *Dictionnaire de Chimie de l'Encyclopédie*, method. I. 714.

their weight of iron, or alumine; afford, by their combustion, carbonic acid like charcoal, and the diamond.

They approach charcoal by their colour, their lightness, and their opacity. Like that substance, they decompose water, cement iron, difoxigenate metals, sulphur, phosphorus, and arsenic; and, like charcoal, they conduct the electric fluid.

They approach the nature of the diamond in containing much more combustible matter than charcoal, which is manifested by their absorbing more oxygen, and producing more carbonic acid; in their decomposing more nitrous acid; in their refusing to burn, even in fused nitre, unless at a more elevated temperature; and by their combustion ceasing when that temperature is diminished.

They appear to differ from each of these bodies, by their property of producing the Galvanic irritation with zinc, as well as silver does; an effect which is not produced either with the diamond, or with charcoal.

12. The diamond, therefore, is pure carbon, the pure acidifiable basis of the carbonic acid.

Its combustion is effected in three terms, which require three different temperatures.

At the first and most elevated temperature, the diamond assumes a black leaden colour. This is a first degree of oxidation; it is the state of plumbago, and the anthracolite.

At the second temperature, which may be estimated at eighteen or twenty pyrometric degrees, there is a new, slow, and successive combination of oxygen. It is a progress of oxidation, which constitutes the habitual state of charcoal, or rather that in which it is found after the action of a strong heat in closed vessels has disengaged part of its oxygen.

Plumbago is, therefore, an oxide of the first degree; charcoal, an oxide of the second degree; and the carbonic acid is the product of the complete oxygenation of carbon.

Supposing, therefore, that we could operate with sufficient precision to take from the surface of the diamond the black matter gradually as it is formed, by withdrawing it suddenly each time, from the action of the solar heat, we should undoubtedly convert it into charcoal, or at least into plumbago, if the too rapid transition of the last degree of oxidation to oxygenation should not permit us to obtain it in this state.

13. Lastly, from these principles flow many important consequences to chemistry and the arts.

After this conclusion, it will, no doubt, be demanded, how it happens that the simple matter, the pure carbon, or diamond, is so scarce, while its compounds in different states are so abundantly dispersed. To dispel the astonishment of those who might consider this a ground of distrust, I shall remind them that the aluminous earth is likewise one of the commonest substances, though the adamantine spar, no less rare than the diamond, is nevertheless alumine; that iron exists every-where, under every form, excepting in the state of purity; for the existence of native iron is still doubtful. The wonder consists only in the opposition between facts and our opinions; it disappears in proportion as we discover and appropriate the powers of nature to produce the same effects.

Those who have never turned their attention to the philosophical sciences—at least, for the purpose

purpose of estimating their influence on public happiness—are disposed to consider such researches, as are not directed towards an object immediately related to some new enjoyment as a matter of vain curiosity. What would have been the astonishment of such men, if they had been told that researches into the nature of the diamond should one day afford truths leading to the happiest ameliorations in the practice of the most familiar arts, and the use of the commonest combustibles. Such, however, are the consequences which are promised by our better knowledge of the essential coaly principle in its different states.

VI.

On the apparently spontaneous Combustion of living Individuals of the human Species.

*By CIT. LAIR.**

WE find accounts, in several works, of the combustion of human individuals, which appear to be spontaneous. They have been reduced, in short, to a mass of pulverulent fatty matter, resembling ashes. These accidents have been accompanied by phenomena similar to those which are observed in the process of combustion, and this destruction has not been produced by the combustion of the surrounding bodies.

The author of this memoir has collected all the circumstances of this nature which he has found dispersed in different books, and has taken care to reject those which did not appear to him to be supported by respectable testimony.

These narratives are nine in number, taken from the Acts of Copenhagen, 1692; from the Annual Register, 1763 and 1775; Philosophical Transactions, 1744; Observations of Lecat, in the years 1725 and 1749; and from the Journal of Medicine for 1779 and 1783.

To these the author has added some others, related by persons still living at Caen, and on the testimony of a surgeon of that town, who drew up a verbal process, containing an account of the circumstances of an event of this kind.

Several members of the society, who were present at the reading of this memoir, and had travelled in the North, had frequently heard such accidents mentioned. Doctor Swediaur related some instances of porters at Warsaw, who, having drank abundantly of malt spirits, fell down in the street, with the smoke issuing out of their mouths; and the people came to their assistance, saying that they would take fire; to prevent which they made them drink a great quantity of milk, or used a more singular expedient, by causing them to swallow urine immediately on its evacuation.

However difficult it may be to give credit to such narratives, it is equally difficult to reject them entirely without refusing to admit the numerous testimonies of men for the most part worthy of credit, or attributing to them criminal views; when we reflect on the difficulty of

* Communicated to the Philomatic Society at Paris, and inserted in the Bulletin, Thermidor, an. 5. No. 29.

reducing the body of an animal to ashes, the time and quantity of wood this incineration would require; and especially when we take notice, as Cit. Lair has done, of the resemblance which exists between the circumstances which almost constantly precede and accompany these singular accidents. These circumstances may be reduced to the nine following facts.

1. The persons who have experienced these effects of combustion were generally much addicted to the drinking of extremely strong spirituous liquors. It has accordingly been remarked, that the inhabitants of the North are most subject to these accidents.

2. They were usually very fat.

3. This combustion has happened more frequently in women.

4. These women were old.

5. Their bodies did not appear to have been burned by a combustion perfectly spontaneous; but it appears that the fire had taken place in consequence of some very slight external cause, such as the fire of a taper, candle, or a pipe.

6. The extremities of their bodies, such as the legs, the hands, or the cranium, escaped the fire.

7. Water, instead of extinguishing the fire of the burning parts of the body, gave it more activity, as also takes place in fat that is burnt.

8. The fire very slightly damaged, and, in many instances, did not injure, the combustible objects which were in contact with the body at the moment it was burning.

9. The combustion of these bodies left a residue of oily and fetid ashes, with a greasy foot of a very penetrating and disagreeable smell.

The author afterwards endeavours to ascertain the cause of so surprizing a phenomenon. We shall not (say the editors) follow him in the hypotheses he advances, as the facts are not sufficiently numerous nor well known to establish a satisfactory theory concerning the spontaneous combustion of human bodies. We shall content ourselves with remarking, that the author appears to attribute this combustion to a particular state of the fat produced by spirituous liquors, and he founds his opinion principally on the persons being very corpulent who have fallen victims to these accidents, and on the observation that has been made, that the parts which were not so fat, such as the extremities and the head, have escaped. And, lastly, he supports his theory by the well-known fact of the spontaneous combustion of a mixture of animal foot and lintseed oil, which is a mixture similar to that of a fat body containing charcoal in a very minute state of division.

VII.

*Remarks on the Conflagration of the Odeon. By B. G. SAGE, Professor and Director of the first School of Mines.**

THE effects of conflagration depend upon the degree of fire; and this last depends upon the quantity and nature of the combustible matters. A fire is active, rapid, and unextinguish-

* J. de Phys. v. 534.

able, if the wood happens to be resinous, or if it be found to contain oil; for the fire partly converts them into inflammable air, which immediately, on the access of the atmospheric air, becomes inflamed, and cannot be extinguished by water, which only serves to extend and direct the flames. Hence it happens that water can have no effect on such a conflagration, till the wood becomes converted into charcoal.

It is said that the fire of the Odeon (at Paris) was first discovered in a room near the curtain of the theatre, where it communicated to the decorations and deal frames which support them. The great quantity of air which was contained in the hall of the Odeon served, for a considerable time, to maintain the fire which had decomposed a considerable part of the wood and oil with which it was incorporated, and reduced itself into smoke and inflammable air, which entirely filled the hall. As soon as the atmospheric air was introduced it mixed with the inflammable gas, and exploded. At that instant the roof of the Odeon fell in, and the flames burst forth in all parts at once, because the inflammable air occupied the whole capacity of the hall.

The sulphur, which continued to burn in several parts of the Odeon three days after the fire, gave rise to a notion that the fire had been caused by malevolent incendiaries. I was myself greatly surprized at the quantity of sulphur which I saw in different parts; but, on examining it attentively, I discovered that it was perceivable only where there had been a great number of laths, timber, and burnt plaster, as in the corridors, the front of the peristyle of the Odeon, which formed four stories, and near the situation of the two staircases of wood which were erected near the middle of the hall.

I observed that the sulphur sublimed into a yellow dust, and that some scorixæ had crystalized; but the greatest quantity appeared to be in the state of calcareous liver of sulphur, blueish, friable, which took fire with decrepitation as soon as it came in contact with the air. It also emitted the smell of decomposed liver of sulphur; which confirmed me in the opinion that it was calcareous pyrophore, formed by the decomposition of plaster, which (as is well known) is itself composed of sulphuric acid combined with calcareous earth. This acid, by the assistance of the heat, united with the inflammable principle of the charcoal, and formed the sulphur which was found in the state of pyrophorus or carbonated calcareous liver of sulphur in the remains of the fire of the Odeon.

I shewed, about twenty years ago, that the pyrophore afforded by the coal of turf is owing to a portion of selenite or plaster-stone contained in the turfs of France. During the carbonization of these turfs, the sulphuric acid of the selenite *, combining with the inflammable principle of the coal of the turf, forms sulphur and a calcareous pyrophore.

On visiting the ruins of the Odeon, I found Monette, the architect of the department and of the Central Bureau, who informed me, that he had observed, in the ruins of two different theatres destroyed by fire, the same smell of the liver of sulphur, and the same effects as took place at the Odeon.

* The reader may perceive that M. Sage does not follow the new system of chemistry.

This production of sulphur by the calcination of plaster, in the large way, is a constant fact, similar to that which I discovered about twenty years since, and is confirmed by this unfortunate event.

State on the substances which were found after the fire of the Odeon.

A mass of calcinated iron mixed with slate and brick partly vitrified.

Hexagonal tiles and bricks changed by the fire, and mixed with black porous scorix and an earthy yellow friable matter.

A blackish martial scoria tuberculated and brilliant in its fracture.

Cellular martial scorix of a reddish-brown colour, covered with a kind of white compact frit.

Slates partly vitrified.

Calcareous and gypseous earth impregnated with pyrophorus and coaly liver of sulphur, which gave it a blueish colour; this earth, though penetrated with water, being taken from under the ruins, emitted a smell of decomposed liver of sulphur, and acquired a tinge of a violet from the combustion of the sulphur.

This earth had a slight styptic taste.

Exposed to the flame of a taper the sulphur which it contained burned very visibly.

A brick which had upon its top and bottom some of this earth, coloured by the pyrophorus, had been ignited to such a degree as to become vitrified.

Plaster partly decomposed penetrated with liver of sulphur, which manifested itself when the nitric acid was poured upon it. A strong effervescence was occasioned by the calcareous earth contained in a disengaged state.

An hexagonal tile covered with a yellowish glass of lead: this tile was also impregnated with liver of sulphur, as was observed by the application of nitric acid.

Slates partly vitrified and agglutinated, which had assumed a reddish colour.

VIII.

*On the chemical Action of different Metals on each other at the common Temperature of the Atmosphere. By CIT. FABRONI.**

THE peculiar sensation discovered by Sultzer, and which is manifested on the tongue when touched by two metals in mutual contact, though none is excited, when they are applied separately to this organ, is ranked among the galvanic phenomena. Cit. Fabroni, far from attributing these effects to an almost unknown agent, such as the electrical fluid, is of opinion that they depend on a chemical operation, in the same manner as the sensation of taste most probably does. He endeavours to prove this by a number of observations and experiments.

* Communicated to the Philom. Society at Paris, Bulletin, Thermidor VII. No. 29.

He has remarked that several metals, such as mercury, tin, and lead, preserve their metallic lustre as long as they are pure; but that their alloys are quickly tarnished and oxidized; and that the mere contact of two different metals appears to hasten their oxidation. In this manner the alloy employed to folder the plates of copper which cover the observatory of Florence was soon changed into a white oxide, at the place of its close contact with the copper, &c. He thinks, that in this case the metals have a mutual action on each other; and that this action, though more efficacious and perceptible when the aggregate attraction of the metals is destroyed by fusion, does not the less exist in solid metals when they touch each other.

If we attribute to a particular fluid, not galvanic, or to the electric matter, those effects which with the quickness of lightning are produced on the tongue by two metals brought into contact, it is because we do not recollect that the chemical action is also exerted between bodies with the utmost rapidity. The signs of electricity which are sometimes observed when two metals are separated from contact, are rather the consequence of that action than the cause. For we know that most chemical operations change the electric equilibrium of bodies, and consequently must produce the phenomena of electricity. And hence, without entirely excluding electricity from all the facts relating to galvanism, Cit. Fabroni thinks this fluid has no part in the sensation experienced on touching the tongue with the two metals. This action of the metals in contact is proved by the following experiment:—Cit. Fabroni placed different pieces of metal in glasses full of water; one in each glass. In other glasses he put two different pieces of metal, but he separated the metals by a plate of glass. In a third set of glasses he also put two pieces of different metals; but so as to touch each other. He did not observe any alteration in the metal in the two first sets of glasses; but the most oxidable metals of the third set became loaded with oxide, which was considerably increased in a few days, and the metallic pieces adhered together very strongly. The quantity of caloric disengaged during these combustions is too small to be measured; but we may perceive the light which proceeds from it if the eye itself be made an instrument in the experiment, by holding a piece of silver in the mouth, and applying a piece of tin to the ball of the eye. As soon as the two metals come in contact, a faint but distinct light appears, which vanishes in a few moments, because the eye accustoms itself to this weak sensation; but it might be renewed on passing the metal first over the opaque, and then over the transparent cornea. The author attributes to a convulsive sensation, the flash of lightning which some persons imagine they see at the moment the two metals touch, when one is applied on the tongue and the other between the gums and upper lip.

The presence of air is required for the oxidation of the two metals in contact beneath the water. Cit. Fabroni thinks that the air is of use in these circumstances, to add oxygen to the water in sufficient quantity to be received by the metal, as silver is added to gold in the parting assay.

If a piece of silver and a piece of tin are put so as to touch each other in water, in a vessel

of flint-glass, hermetically closed, the pewter is oxidized, but the oxide of lead of the flint-glass is decomposed, and the glass becomes black.

The philosophers who have attributed these phenomena to electricity, assert, in support of their opinion, that they take place when the metals are joined by a considerably long chain. Cit. Fabroni has determined the limits of this chain at 6 or 7 metres. Beyond that distance these phenomena are no longer perceptible; on the contrary, the electrical fluid is propagated at unlimited distances.

If the phenomena in the experiments of Sultzer be really produced by electricity, they should take place with all metals, what relation soever these metals might otherwise have to each other. Cit. Fabroni mentions a great number of these combinations, in which no effect was produced, and of other combinations of the same metals which have produced very distinct sensations. Thus, if silver be placed on the eye, gold on the tongue, and they are united by means of copper, the sensation is extremely slight; but, on the contrary, it is very evident, if iron touches the eye, silver the tongue, and copper be used to form the communication.

With regard to the hydrogen of the decomposed water, the author of this Memoir thinks it may also be absorbed by the metal. He even considers the octahedral crystals which he has observed on the surfaces of the pieces of tin he employed in his experiments as the hydrogenated oxide of tin.

"It is evident from these results which I have obtained by the simple contact of metals in water," says Cit. Fabroni, "that is to say, the oxide and saline crystals, that the operation is chemical, and that we ought to attribute the sensations which are felt on the eye and tongue to a chemical cause. It therefore appears probable to me, that it is to these new compounds and their elements we owe that mysterious stimulus which produces the convulsive motion of the animal fibres in a great part, at least, of the galvanic phenomena."

IX.

On the Harp of Eolus. By MATTHEW YOUNG, B. D.

TO MR. NICHOLSON.

SIR,

OBSERVING a passage in a note at p. 12. of your second volume, respecting the harp of Eolus, in which you offer some conjectures respecting its mode of operation, I have thought it might be acceptable to you and your readers to point out some good experiments on that instrument. They are to be found in "An Enquiry into the principal Phenomena of Sound and musical Strings, by Matthew Young, B.D. Trinity College, Dublin." The work is an octavo of 203 pages, and was printed in London in 1784. I transmit you a copy of as much

much as relates to the Eolian harp, which, from the confined circulation of such works as the original, must be known to few of your readers.

I am, Sir,
Your obliged,

September 8, 1799.

P. P.

Of the Harp of Eolus.

THIS pleasing instrument, which has been related by some to be a modern discovery, was, in truth, the invention of Kircher, who has treated largely of it in his *Phonurgia*. It is an instrument so universally known, that it may well be presumed unnecessary to give an account either of its construction or the manner of using it.

To remove all uncertainty in the order of the notes in the lyre, I took off all the strings but one; and, on placing the instrument in a due position, was surprized to hear a great variety of notes, and frequently such as were not produced by any aliquot part of the string; often too I heard a chord of two or three notes from this single string. From observing these phenomena, they appeared to me so very complex and extraordinary, that I despaired of being able to account for them on the principle of aliquot parts. However, on a more minute enquiry, they all appeared to flow from it naturally and with ease.

But before we proceed to examine the phenomena, let us consider what will be the effect of a current of air rushing against a stretched elastic fibre. The particles which strike against the middle point of the string will move the whole string from its rectilinear position; and as no blast continues exactly of the same strength for any considerable time, although it be able to remove the string from its rectilinear position, yet, unless it be too rapid and violent, it will not be able to keep it bent; the fibre will, therefore, by its elasticity, return to its former position, and by its acquired velocity pass it on the other side, and so continue to vibrate and excite pulses in the air, which will produce the tone of the entire string. But if the current of air be too strong and rapid when the string is bent from the rectilinear position, it will not be able to recover it, but will continue bent and bellying like the cordage of a ship in a brisk gale. However, though the whole string cannot perform its vibrations, the subordinate aliquot parts may, which will be of different lengths in different cases, according to the rapidity of the blast. Thus when the velocity of the current of air increases so as to prevent the vibration of the whole string, those particles which strike against the middle points of the halves of the string agitate those halves, as in the case of sympathetic and secondary tones; and as these halves vibrate in half the time of the whole string, though the blast may be too rapid to admit of the vibration of the whole, yet it can have no more effect in preventing the motion of the halves than it would have on the whole string were its tension quadruple; for the times of vibrations in strings in different lengths, and agreeing in other circumstances, are directly as the lengths; and in strings differing in tension and agreeing in other circumstances, inversely as the square roots of the tensions (*see Smith*

or *Malcolm*): and, therefore, their vibrations may become strong enough to excite such pulses as will affect the drum of the ear: and the like may be said of other aliquot divisions of the string. In the same manner as standing corn is bent by a blast of wind, and if the wind be sufficiently rapid it will have repeated its blast before the stem of corn can recover its perpendicular position, and therefore will keep it bent: but if it decays in rapidity or strength, the stem of corn will have time to perform a vibration before it is again impelled; and thus it will appear to wave backwards and forwards by the impulse of the wind. Those particles which strike against such points of the string as are not in the middle of aliquot parts, will interrupt and counteract each other's vibrations, as in the case of sympathetic and secondary tones, and therefore will not produce a sensible effect. That we may be more fully persuaded of the truth of these principles, I shall here set down the order of the Eolian notes as accurately as a good ear could discover.

Observation I. The original note of the string being the grave fifteenth to low F on the violin, the Eolian notes as given in the annexed page * were distinctly perceived, and nearly in the same order in which they are set down.

From the table of proportions in Smith's Harmonics, p. 10, we may see that these notes were produced by such aliquot parts of the string as are denoted by the fractional indexes, which are written over them agreeable to the theory laid down.

Observation II. While some of these notes were sounding I applied an obstacle indifferently to any point, which divided the string into such aliquot parts as would produce these notes, and the Eolian note was not interrupted: but if I placed it in any other part, the tone was instantly extinguished. This evidently shews that the entire string is, in fact, resolved into such parts, as, from the preceding chain of reasoning, we should have been induced to prescribe for it.

Observation III. I applied an obstacle slightly against the string, so as that its distance from the extremity should be an aliquot part of the whole; and the Eolian note was that which would be produced by such an aliquot part; thus we may in general predetermine what note the harp shall sound. But this effect will not invariably take place; because, though the

* The engraved table of notes consists of three sets of lines, with the cliff G on the second line from the bottom, as usual. In these the notes are written, and above each note the fraction which expresses the division of the string. As all these fractions have 1 for their numerator. I shall here give the letters expressing the notes, and the denominator or number denoting the subdivision:—Middle C 6;—lower F 4;—mid. A 5;—upper E, flat, 7, nearly;—mid. C 6;—mid. A 5;—up. D 7, nearly;—up. F 8;—up. D 7, nearly;—mid. C 6;—mid. A 5;—up. D 7, nearly;—up. F 8;—up. F 8, with up. D 7, nearly;—mid. C 6;—up. D 7, nearly;—up. E, flat, 7, nearly;—up. E 7, nearly;—up. F 8;—up. A 10;—up. G 9;—up. F 8;—mid. C 6, with up. E, flat, 7, nearly;—mid. C 6;—low F 4, with mid. A 5;—mid. A 5;—up. E, flat, 7, nearly;—mid. A 5;—up. E, flat, 7, nearly;—mid. C 6;—up. F 8;—up. G 9;—up. F 8;—up. E sliding to up. E, flat, 7, nearly, and to up. D;—mid. C 6;—up. F 8;—up. E, flat, 7, with mid. C 6;—mid. C 6;—up. E, flat, 7, nearly;—mid. A 5, with mid. C 6, and up. E, flat, 7, nearly;—up. E, flat, 7, with up. F 8;—up. F 8;—up. B 11, nearly;—up. A 10;—up. F 8, with up. G 9;—up. E flat, sliding to up. D;—up. C 6;—up. D, 7, with up. F 8, &c. &c.

obstacle may determine the string to resolve itself into such aliquot parts rather than any others, yet the blast may be too strong, or too weak to admit of such a part vibrating with sufficient strength to produce a sound; however, if any note be produced in this case, it must either be that of this very aliquot part, or of some of its own aliquot divisions; for the obstacle must necessarily determine one of the intersections of the equal indentures.

Observation IV. When the blast rises or falls, we find the tone also gradually rise or fall: because, as the blast rises, it grows too strong to admit of the vibrations of the longer aliquot parts; the vibrations of the short aliquot parts, therefore, will predominate, and will gradually shorten, as the blast rises in strength. But in cases of sudden variations in the strength of the blast, there will be also sudden transitions in the tones.

Observation V. We sometimes hear a chord consisting of two or three Eolian notes; because the blast, which is of such a degree of strength as to admit of the vibrations of certain aliquot parts, may also admit of the vibrations of other parts, if they be not very different in length; for their vibrations will be performed in times not very different. But if the length of these parts, and consequently their times of vibration, be very different, the blast that admits of the vibration of the one will prevent that of the other. Accordingly, in looking over the foregoing table, we find that the chords consist of those notes which are produced by such different aliquot parts as are least unequal: thus, one chord consists of C and E, which notes are produced by one sixth and one seventh of the string. Another chord consists of F and A, which are produced by one fourth and one fifth of the string. Another consists of A, C, and E, which notes are produced by one fifth, one sixth, and one seventh parts of the string.

It is also worthy of observation, that in long strings we never hear the original note and its octave at the same time; because, though they are the next aliquot parts, yet their difference is so great, that the blast which admits of the vibration of one of them, will obstruct and prevent the other. It is only in the higher divisions of the string that the chords are heard at all; and the slacker the note, the more frequent are the chords, for the reasons assigned above, namely, because the different aliquot parts, in such cases, approach nearer to equality.

Observation VI. Eolian tones are often heard, which are not produced by any exact sub-multiple of the string; but such notes are very transitory, and immediately vary their pitch, gradually falling or rising to the notes next below or above them, which are produced by exact aliquot parts of the whole string. This arises from the transition of the divisions of the string from one number to another; for during this transition, the parts of the string, whose vibrations produce the note, are gradually lengthening or shortening. Thus, suppose the Eolian tone was produced by one third of a string; and that the breeze so varies as to cause this tone to fall into the octave of the original note; the points of quiescence will gradually run along the strings, and by so doing will produce a more gradually flattening, until it terminates in the octave to the whole string.

Discords are also often heard from the unison strings of this instrument; the cause of this is
also

also evident from the manner in which the notes are generated; for the aliquot parts of a string contain in themselves an infinite variety of discords. Kircher in his *Phonurgia*, page 148, has attempted to account for these phenomena of the Eolian lyre, by supposing the current of air to strike on different portions of the string. But this is absolutely overturned by experience: for, suppose the Eolian note to be one fifth above the original note of the string, that is, one third of the whole; then, according to Kircher, the remaining part would be at rest, which is false; for an obstacle applied to any other point than the quiescent points of division, will destroy the eolian tone. Besides, the chords that would arise on this theory are not such as really take place in nature; thus, where the chord consists of the note F and A, the first note F is produced, according to Kircher, by the blasts striking on one fourth of the string: now, in this case, the remaining part of the string must be at rest, according to Kircher, but contrary to experience; or if it be agitated as one string, it must produce the note of three fourths of the whole string, that is a fourth above the bass note; whereas, the note really produced is the double octave to the third above the bass note, as may be seen in the table of the Eolian tones.

X.

*Analysis of the Chromate of Iron of the Bastide de la Carrade. By CIT. TASSAERT.**

THIS metallic combination which has been lately discovered at the Bastide de la Carrade, near Gassin, in the department of Var, has the form of an irregular mass; its colour is a deep brown, nearly resembling that of brown blende; it possesses the metallic lustre; its hardness middling; its specific gravity, 4.0326. Before the blow-pipe, it melts with difficulty; and when fused with borax, it gives a dirty greenish colour.

This substance was sent to the Council of Mines, under the denomination of a brown blende; which it sufficiently resembles, though it is much heavier. In order to ascertain the effect of acids on this substance, two grammes were reduced into impalpable powder, and boiled with weak nitrous acid. This acid not having attacked the powder, the muriatic was tried, which being attended with no greater effect, it was thought proper to attack it by alkalis.

Two other grammes were therefore taken, and ignited with five times their weight of potash. After a quarter of an hour's application of the fire, the crucible was suffered to cool, and the matter was mixed with water. The fluid exhibited a beautiful lemon colour which led to a suspicion of the chromic acid; and, in fact, when trial was made with the nitrates of lead, silver, and mercury, there remained no further doubt of its presence; particularly when

* *Annales de Chimie*, XXXI. 220. I retain the word Bastide, which is an old provincial term, denoting a country mansion, or castle.

the slight residue left by the potash was treated with the nitric acid, it swelled up like a vegetable extract, which is one of the most prominent characters of the chromic acid.

A. It being therefore ascertained that the chromic acid is the predominating ingredient in this compound, 500 centigrammes of this metallic salt were taken, reduced into impalpable powder, and exposed to heat in a crucible of platina, with eight times the weight of potash. The mixture entered into perfect fusion, and was kept red-hot for half an hour, after which the crucible was taken from the fire, and left to cool. The mass, when cold, had a fine yellow colour, much inclining to green. This was dissolved in water, to which it communicated a very fine lemon-yellow colour, a brown powder subsiding to the bottom, which was collected on the filter. After well washing and drying, its weight amounted to 300 centigrammes.

B. This powder was boiled in muriatic acid. Much oxygenated muriatic acid was disengaged. The liquor assumed a fine emerald colour. It was then diluted with water, and filtered, to separate a brown powder, which had not been attacked by the acid.

This powder was of the same nature as the natural metallic salt, and after three successive operations with potash and the acid alternately, it was totally decomposed. It appears, however, that this alternation is necessary, because the chromate of iron, with excess of the oxide, is not decomposable by potash, however great the quantity may be; so that it becomes necessary to remove the excess of oxide by an acid, before the potash can take up another portion of the chromic acid. If the nitric acid be used, instead of the muriatic, to remove the excess of oxide, which is set at liberty by the alkali, the matter is seen to swell up like a vegetable extract, a property which immediately manifests the presence of chrome.

C. After the whole of the mass was converted into chromate of potash and metallic muriate, the latter solution was decomposed by potash, and afforded a precipitate of a dark brown colour. This last, after washing and drying, weighed 185 centigrammes, and consisted, for the most part, of oxide of iron. The fluid separated from this precipitate, had a fine lemon-yellow colour, and was, therefore, added to the chromate of potash.

D. As it was suspected that the oxide of iron contained a portion of chromic acid, or perhaps oxide of chrome, it was boiled with nitric acid, and afterwards with caustic potash, and afforded a small portion of chromate of potash, which was separated from the oxide. The latter, after being well calcined, weighed 180 centigrammes; it was then dissolved in muriatic acid; the solution was a fine yellow colour, and afforded a black precipitate by the addition of gallic acid. With the prussiates it afforded blue, so that it cannot be doubted but that it was the oxide of iron.

E. All the solutions of chromate of potash being then put together, they were saturated with nitric acid, taking care to add a slight excess; after which, the whole was diluted with a sufficient quantity of water, and a solution of the nitrate of lead was added. A very abundant precipitate of a superb yellow colour fell down. It weighed 883 centigrammes, which, according to the proportions of component parts in the chromate of lead, gives 318 centigrammes.

It follows, therefore, from the experiments, D and E, that the chromate of iron is composed of

Chromic acid	-	-	-	-	-	-	318
Oxide of iron	-	-	-	-	-	-	180
							<hr/> 498
Loft	-	-	-	-	-	-	<hr/> 2
							<hr/> 500

Which gives for 100 parts,

Chromic acid	-	-	-	-	-	-	63, 6
Oxide of iron	-	-	-	-	-	-	36, 0
Loft	-	-	-	-	-	-	<hr/> 0, 4
							<hr/> 100, 0

The chromate of iron is likewise decomposed by the saturate carbonate of potash.

XI.

*Report made to the National Institute of Sciences and Arts (at Paris), on the 29th Prairial, in the seventh Year (June 17, 1799), in the Name of the Class of Physical and Mathematical Sciences, on the Measure of the Meridian of France, and the Results which have been deduced to determine the new Metrical System.**

CITIZENS,

TO employ, as the fundamental unity of all measures, a type taken from nature itself—a type as unchangeable as the globe on which we dwell; to propose a metrical system, of which all the parts are intimately connected together, and of which the multiples and subdivisions follow a natural progression, which is simple, easy to comprehend, and in every case uniform: this is most assuredly a beautiful, great, and sublime idea, worthy of the enlightened age in

* Two separate reports were read to the class of physical and mathematical sciences, in the name of the commission of weights and measures; one on the 6th Prairial, by Citizen Van Swinden, on the measure of the meridian, and the determination of the metre; the other, on the 11th of the same month, by Citizen Tralles, on the unity of weight. It was decided by the class, that these two reports should be united and digested into one, to be read at a general sitting of the Institute; and one of its members was accordingly charged with this office. It was performed by Citizen Van Swinden.—*Note of the reporter.*

I have translated the above from the Journal de Physique, Thermidor, an. 7; and to avoid the probability of any error of the press in the numerical results, I have compared them with the same in the bulletin of the Philomathic Society, and the Decade Philosophique, with which I find they agree.—N.

which

which we live. Accordingly, the Academy of Sciences, which, from its first establishment, had fixed its attention upon the experiments of Huyghens on the simple pendulum, did not fail to direct the meditations of men of science to the uniformity and invariability of measures. That learned body was aware of the great importance of this object: the wishes of the mathematical world were well known to them in this respect, and they beheld one of their number, the celebrated Condamine, employ his talents with the greatest zeal, in destroying the objections which ignorance and prejudice did not cease at that time, any more than at present, to oppose against its establishment*. This academy did not fail to seize the moment when the people of France began to occupy themselves in their political and social regeneration to resume this interesting subject, the execution of which seemed to have waited till the period, when the impulse given to the spirits of men, induced them eagerly to seize every thing which could tend to the public good; and when the existing circumstances permitted them to attend them without constraint, and with the prospect of success. When consulted by the constituent assembly, whose attention was fixed to this object by the proposition of Citizen Talleyrand†, and charged to determine the unities of measure and of weight, they employed, for good reasons, which were at that time developed‡, as the base of the whole metrical system, the fourth part of the terrestrial meridian comprehended between the equator and the north pole. They adopted the ten millionth part of this arc for the unity of measure, which they denominated metre, and applied it equally to superficial and solid measures, taking for the unity of the former the square of the decuple, and for that of the latter the cube of the tenth part of the metre. They chose for the unity of weight the quantity of distilled water which the same cube contains when reduced to a constant state presented by nature itself; and lastly, they decided, that the multiples and sub-multiples of each kind of measure, whether of weight, capacity, surface, or length, should be always taken in the decimal progression, as being the most simple, the most natural, and the most easy for calculation, according to the system of numeration, which all Europe has employed for centuries. Such are the fundamental and essential points of the new metrical system, which the academy has proposed; which has been adopted by the constituent assembly; and which, under names different indeed from those chosen by the academy, have been confirmed by the law of 18th Germinal, in the third year of the republic.

But as the basis of the new metric system depends on the fourth part of the terrestrial meridian, it is necessary that the magnitude of this arc should be known, if not with an extreme precision, yet, at least, with a degree of precision sufficient for practice. Various operations had been already made in France, about the end of the last century, to determine the magnitude of several arcs of the meridian, which crosses this vast empire; and though there might remain some doubts with regard to the perfect accuracy of these operations, notwithstanding the verifications which have from time to time been made, there were good reasons to con-

* *Memoirs of the Academy for 1748.*

† *Decree of the 8th of May, 1790.*

‡ *Memoirs of the Academy of 1789.*

clude, after the researches of the celebrated La Caille, that the mean degree would not differ much from 57027 toises; and consequently that the fourth part of the meridian would contain 5,123,420, and that the ten millionth part of this arc would be 443,443 lines. In the just impatience to enjoy the great benefit of exact, uniform, and universal measures, the length of the metre was provisionally settled at 443,44 under the well-formed persuasion that the more precise determinations, which were to be expected, would produce but a slight change in this magnitude.

Nevertheless, the academy, which considered this subject in its true point of view, as well in its general as in its particular relation, with regard to the public utility, its intimate connexion with the most important points of physical astronomy, with the national reputation, to which it was of consequence that the foundations of a new metrical system, proposed to a great people, and presented to the whole world for their adoption, should be determined with the greatest precision, conceived the great project of obtaining a new measure of the meridian which crosses France, by extending it beyond the frontiers, as far as Barcelona, and to apply this great arc to determine the fourth part of the earth. The constituent assembly adopted this vast project, and entrusted the execution to the academy, which nominated, without delay, several of its members to employ themselves on the different parts which composed the totality of the metrical system; and lastly, they charged the Citizens Mechain and Delambre, so worthy, in every respect, of this honourable, though laborious mission, with the task of measuring the meridional arc. The Institute afterwards nominated Cit. Lefevre Gineau to make the experiments relative to the determination of the unity of weight; and he has proved, by the beauty and accuracy of his work, how truly deserving he was of being associated with his illustrious brethren of the academy.

This great and important operation, projected by the academy of sciences for the establishment of a new metrical system, began by their orders, and happily terminated under the auspices of the institute, after seven years' labour and care, is in many respects deserving of remark. It is singular for the extent of the terrestrial arc, which being more than nine degrees and two thirds, surpasses all those which have been measured;—for the extreme exactness with which all the parts have been executed; the terrestrial survey for determining the length of the arc; the astronomical observations; the operations for fixing the unity of weight; the experiments on the length of the pendulum; all these have proceeded together, and each has been treated with the same precision; and lastly, it is remarkable, and perhaps without parallel, for the degree of authenticity with which it is sanctioned. In fact, the Institute has desired not only that commissioners chosen from its own body should examine every thing that is done, but likewise that learned foreigners should join, and make it a common work. The government has seconded this wish, by inviting the allied or neutral powers to send deputies for this object. Several have accepted this invitation; and these deputies, joined to the French commissioners, composed the commission of weights and measures*, which

* The following is an alphabetic list of the names of the members of the commission of weights. *Æneæ*, from the Batavian republic; Balbo, deputy from the king of Sardinia, afterwards replaced by Cit. Vassali; Borda,

which has met, for some months past, in this palace under your auspices, to fix decisively the magnitude of the basis of the new metrical system. This commission made the most minute enquiries into all the details of every observation, and each individual experiment. They weighed all the circumstances jointly with the observers themselves; deduced from the observations those results which were to serve as grounds for calculation; and they determined the unities of measure and weight, which are the definitive results of the whole undertaking. Never did an operation of this kind undergo so strict an examination; and the commission, as well from duty, as to express the satisfaction they have received, have thought fit to acquaint the Institute, that the Citizens Mechain, Delambre, and Lefevre Ginneau, were, in every case desirous of submitting their original documents to their inspection; that they readily gave every possible explanation, as well with regard to the instruments as the methods they employed; and in a word, that they anticipated the desires of the commissioners in every point with the attention of brothers and friends, and with that respectable openness of character which distinguishes the accurate observer, who far from fearing a severe examination, is, on the contrary, desirous that the minutest detail, and the most scrupulous enquiries, should be entered into, in order that the truth may appear in all its lustre.

In the performance of my charge of rendering this account of the work of these excellent observers, and of the operations of the commission of weights and measures, to fix the unities which serve as the base of the new metrical system, let me be allowed, for the sake of order among the multiplicity of objects I am to submit to your judgment, to speak first of that which relates to the measure of the arc of the meridian, and the determination of the metre, or unity of linear measures thence resulting; after which, I shall offer to your consideration, the experiments which it was necessary to make to fix the unity of weight; and lastly, while I present to you the standards of these two unities, may I offer some reflexions on their nature, their use, and the method of restoring them with the greatest exactness, supposing even that every standard was destroyed, and the name only to remain; an invaluable advantage of these new measures, which gives them a right to the title of invariable.

Let us begin with what concerns the measure of the meridian. Citizens Mechain and Delambre shared this immense work. The northern part from Dunkirk to Rhodes was performed by the first, and Citizen Mechain performed all the rest from Rhodes to Barcelona. He greatly regretted that circumstances did not permit him to carry his operations, as far as the island Carbera. He had even made all the preparations for that enterprize, had under-

Borda, who died in Ventose last; Brisson, Bugge, deputies from the king of Denmark; Ciscar, deputy from the king of Spain; Coulomb, Darcet, Delambre, Fabbroni, deputies from Tuscany; la Grange, la Place, Lefevre-Gineau, Legendre, Franchini, deputies from the Roman republic; Mascheroni, deputy from the Cisapine republic; Mechain, Multedo, deputies from the Ligurian republic; Pederayes, deputy from the king of Spain; Promi, Tralles, deputies from the Helvetic republic; Van Swinden, deputy from the Batavian Republic; Vassali, deputy from the provisional government of Piedmont.—*Note of the Reporter.*

I have translated the above note as it stands in the original; but I suppose that Brisson, Coulomb, Darcet, Delambre, la Grange, la Place, Legendre, Mechain, and Proni, are not deputies from foreign powers.—N.

taken the necessary excursions to examine the place, and settle the stations; and he even laid down on paper the triangles necessary to be measured; so that the whole of this part is sketched out, and by the activity and care he has bestowed upon it, it will be easy to add that arc to what has already been measured, and by that means to prolong the meridian two degrees. Let us hope that more favourable circumstances may hereafter permit that to be carried into effect which hitherto has been impracticable*.

The observers made use, for the measurement of every kind of angles, of the entire circle of Borda, which is justly called the repeating circle, from the valuable advantage which it affords of repeating the angle to be observed any number of times, and consequently diminishing the errors in the same proportion. This circle, which was constructed by the celebrated Lenoir, under the inspection of Borda himself, was fully tried by the observations made in the year 1787 by Cassini, Mechain, and Legendre, in the operations for the junction of the observatories of Paris and Greenwich.

More than one series of observations was made at each station, and the observers formed each series out of such a number of observations as they thought necessary to produce a constant and sufficiently accurate result. They noted in their register the numbers indicated by each observation, as well as the particular circumstances which took place, such as the state of the atmosphere, the direction of the light, and, in a word, every thing which could serve to determine the intrinsic value of an observation. The members of the commission nominated for the arrangement of these observations were, therefore, capable of judging of this value, not only from the facts so recorded, but also from the information they received from the observers themselves.

From such an attention to the agreement or variations between the different series of observations, the commissioners were enabled to determine the value of each angle abstractedly, without paying any attention to the others, nor to the sum of the three angles of each triangle. They have thought it their duty to take the angles, such as they were, without making the least correction, or proceeding in any other manner than by taking the means of the observations, according to the authority which the register appeared to give to each. These careful discussions for the most part related to the determination of tenth parts of seconds, and very seldom to whole seconds. The commissioners in this manner formed tables of all the triangles, which have served for the determination of the medium. They have presented this to the general commission, together with a detail of the method they have employed, and the reasons of their determinations. The commission has received these tables, and passed their resolutions, by which they are deposited in the Institute as authentic, as including all the principles applicable to the computation of the triangles and the parts of the meridian; and, in fact, the calculations were afterwards made from them.

* On account of the length of this report, I shall confine myself to such extracts as relate to the immediate operations and results, omitting his general remarks. Cit. Delambre has published an account of his researches, under the title of *Méthodes analytiques pour la Détermination d'un Arc du Méridien*, in quarto. This work is preceded by a memoir of Legendre on the same subject.—N.

The precision with which the angles were observed is such, that out of ninety triangles, which connect the extremities of the meridian, there are thirty-six in which the sum of the three angles differs from its proper quantity by less than one second; that is to say, in which the error of the three angles taken together is less than one second: there are 27 triangles in which this error is less than two seconds; in 18 others it does not amount to three seconds; and there are four triangles in which it falls between three and four seconds, and three triangles only in which it is more than four, but less than five seconds. It may be doubted whether a greater degree of accuracy be obtainable, particularly in the country where the operations were performed; and accordingly it may be supposed by those who consider these tables without being informed of the manner in which they were made, that this appearance of precision may have been given by management and subsequent correction; but the original registers of the observers, the results which they themselves sent to Paris, long before the bases were measured, and at a time when they were still busied with their operations, and the labours of the commissioners themselves, prove the contrary in the most authentic manner. No arbitrary or conjectural correction, however slight, has in any case been made; and all the angles have been determined from considerations derived from the observations themselves.

Two bases were measured by Cit. Delambre; one between Melun and Lieufaint, the other near Perpignan, between Vernet and Salces. The care and precaution with which these operations were performed, and the means adopted for that purpose, are detailed at full length by Cit. Delambre, in his Memoir already mentioned. The instruments were four rods of platina, constructed with great care by Citizen Lenthire, from the instructions, and under the inspection of Citizen Borda. Each of these rules is covered to within four inches of its anterior extremity with a similar plate of brass, movable in the direction of the length of the rule of platina, and fixed to it by the extremity which is most remote from the uncovered part. This plate of brass forms, by the different dilatations which the same variation of temperature produces in the brass and the platina, a very sensible metallic thermometer, the dimensions of which are engraved upon the anterior extremity, which carries a vernier and a microscope to ascertain the subdivisions. Before these rules were used a number of experiments were made to ascertain their dilatation, the state and motion of the metallic thermometers, and their comparison with the ordinary thermometers. The lengths of the rules, Nos. 2, 3, and 4, were compared with that of No. 1, to which all the measures were reduced, which for that reason was called the original (*le module*); which comparisons were made with such accuracy as to have no doubt of the two hundred thousandth part. Citizen Borda has delivered to the commission a memoir containing a detail of all his experiments, which will form an essential and interesting part of the collection to be published respecting this great operation.

These rules were placed in proper cases, to secure them for every external action or flexure without checking their expansion or contraction, as well as to defend them from the rays of the sun, &c.

Every

Every care was taken to support and dispose them properly in measuring the bases themselves. Their extremities were never brought into contact; but an interval was left, which was measured by a tongue of Platina, sliding from the end of one of the rules, and carrying a vernier and microscope. The corrections or allowances for differences of temperature, for obliquities of the line actually measured, and for the elevation above the level of the sea, were also of necessity to be attended to and allowed for.

As the length of the bases are expressed in modules, all the other results are denoted by the same unity. But to give a proper notion of the comparative value of this unity, with regard to the standards employed in other great operations, it became necessary to ascertain the length of No. 1, or the module from the toise of the academy called the toise of Peru; which was done before the commencement of the measuring the base. This comparison was made with a degree of precision sufficient to ascertain the hundred thousandth part of a toise. The details of these experiments are given in the Memoir of Cit. Borda, already mentioned. After his return Cit. Delambre did not fail to compare the rules which had been used for the measurement of the bases, and did not find the slightest change in their length. Lastly, the commission charged several of its members to repeat the same comparison of the module with that of the toise of Peru, that of the North, and that of Mairan, all three of which have become celebrated or important; the two first by the great operations to which they have been applied, and the third, because it was in parts of that toise that Mairan has expressed the results of his valuable experiments on the length of the pendulum, and because it is the standard of the toises which were used to measure the two terrestrial degrees in the neighbourhood of Rome, by the celebrated Bosovich and Lemaire. This new comparison of the module to the toise of Peru again afforded the same result, namely, that the scales had undergone no changes, and proved, moreover, that the module is exactly twice the length of the toise of Peru, and consequently 12 feet in length, when the centigrade thermometer is at $12\frac{1}{2}$ degrees: whence it is deducible, as well by a calculation from the dilatation of the metals as from the direct experiments of Borda, that at the temperature of $16\frac{1}{4}$ degrees, which answers to 13 degrees of Reaumur's thermometer, the module is shorter than the double toise by two hundred parts of a line; that is to say, about the eighty-fifth thousandth part of the whole.

The observations of Azimuth were made on the sun, and on the pole star at Watten, at Bourges, at Carcassonne, and at Montjouy; that is to say, at the two extremities of the meridian, and two intermediate places. The observations of latitude, which were made with the circle of Borda. From the great number of the observations, and their agreements with each other, it is considered as a certainty that the error cannot amount to any thing near half a second in any of the latitudes observed.

These observations were made at Dunkirk and at Evaux by Citizen Delambre; at Carcassonne and Montjouy by Citizen Mechain; and at Paris by Citizen Mechain, at the national observatory, and by Citizen Delambre at his own private observatory.

Four commissioners were specially charged with the computation of the triangles, which they performed separately, by different methods, in order to leave nothing doubtful as to the certainty of the results. They have also calculated, and in every case by different methods, the four portions of the meridian comprehended between the places at which the latitude was observed; namely, the terrestrial arc comprehended between Dunkirk and the Pantheon at Paris; the Pantheon and Evaux; Evaux and Carcassonne; Carcassonne and Montjoux. The details of these calculations, and the principles on which they are founded, are contained in a memoir deposited in the archives of the Institute*.

Among other conclusions which present themselves in these calculations, there are two, to which the attention of the Institute is directed: the first, that the mean degrees concluded for the four intervals, of which mention is made, all decrease as they approach the equator; and, consequently, that this operation alone would prove the oblate figure of the earth, if this article required any proof: the second, which was far from being suspected, and exhibits a very remarkable phenomenon, worthy of the enquiries of the most profound mathematicians, that these same degrees do not follow a gradual diminution, but decrease at first very slowly, between Paris and Evaux, only two modules for a degree of latitude; afterwards very rapidly, namely, sixteen modules for the degree of latitude between Evaux and Carcassonne, and that this rapid diminution becomes less between the last-mentioned town and Montjoux, being no more than seven modules†.

This remarkable fact is intimately connected with another, namely, that there are differences between the azimuths calculated for Bourges, Carcassonne, and Montjoux, from that

* The meridian between Dunkirk and Montjoux, which subtends a celestial arc of 9,6738 degrees, and of which the middle point passes through $46^{\circ} 11' 5''$ of latitude, is equal to 275792.36 modules.

That is to say;

	Modules.
1. The distance between the parallels of Dunkirk and the Pantheon, the middle point of which lies in lat. $49^{\circ} 56' 30''$, subtends an arc of	$2^{\circ}.18910$ and measures 62472.59
2. The distance between the parallels of the Pantheon and Evaux, the middle point of which lies in lat. $47^{\circ} 30' 46''$, subtends an arc of	2.66368 and measures 76145.74
3. The distance between the parallels of Evaux and Carcassonne, the middle point of which lies in lat. $44^{\circ} 41' 48''$, subtends an arc of	2.96336 and measures 84424.55
4. The distance between the parallels of Carcassonne and Montjoux, the middle point of which lies in lat. $42^{\circ} 17' 20''$, subtends an arc of	1.85266 and measures 52749.48
Whole celestial arc	9.67380 Measure 275792.36

† If from the four intervals before given, we deduce the mean degree, which may be concluded from the spherical hypothesis, which is sufficient for a cursory view, we shall find the mean degree in round numbers;

	Modules.	Difference.	Difference for one degree.
Between Dunkirk and the Pantheon, mean latitude, $49^{\circ} 56' 30''$.	28538	5	2
Between the Pantheon and Evaux, mean latitude, $47^{\circ} 30' 46''$.	28533	44	16
Between Evaux and Carcassonne, mean latitude, $44^{\circ} 41' 48''$.	28489	12	7
Between Carcassonne and Montjoux, mean latitude, $42^{\circ} 17' 20''$.	28472		

of

of Dunkirk taken as a base, and the azimuths actually observed at these three stations. These two facts eventually confirm and support each other; and when combined, they indicate, either an irregularity in the terrestrial meridian, or an elliptic form in the equator and its parallels, or an irregularity in the internal structure of the earth, or an effect of the attraction of mountains, or a powerful action of all these causes, or a certain number of them united. It is a task worthy of the most celebrated mathematicians to fix their attention upon these facts, and endeavour to develop their elements, in order to obtain a more perfect theory of the earth than we have hitherto possessed.

The commissioners, whose object it was to determine the length of the fourth part of the meridian, and thence the unity of measure, directed their whole attention towards that object. They employed the whole arc comprized between Dunkirk and Montjouy in their calculations, which were strictly made, according to the elliptical hypothesis. To make this calculation, it was requisite to know the difference between the equatorial and polar diameters. This was obtained by comparing the newly-measured arc with the largest and best situated of the arcs already measured; namely, that in Peru. The computations carefully made, and by different formulæ, gave one three hundred and thirty-fourth part for the flattening of the earth, which is the same as results from the combination of a great number of experiments at different places on the earth, on the length of the simple pendulum, as well as conformable to the theory of the nutation of the earth's axis, and precession of the equinoctial points. It is, moreover, observed, that as the middle of the entire arc, terminated by Dunkirk and Montjouy, passes near the forty-fifth, or mean degree, of latitude, a slight error would have the less influence on the final determination.

By various methods of computation, employing the arc between Dunkirk and Montjouy, of two hundred and seventy-five thousand seven hundred and ninety-two modules, and thirty-six hundredth parts; and the quantity, three hundred and thirty-four for the oblate figure; it was found that the fourth part of the terrestrial meridian is two million five hundred and sixty-five thousand three hundred and seventy modules; and consequently, that its ten millioneth part, or the *metre*, or unity of measure, is 256537 millioneth parts of the module.

To reduce this length to the ancient measures, it is observed that if the module and the toise of Peru were supposed to be each at the temperature of the latter when employed by the academicians, which answers to the thirteenth degree of the thermometer of Mercury, divided into 80 parts, or sixteen and a quarter of the centigrade thermometer, the *metre* would be equal to 443,291 lines of that toise; but by reducing the module to the temperature (as it ought to be) to which it was reduced in the expression of the length of the bases which was used to calculate the triangles, and the length of the portion of the terrestrial meridian, *the true and definitive metre is four hundred and forty-three lines, and two hundred and ninety-six thousandth parts of a line of the toise of Peru*, this last being constantly supposed to have the temperature of sixteen degrees and a quarter. This last correction became necessary, on account of the difference of expansion in the two metals.

(To be continued.)

XII.

*Detached Facts or Notices on chemical Subjects—Nitric Acid—Oxide of Azote—Indigo—Nitre of Potash—Residue of Ether—Inflammation of Oils by Nitric Acid—Ammoniac—Putrid Water at Sea. By CITIZEN PROUST, Professor of Chemistry at Madrid.**

Nitric Acid.

WITH very dry nitre and sulphuric acid I prepared nitric acid, which was pure, and supported the proof of barytes. After it had been deprived of gas, by careful distillation, it continued yellow, and had a specific gravity of 152.—When again distilled, the first product had the specific gravity of 151, and was less yellow; the second, though less coloured, was only of the same density; but I was surprized to find the residue, which was perfectly colourless, weighed only 147. This being also distilled over, the first portion was of 149, and the rest did not exceed 144.

In another experiment the acid obtained from very dry nitre had the density 155. It was first cleared of gas, and afterward distilled. Its first product had the density 162. The second paler, 153; and the colourless residue weighed no more than 149. Hence we may observe that the acid in its concentration is affected very differently from what we might suppose †, and that it is the reverse of other acids. I have remarked on these occasions, that the stronger the acid the easier the distillation, and that it is not necessary to boil it, in order that it may evaporate with more rapidity. A notion of the force which concentrated acid exerts upon its own parts may be formed by pouring the acid of 148 upon pulverized tin. It produces no more effect than upon sand. This, however, is not the case with zinc‡.

Oxide of Azote.

To obtain this gas with certainty, the acid made use of must have the specific gravity of 15° of the areometer of Baumé (1,114) to be applied to zinc. An acid at 18 or 20 afford a mixture of nitrous gas, and no doubt azote.

Indigo.

An acid of 148 dissolves this substance, and turns it yellow by oxidation. It is a resin soluble in alcohol and separable by water. It was with surprize that I found, by means of weak acids, magnesia even abundantly in indigos, besides the extractive matter which they are known to contain.

An acid of 151 or 152 inflames indigo, as Sage has published, and as Woulf likewise shewed to Hillare Rouelle, who shewed it in his lectures.

* Journal de Physique, Messidor VII.

† The same facts have been observed by Chaptal and other French chemists.

‡ The majority of chemists ascribe this difference of effect to the decomposition of water, which, from many facts, appears to be the true solution.

Nitrite of Potash.

This salt is totally deliquescent. It is separated from the fused and ignited nitrate by means of crystallizations, which gives solidity to all the nitrate which has remained unchanged. Weak sulphuric or nitric acid separate nitrous gas in abundance. Distilled vinegar precipitates nothing, though it occasions a slight effervescence.

Expecting to separate the nitrate from the residue of the crystallizations, I added spirit of wine, which threw down a saline precipitate. Losing sight of my nitrate afterwards, I took a fancy to decompose it by an acid in the midst of the spirit of wine. The consequence was, a new fact which produced ideas of some importance, from what at first promised nothing.

Weak sulphuric acid poured into the mixture occasioned an effervescence, and its product instead of being nitrous gas, proved to be nitric ether with disengaged heat. This experiment certainly deserves to be repeated. I set fire to the vapour, and the greenish colour in the flame was similar to that of the ether. The nitrous gas was probably decomposed, its oxygen absorbed by the alcohol, and the azote undoubtedly dissipated with the ethereal gas; but, in order to transform alcohol into nitric ether, is there any thing else wanting but a simple solution of concrete oxygen? The following facts seem to prove this:

I poured 4 oz. of spirit of wine, well dephlegmated, into a pint bottle filled with the oxygenated muriatic gas. The gas was quietly dissolved, and the bottle became clear. A few minutes afterwards the spirit of wine was discoloured, and there was a disengagement of heat without the appearance of any kind of gas. At the opening of the bottle I perceived the existence of nitric ether.

This experiment was repeated with the same spirit of wine successively, on clean bottles of the gas. The same phenomena were re-produced, and the ether appeared to increase; after which I judged, by the diminution of the smell, that it advanced to another state, which is probably the transition from alcohol to the states of vegetable acid, among which the muriatic acid re-appeared with all its inertness.

Why may it not be the same with alcohol as with most of the other vegetable products, which are very capable of being superoxidized in their totality without disunion or combustion of any one of their principles? Such are the alterations which we observe in the volatile oils, fat, tallow, colouring resins, &c. all which are capable of receiving new appearances by the addition or spontaneous absorption or a slight dose of oxygen, while the maximum produces their entire decomposition. These observations are not applicable to sulphuric ether.

Residue of Sulphuric Ether.

The residue of this ether, distilled to the utmost, never affords sulphur; notwithstanding the observations of a number of writers. If a small portion be distilled in a large retort, the product is spirit of wine, oil, the common gases, and water.

After

After the intumescence has subsided, the pitchy matter, which is merely oil, rendered thick by charcoal, takes this last state by the concentration of the acid, and begins to separate to such a degree that the liquid becomes visibly clearer.

The distillation being still urged, all this charcoal becomes collected together; sulphureous gas passes over, and afterwards oil of vitriol, slightly coloured by charcoal. All the acid passes at last in this degree of concentration, and the residue at the bottom of the retort is a plate of pure charcoal; such as is obtained by distillation of sulphuric acid from any charcoal whatever.

When, in order to obtain the olefiant gas of the Dutch chemists, three parts of the acid are distilled with one of the spirit of wine, the pitchy matter becomes entirely charred, and in this manner a dram of dry charcoal is obtained from one ounce of alcohol.

On the Inflammation of Oils by the Nitric Acid.

With strong nitric acid of the specific gravity 1.52 it is impossible to inflame linseed oil by the ordinary method; but in order to succeed in such a manner as to form a striking appearance, it is proper to pour linseed oil gently to the depth of an inch upon the same depth of the acid. The wine-glass (*verre à patte*) being then placed upon a plate with a small quantity of water, the whole is to be covered with a bell-glass, sufficiently tall. In less than a quarter of an hour the ebullition begins, which is speedily followed by the inflammation. The experiment succeeds equally well, though rather more slowly, with acids of 1.49 and 1.50.

Oil olive treated in the same manner, but always with the mixture of sulphuric acid, takes fire with the same success. In these experiments the operator has the satisfaction to observe, at his ease, the charcoal disengage itself from the other principles of the oil before the inflammation.

And in order to observe with accuracy the changes which the acid undergoes, on its part, from the olive oil, for example, two fingers depth of the latter, are to be poured on the same quantity of acid, at 1.49, or thereabouts, placed at the bottom of a cylinder, with a foot, or one inch in diameter; and the line of contact of the two liquids being marked, the following effects are seen:

The height of the acid is rapidly diminished; nitrous gas escapes through the oil; and the phenomena are completed when the remainder of the acid, becoming too aqueous, ceases to act on the oil, which on the other hand becomes somewhat thickened.

If, instead of oil, a spirit of wine of 35° (0.842. I. p. 39), all the series of phenomena which belong to the formation of nitric acid are very agreeably observed. This process, in some respects, resembles that of Black, which consists in skilfully causing three strata of different liquors in a matras, placed in ice-cold water. These liquors are the fuming acid, water, and spirit of wine. These three strata may be easily arranged by means of a ball and

syphon, with which the liquors are suffered to run down on one of the sides of the neck of the vessel; which neck for that purpose ought not to exceed two inches in length. I have not seen this process in any French author (if I am not deceived), though it is described in one of the first Journals of Crell.

On Ammoniac.

It is known, that the flame of a candle placed at the mouth of a cylinder full of ammoniacal gas is enlarged. From this fact, I have usually in my lectures mixed it with one part of oxygen gas, in which case it takes fire, and detonates. For want of time, I have not constructed an eudiometer of iron, or of glass, with a conductor of iron to operate over the water bath. Mixing these two gases afterwards, and detonating them, the quantity of azote contained in the volatile alkali, may be very accurately separated in putrid sea-water.

Sea-water.

Two years ago, the Prince of Parma had the goodness to send me some bottles very well corked of putrid water, taken from a vessel in its return from Carthage.

Upon opening one of them, I readily perceived the smell of hepatic gas. Water tinged with a solution of copper was precipitated of a brown colour, and this precipitate, collected and examined by the blow-pipe, proved to be the blue sulphuret of copper.

After the water had lost its smell by exposure to the air, I examined it, and found it excessively loaded with plaster, which led me to conclude that the cask had certainly not been filled with river-water.

So large a quantity of sulphate of lime in this water could not but produce sulphurated hydrogen in its putrefaction; for which reason I was not surprized at its quality; but in river water which does not contain plaster, what may be the nature of their putrefaction? As I have never seen stinking water of this second kind, I suppose it would be necessary to be in some sea-port to observe, analyze, and endeavour to render them sweet.

In the mean time, the following facts with regard to hepaticized waters, may be of some use towards the solution of this important problem.

With about half an ounce of magnesia in powder, agitated in the putrid water, I caused all its bad smell to disappear in less than four minutes.

In order to ascertain still more clearly the effect of magnesia, it was necessary to apply it to water in the highest degree stinking. For that purpose, I strongly agitated an ounce of magnesia in powder, in a pint of artificial hepatic water, the most stinking I could possibly make, and in a few instants I succeeded very well in ameliorating the smell; but it obstinately retained a smell of onions which I could not dispel: but even in this state, in case of necessity, I could have drank it without repugnance.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

National Institute of France.

THE philosophical and mathematical communications of this establishment during the last trimestre are as follows:

Gayton related a number of experiments on the mutual affinities of the earths to each other in the humid as well as in the dry way. His experiments of the latter kind were made in crucibles of platina; and on the occasion of the former, he discusses the consideration how far barytes and strontian ought to be ranked with alkalis. At the same sitting, he communicated an account of the experiments on the combustion of the diamond. (See page 295.)

It is well known to chemists, that Scheele is the first who has spoken of a brown oxide of lead, and professor Proust has lately described some of its properties, and the manner of making it. The experiments of Vauquelin shew that this oxide, which is of a dark brown colour, becomes yellow by the slightest impression of heat from the blow-pipe, and afterwards melts. It affords a very great quantity of pure oxygen gas, when heated in a retort with the pneumatic apparatus. When heated with weak sulphuric acid, it also gives out oxygen gas, precisely like manganese, many of whose properties it appears to possess. Its most remarkable property is, that it takes fire speedily by simple trituration with sulphur. The inflammation is rapid and brilliant, but without detonation or explosion. This super-oxygenated lead, after this combustion of the sulphur, is converted into galena, of which it has the black blueish colour, and all the other characters.

The same chemist communicated likewise some observations on the decomposition of the muriate of soda by the oxide of lead; a fact, which though well known before, was not satisfactorily explained previous to the experiments and researches of Vauquelin.

Huzard read certain observations and reflections upon canine madness, by the late Flandrin, associate of the Institute. Among other important observations, the most remarkable is, that herbivorous animals, though susceptible of this disease, very seldom communicate it. The same citizen read for Cit. Gilbert, a paper on agriculture, and the perfection of the race of sheep.

Cit. Teissier likewise read some useful agricultural observations applicable to France; and Rougier la Bergerie read a memoir on a disease of barley. Lacepede presented a new methodical table of mammiferous animals; that is to say, viviparous quadrupeds and cetacea. Cit. Desfontaines communicated observations which he had made in the desert of Sahara, on the culture and uses of the date-bearing palm.

Cits. Ventenat and Vauquelin presented each a several work. The first, a table of the vegetable kingdom, according to the natural method of Jussieu. A work in which is contained, not only the abridged history of the science, with the anatomy of vegetables, but likewise the order of their arrangement, the character of the classes, the families, and a great number

number of known genera. The second work of Vauquelin consists of a series of experiments, made in order to analyse the sap of the elm, the beech, the maple, the birch, and the chestnut-tree.

The same chemist has also published a work, entitled, *Manuel de l'Essayeur*, or a treatise on the art of assaying gold and silver by the cupel, and the process of parting; containing every thing which is absolutely necessary to be known in the practising that art. Cit. Delambre has published his method of computing the arc of the meridian from the late measurements in France.

The transit of Mercury over the sun's disc, which happened on the 18th of Floreal, year 7 (29 May, 1798), and lasted seven hours twenty-four minutes and fifty seconds, was observed by Cit. Messier, during the whole time of the passage. The great number of accurate observations which this astronomer made, are recorded in a memoir read at one of the ordinary sittings. Mercury appeared perfectly round, and surrounded by a very sensible atmosphere, when the light of the sun was moderated by slight clouds. Cit. Jeaurat observed the same phenomenon, and read a memoir containing his observations.

Cit. Cassini, associated member, gave a description of a new compass for determining the declination of the magnetic needle with the greatest precision. The needle of his compasses is suspended by wire or thread according to the method of Coulomb; and Cit. Cassini, by adapting a telescope, and an entire movable circle, obtains an instrument with which an angle may be measured with the same degree of exactness, as by the repeating circle of Borda. According to the observations made with this compass on the platform of the National Observatory, it is proved that the bars of iron, which enter into the construction of the edifice, render all the observations of the needle defective: and by other experiments made at Montmartre, far from all foreign influence, Cit. Cassini has settled the declination of the magnetic needle at $22^{\circ} 49'$ on the 5th Prairial, in the year 1.

Van Swinden gave an account to the class of mathematical and physical sciences, of the operations for determining the length of the meridian, and that of the metre. Cit. Tralles made a report of the operations relative to fixing the unity of weight.

The standards, in platina, of the metre and kilogramme, and the general report of the whole operation by Van Swinden, were presented to the legislative body, and deposited among the national archives.

Note concerning the Devahk, or Cubit of the Nilmetre.

This measure, which is of the highest antiquity, is valued at 20,544 inches, in the History of Modern Astronomy, tome II. p. 146 (I suppose of Lalande), which amounts to 0,5559 metres. Cit. Dillon, examiner of weights and measures, having procured a new copy of the cubit of the nilometre, very carefully made in the workshop of the widow Lennel, after another copy, taken immediately upon the spot by a learned Englishman, found it correspond with 0,5555 metres, a result falling short of the former determination by 0,0004 only,

only, whence it follows that the cubit of the nilometre is within one thousandth part of five ninths of the metre, or the eighteen millionth part of the quarter of the meridian.—Soc. Philom. No. 27. An. VII.

Citizen Lalande has communicated to the Institute of France the observation and computation of the last opposition of Mars, and by comparing it with that of 1790, he finds that no more than 58 seconds are required to be taken from the place of the aphelion of Mars, employed in the last edition of his astronomy. But he announces the great labours of his nephew, Lefrançais Lalande.

C. Lalande also gave computations of solar eclipses, and sidereal observations observed for several years past, to determine the positions of different places. He places Hamburg at 30' 9" from Paris; Cobourg, 34' 30"; Mulheim, 21' 20"; Halle, 38' 28"; and Königsberg, 1^h 12' 35".

*On the Use of the Actual Caustery in a Disease of the Chestnut-tree, by the Cit. CHAPTAL.**

The chestnut-tree forms the only resource of the cultivator in various parts of France, where it is particularly encouraged on account of its utility. This tree lives long, and often grows to an extraordinary size; but unfortunately its ligneous part degenerates in several circumstances: it softens, falls into dust, and, in the course of time, a cavity is formed in the very heart of the tree, which becomes enlarged by the progress of the decomposition; so that at last the trunk presents nothing but a shell of bark, which being too feeble to support the weight of the branches, or to resist the violence of the winds, can no longer secure or prolong its existence. By such alterations and decompositions of the ligneous principle it is, that trees are seen to perish in a very short time, whose growth have required whole centuries.

Citizen Chaptal, in his travels through different parts of the republic, and chiefly in the Cevennes and in the department de l'Allier, observed that the internal part of a great number of chestnut-trees was dug out and burned to a coal all over its surface. The inhabitants explained to him, that this was done to stop the progress of the rotteness, which would otherwise destroy the whole tree.

When they perceive that this very common and destructive disease begins to make some progress in the chestnut-tree by excavating its trunk, they collect heath, and other combustible vegetables, and burn them in the very cavity, till the surface is completely converted into a coal: it seldom happens that the tree perishes by the effect of this operation, and it is always found that this remedy suspends the progress of the decay. It is practised in the same manner, and with similar success, on the white oak. When we compare the effects of the ac-

tual

tual cautery on the animal system, in similar diseases, a new resemblance is seen between the diseases which affect the organic beings of both kingdoms, as well as between the remedies by which they may be opposed.—Soc. Philom. No. 27. An. VII.

NEW PUBLICATION.

A System of Familiar Philosophy in twelve Lectures; being the course usually read by Mr. A. Walker. Containing, the elements and the practical uses to be drawn from the chemical property of matter; the principles and application of mechanics; of hydrostatics; of hydraulics; of pneumatics; of magnetism; of electricity; of optics; and of astronomy. Including every material modern discovery and improvement to the present time. London, printed for the author, and sold by Kearsley in London; price two guineas, 4to. 571 pages, with 47 copper-plates.

1. Mr. Walker's first lecture is introductory, on the nature of light and fire. 2. On the properties of matter. 3. Mechanical powers. 4. Chemistry. 5. The atmosphere. 6. Hydrostatics and hydraulics. 7. Electricity. 8. The same subject. 9. Optics. 10. Astronomy. 11. Of the moon in particular. 12. Astronomy continued.

The public, for a long series of years, has attended with pleasure and improvement to the clear and perspicuous lectures of this philosopher, which makes it the less necessary to enter, at present, into any general character of his performance. Its particular character, or the features of novelty it exhibits, will be found to consist in certain theoretical positions, respecting the mechanical and chemical effects of light and fire, in the system of the universe. As I cannot, with sufficient brevity, give a detail and examination of these, upon which the author has employed much time and labour, I must leave this part of the subject to the scientific reader himself.

SECTIONS illustrative of the GEOLOGY of the Country near HULL.

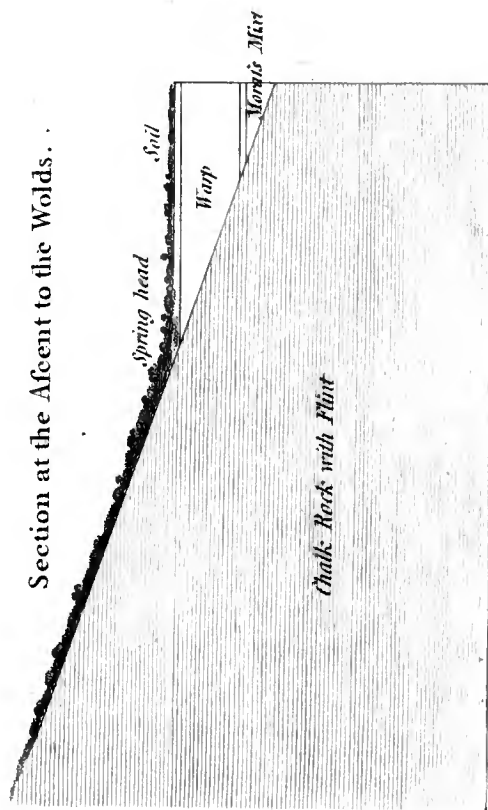
from the Cliff to near Swarland 13 or 14 Miles.

Philos. Journal, Vol. III. Pt. VII. (Nov. 1792).

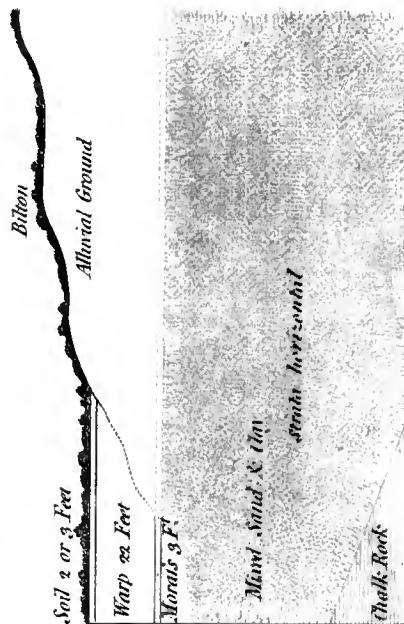


ENLARGED SECTIONS.

Section at the Ascent to the Wolds.

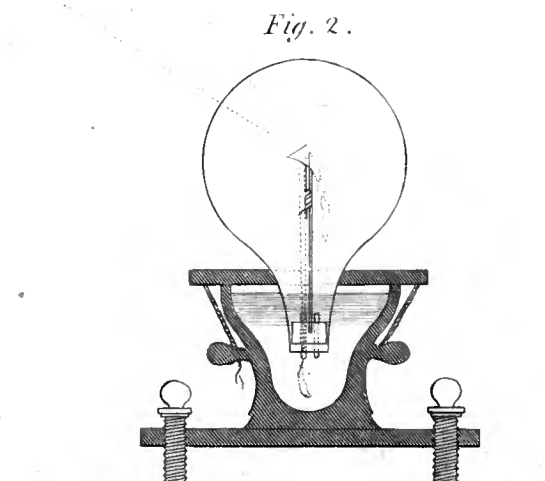
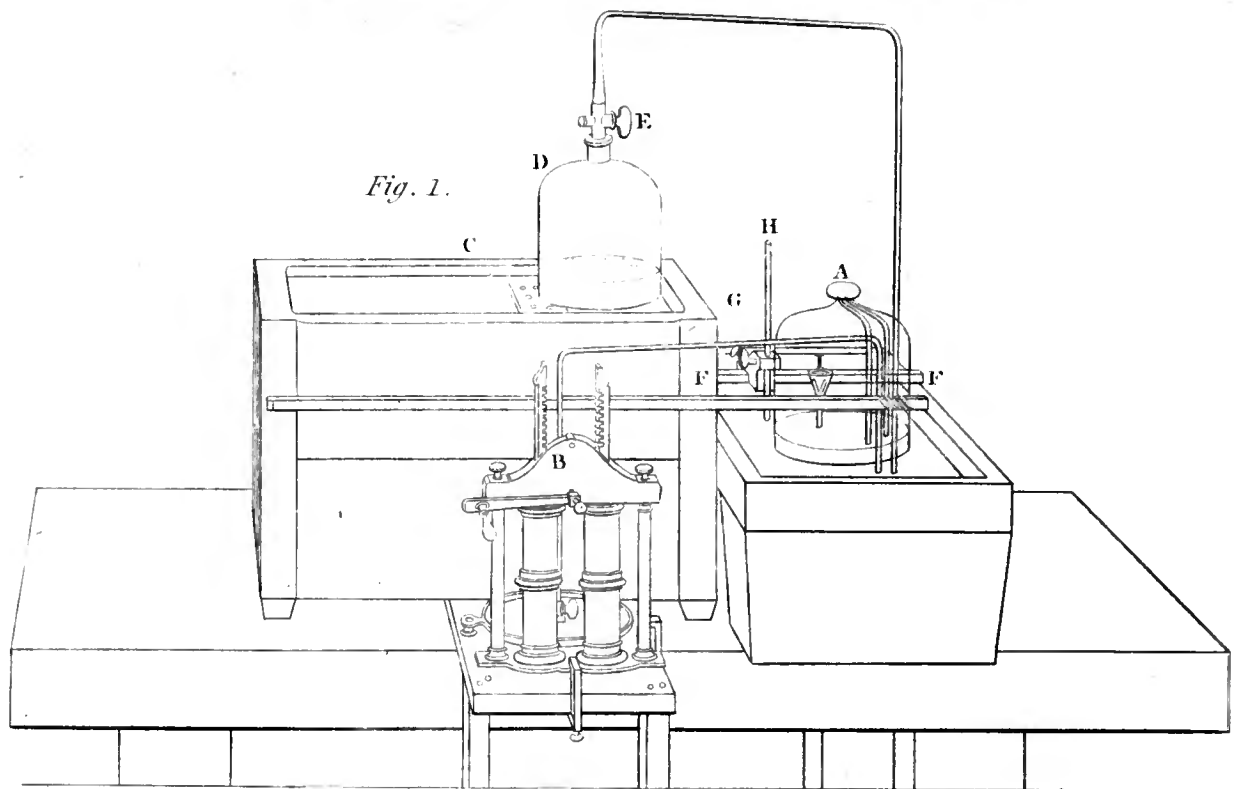


Section at Bilton.





Apparatus for the combustion of the Diamond in oxygen gas.



Barlow sculp.

Fig. 3.



5 Decimetres .



A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

NOVEMBER 1799.

ARTICLE I.

Chemical Experiments and Observations on the Production of Sugar, and an useful Syrup from indigenous Plants, by SIGISMUND FREDERIC HERMBSTADT.*

SUGAR has become an indispensable article of consumption, and, on that account, the monopoly of that commodity from the East and West Indies, which is favoured by nature itself, turns out to be very oppressive: and the more so, as the demand for sugar increases by the increasing luxury, even among the poorer class of people. For this reason, that monopoly becomes an oppressive burthen, not only to the individual consumer, but also to the state: for the millions of dollars, which are sent in cash to the East and West Indies without any considerable barter, contributes its share to the exhausting of the public finances. These were, no doubt, the chief motives which, above fifty years ago, induced several German and foreign chemists to institute experiments for discovering a native substance, which might contain, in mixture or combination, a sufficient quantity of sugar to be obtained with profit, and thus remove a burthen that daily becomes more oppressive. These motives are of themselves sufficient to make the home-production of sugar desirable; even if it were not combined with another advantage, of great importance in a physical as

* Translated from the *Neueste Schriften der Gesellschaft Naturforschender Freunde zu Berlin*, 4to. vol. II. Berlin, 1799. page 324. 350.

well in a moral view, I mean the abolition of the slave-trade, *so degrading to the dignity of man*, which may probably in part be effected by such a discovery.

The chemical analysis of vegetable substances, and the examination of their mixed or combined parts, has sufficiently proved, that the East and West Indies are not the only countries which nature has blessed with plants containing sugar. Nature has rather propagated the saccharine matter in the products of the vegetable kingdom in such extraordinary abundance, that nothing but perseverance of research is required, to discover those single links in the great chain of natural products, from which the saccharine constituent part may be experimentally obtained the most pure, cheap, and abundant.

Of all the plants hitherto examined with this view, there is certainly none which deserves to be placed so near the *true sugar-cane*, as the whole genus of the maple-tree; and of these more especially the sugar-maple (*acer saccharinum*), and the silver-maple (*acer dasycarpum*. Erhard.)

Both these trees have been made use of in North America, more than fifty years ago*; and, during the last eight years†, with uncommonly great profit, for the purpose of making a very useful sugar. My own experiments, made rather in the large way with most species of maple, during a period of nearly three years, namely, since the winter of 1796, have convinced me, first, that all of them may be more or less advantageously employed for making sugar; and, secondly, that from the sugar and silver-maples cultivated on German ground, and even of inferior goodness, a very good *raw sugar*, or *muscovado*, in every respect like the best obtained from the West-Indian sugar-cane, may be produced so cheap, that the pound will cost no more than eighteen or twenty pennies‡; notwithstanding the juice was boiled with charcoal, and too great a number of workmen had been employed, which in experimental operations, where nothing but conviction is aimed at, cannot be otherwise. But from this it may be presumed with certainty, that if the process were carried on in the large way, where a single workman, during the tapping, may take care of at least five hundred trees, and where the boiling of the liquor may be performed with pit-coal or turf, the pound of raw sugar procured from the maple-tree will not amount above *one groschen*||.

The

* An account of a sort of sugar, made of the juice of the maple, in Canada. Philos. Transf. No. 171.

Kalin's description how sugar is made from various species of trees in North America. Transactions of the Royal Academy of Sweden for the year 1751, vol. XIII. Also Mémoires sur le Sucre d'Erable, utilisé dans le Canada, in Nouvel. Oecon. Hist. 1757.

† Notice sur l'Erable à Sucre des Etats Unis, et sur les Moyens d'en extraire le Sucre, etc. par Mr. Rush, in Rozier's Observations sur la Physique, etc. tom. XLI. Paris, 1792, page 9, etc.

‡ The Berlin pound is about a half-ounce heavier than the English avoirdupois; and 827lb. Berlin = 854 English. One pound sterling being reckoned at par equal to six dollars fifteen grosch four pennies, Berlin, the above eighteen pennies will give about *two-pence halfpenny* English money.—Translator.

|| My experiments for procuring sugar from maple-trees, made several years since, on the suggestion of his Excellency the minister of State *Struensee*, are in this place mentioned only by way of digression. I intend to give, perhaps in the next volume of these Memoirs, a more circumstantial description of them, together with

The process of boiling down of the juice is likewise so very simple, that any farmer is able to perform it. The following instance will prove this. Count Podevills of Gufow introduced an huntsman to me, for the purpose of seeing and being instructed in the method of reducing the juice of the maple-tree to raw sugar. This man afterwards instructed the manager of Count Podevills's estates, who soon afterwards sent me a considerable quantity of raw sugar from the maple. But as, according to my experiments, only the sugar and silver maple-tree afford these advantages; and as the ash-leaved (*acer negundo*), the common maple (*acer campestre*), the plane-tree (*acer platanoides*), the sycamore-maple (*acer pseudo-platanus*), and all the other species, not only yield less juice, but also less rich in saccharine matter, the profits of making of sugar from the maple-tree are not to be expected until the necessary plantations of the sugar and silver-maple have been made, and have sufficiently advanced in growth, which must require twenty or thirty years.

In order, therefore, that Germany, and especially the Prussian territories, may not be deprived for so long a period of the home-manufacture of sugar, it would undoubtedly be highly beneficial to the community, if, among the numerous plants containing sugar, and presented to Germany by nature, some should be discovered, from which a proper substitute for the West India sugar may be extracted, though with less advantage than from maple-trees, and of which a proportional profit may be obtained; at least, until the maple plantations shall have acquired the proper degree of strength*.

From

with their results. I shall only remark, that, according to my experiments, a tree of the sugar and silver-maple from 25 to 27 years age, and twelve or thirteen inches diameter, yields, upon an average, and without particular management, $4\frac{1}{2}$ lb. of raw-sugar. The superficial acre being capable of supporting 180 trees, the sugar obtained from an acre will amount to 765 lb. In consequence of these experiments, I have made an exact computation. Taking it for granted, that at a medium, one tree should only afford one pound of raw sugar, the charge, including the expences of fuel, wages, and wear of vessels, would be only one grosch $7\frac{1}{2}$ penny (about nine farthings). And one square mile (*German, fifteen in. length to a degree of the equator*) would furnish yearly 11,520,000 lb. of raw sugar obtained from maple-trees.

* I cannot avoid refuting, in this place, several opinions which have been vulgarly adopted against the maple-tree; namely, that it dies by freezing in hard winters; that it dies after tapping; and, lastly, that it is destroyed by noxious caterpillars. With regard to the first, I refer the reader to the maple-plantations possessed by Count Veltbeim at Harbke. Most of the stocks of sugar and silver-maple are now above thirty years old. They have, therefore, endured the cold winters of the years 1776, 1788, 1793, and will probably not be destroyed this year, since there is not yet any trace of decay observable, even in the branches. Ought not these trees to have been killed long since by the frost, if they are so easily injured by it? With regard to the second case, we may also apply to experience. The trees at Harbke have been strongly drained for two successive years, without any one having died; on the contrary, they have even blossomed and yielded fertile seeds in the subsequent year. This winter, Count Veltbeim will cause them to be tapped for the third time. In the same manner several trunks of *acer pseudo-platanus*, and *acer platanoides*, in the forests of his royal highness Prince Henry of Prussia, at Rheinsburg, have been tapped in the last winter of 1798, and not a single one of them has suffered any injury by it. For the purpose of learning how far it may be possible to destroy a maple-tree by draining, I had a couple of them pierced with twenty-four holes. When the liquor had ceased to flow, the borings were left without being stopped, each of these trees was, besides, cut

From these motives, I undertook a series of experiments, two years and a half ago, when I began my examination of the maple-sugar, the object of which was to examine and correct the experiments of former chemists, as well as to extend our knowledge of the subject. Of these I shall proceed to give an account.

a. Experiments with India-corn, for the purpose of making Sugar from it.

The India corn (*zea mays*) is stated by Juti*, to contain a very large quantity of true sugar in the knobs of its young stalks. Mr. Jacquin † of Vienna is said to have repeatedly succeeded in preparing sugar from the stalks of India-corn. The same is asserted by Marabelli ‡, in a treatise published on this subject.

It is also reported, that the preparation of sugar from the stalks of the India-corn, especially when grown on a marshy soil, has been attempted in the large way in Italy; but that the sugar obtained from them was still too dear, comparatively with the raw sugar from the West Indies.

As my chief end was to clear up this point by actual experiment, I made some trials with it, of which the following are the results.

In the summer of 1796, I cultivated some India-corn, on a moderately good, but somewhat swampy soil. When the young plants were about six inches high, their leaves, on being chewed, had a taste resembling that of liquorice, but the stalks, especially near the knots, had a saccharine taste. The young plants were then cut off above ground, separated from the leaves and adhering impurities; after which ten pounds of them were cut small, then bruised in a stone mortar, and the juice pressed out. This liquor weighing five pounds, and still possessing an herbaceous taste, was clarified with whites of eggs, by which means it became of a clear yellow colour like wine, and almost entirely lost its disagreeable taste. This being reduced by boiling, afforded eight ounces of an agreeably flavoured useful syrup.

b. Examination of the Ears of the India-corn.

As the young ears of the India-corn possess a pleasing saccharine taste, when scarcely formed, they were likewise subjected to investigation. Ten pounds, freed from all the surrounding leaves, were bruised in a stone mortar, and by pressure yielded four pounds of a milky liquor, which could not be perfectly clarified by whites of eggs. By slow evaporation to the consistence of a syrup, I obtained nine ounces of a brown pleasantly-tasted syrup, which, however, was distinguished from the former by containing more mucilaginous matter.

in eighteen places with an axe, and one of them was deprived of all its branches by lopping. Notwithstanding this, both trees continued in a sound state, and the very next summer all their wounds healed up of themselves. It is otherwise with birch-trees, which always wither immediately after a strong draining. The third objection is absurd, and requires no refutation, because contradicted by experience.

* See his Oekonomische Schriften, part I, page 397, and part II, page 191.

† Croll's Chemische Annalen, for the year 1784, vol. I. page 96.

‡ Franc. Marabelli de Zea Mays Planta analytica Disquisitio. Pavia 1793.

c. Examination

c. *Examination of the Stalks of a more advanced growth of the India-corn.*

Twenty pounds of stalks of this plant, which had grown larger, were cut small, and then pounded in a stone mortar, with the addition of a little water, and their juice pressed out. This liquor had a considerably unpleasent, herbaceous, and rather sharp taste. When purified with white of eggs, and inspissated to the consistence of a syrup, it afforded twelve ounces of syrup remarkable for its disagreeable saline taste, resembling that of a vegetable extract rather than of a syrup.

d. *Experiments instituted with the view of preparing dry Sugar from the India-corn.*

In order to find how far it might be practicable to obtain crystallizable sugar from this plant, the syrups made from its young stalks and ears were redissolved, each by itself, in lime-water newly prepared, and afterwards gently boiled down; by which management a quantity of impurities was separated. The liquids being strained through a woollen cloth, were again separately evaporated to a thick syrup, which last was left standing for eight months in a glass jar, in a moderately warm place; after which I discovered minute grains of true sugar which had crystallized, but could not without difficulty be separated from the large quantity of liquid. In consequence of this, I inspissated each of these syrups in gentle heat, to complete dryness, and digested the dry mass again in six parts of alcohol, with a boiling heat. The fluid, while yet warm, was then hastily poured through a linen cloth, on which the undissolved mucilaginous parts were retained; while, on the other hand, a true sugar of a yellow colour crystallized in the spirituous solution, in small grains, upon cooling. After this the alcohol was distilled off from the remaining fluid; by which treatment an additional portion of true sugar was obtained from the residue by gentle evaporation. On the whole, about two ounces of sugar were obtained from the syrup prepared from the young stalks, and $1\frac{1}{2}$ ounce from that of the young ears.

It is therefore sufficiently proved, that true sugar can be produced from the young fresh stalks, as well as from the young ears of the India-corn. But its separation from the gummy and other parts united with it is subject to such great difficulties, and the quantity obtained is so small, that the pound of raw sugar fit for use could not be prepared from this material at a less price than a dollar (about three shillings). It is obvious, from these facts, that we are probably never to expect any real advantage, with respect to cheapness, from this method of obtaining sugar.

e. *Experiments with the Siberian Bear's-breech, for preparing Sugar from it.*

The Russian bear's-breech from Kamtschatka (*Steracleum spondylium* Lin. *Heracleum sibiricum*) has been long known among the sacchariferous plants. This plant is said by Steller* to be, next to the sugar-cane, one of the most abundant in sugar. By the Russians it

* Steller's Reisen nach Kamtschatka, page 84. Also in dem Sibirisch-Asiatischen Magazin, I. Band. 3tes. Stuck. S. 412.

is called *sweet-herb*, and *kaisch* by the people of Kamtschatka. According to Gmelin it does not differ from the common *acanthus*, but according to others it constitutes a particular species (*sphondylium panaces*). The Kamtschadales gather its stems and the large stalks of their leaves in June, and separate them from the foliage, scrape off the outer rinds with shells of muscles, and dry them by exposure to the sun.

These dried stalks are usually chewed by the Kamtschadales, to suck out the saccharine substance. After the moisture of the juice has evaporated in the sun, the external surface of the stalks becomes covered with a white saccharine meal, which is separated by shaking them in leathern bags, and is preserved for the same uses as powder-sugar. Forty pounds of desiccated stalks are said to yield hardly so much as one-fourth of a pound of this mealy sugar, which consequently is a scarce article. Besides this use, the stems and roots of the bear's-breech are likewise employed by the natives of Kamtschatka for making brandy*.

That I might be enabled to make experiments on this subject myself, the late Professor Junghans at Halle sent me some young plants of the *Heracleum sibiricum*, which the year following were the subject of my enquiries.

However, as I did not find the stalks of this plant in any respect so rich in sugar as those growing in Siberia are stated to be, I collected their roots last autumn, the quantity being four pounds. Their taste is sweetish, like that of parsneps. I parted one root from its outer rind, and suffered it to dry, but could not perceive any saccharine crust; for this reason, I ordered all the four pounds to be grated, kneaded with water, and the juice to be then pressed out, which exhibited a sweetish, slightly sharp taste. It was next boiled with some white of eggs, and the clarified juice inspissated to the thickness of a syrup. A brown and not unpleasant syrup, amounting to six ounces, was in this way obtained, which, after three months, afforded brown granular crystals of sugar, contaminated, however, by a foreign flavour. Hence it is proved, that sugar may be produced from this plant; but that in an economical consideration it would not be serviceable either for sugar or for a syrup, as both would be too expensive: this plant, when growing in our soil, is probably less sacchariferous than that in Kamtschatka.

f. Experiments for obtaining Sugar from Must, or the Juice of Grapes.

Must, obtained by pressure from perfectly ripe grapes, evidently shews, by its sweet taste, the great quantity of saccharine matter it contains, though indeed enveloped in a large proportion of mucilaginous matter. In order to examine the possibility of producing true sugar from it, or least a syrup proper for use, the following operations were performed:

Eight *Berlin* quarts † of must, procured from completely ripe and sweet wine grapes by mere draining, were mixed with whites of eggs, and heated to boiling, and then filtered.

* Gmelin Flora Sibirica, tom. I. p. 214.—Allgemeine Reisen zu Wasser, etc. XX Theil. page 259.—Stralsund. d. d. Magaz. cit.

† The *Berlin* quart holds 58 cubic inches of the former French measure; the *English* wine-quart only 47½ of them.—Translator.

The clarified fluid afforded by evaporation three pounds of a syrup slightly sour, but not disagreeable. It was again dissolved in lime-water, for the purpose of depriving it of its disengaged acid, and lime-water was successively added, until the usual re-agents indicated no signs of acidity. The fluid was then again clarified with whites of eggs, and a second time evaporated. By this means I obtained a very pleasant syrup, but could not succeed in producing a crystallizable sugar. It must also be observed that it would not be possible to prepare this syrup in a profitable way, as long as the hogshhead of good wine-must can be sold for fifteen dollars.

g. Experiments made with the Juice of the black and white Birch, with the intention of manufacturing Sugar.

Opinions have hitherto been greatly divided, whether sugar can be obtained from the juice of birch-trees. Many affirm, but others deny it. This induced me to apply to experiment for a decision. According to *Stålhammer's* * observations, eight *kannen* † of the liquor drawn in the spring from the common white birch (*betula alba*) yield five and a half stop of a syrup, which is said to be weaker than that from the maple, but still better than the common brown syrup. On the contrary, according to *Kalm's* experience ‡, the North American black birch (*betula nigra carpinifolia*), which is also called sugar-birch, yields much sugar, but not so sweet as that from the maple. By the particular kindness of Count Veltheim at Harbke, I received a sufficient quantity of juice from both species of birch which served for the following experiments.

Fifty trees of the white birch, from eight to ten inches diameter, being tapped in the month of April, in which this tree flows the best, yielded, in four days, 140 Berlin quarts of liquor; which, by evaporation, produced 2½ lb of a brown syrup, of an unpleasant taste, from which no crystallized sugar could be obtained.

On the other hand, ten of the black birch trees being bored, afforded 50 quarts of juice in four days; from which when evaporated a pound and a half of a very good and serviceable syrup was obtained. Though much inferior to that from the maple, it was nevertheless superior to the common; and from this I am inclined to conclude, that *Stålhammer* employed the black birch for his experiments. During the period of four months, a considerable portion of sugar has crystallized from the liquor of the black birch; it is therefore proved, that real sugar may be prepared from the black birch; which, however, would be too expensive, and with respect to quality is far excelled by the maple-sugar.

* Transactions of the Royal Swedish Academy of Sciences, Vol. XXXV. page 335, seqq.

† A Swedish *kanne* is of 132 Parisian cubic inches capacity; a *stop* is its half, or 66 inches ditto.—

Translator.

‡ Transactions of the Royal Swedish Academy, Vol. XIII. page 151, seqq.

(To be concluded in our next.)

Observations

II.

*Observations on the various Kinds of Manure. By JOHN MIDDLETON, Esq.**

HAVING made experiments with various kinds of manure, on a farm of which I am the owner and occupier, situated at Merton in Surrey, for the purpose of ascertaining the most appropriate dressing for the soil, which is a tenacious loam, on a substratum approaching towards yellow clay. I am induced, by the regard I feel for the success of British agriculture, to request that you will be so obliging as to lay the following observations on the several experiments before the Society for the Encouragement of Arts, Manufactures, and Commerce, for their consideration. I hope and believe that they will be found not altogether unworthy their attention.

1. *Peat-ashes* from Newbury, Berks. Of these ashes I have spread, in various quantities per acre, 1500 bushels on wheat tares, seeds, and meadow-land, without being able to discover any beneficial effect from them.

2. *Coal-ashes*, spread on three or four acres of grass-land, in March, 1798, produced no visible effect at mowing time, nor have I since observed any.

3. *Wood-ashes*, the produce of my own fires, when spread on the grass in February, or early in March, I have found to be of some, though little, service.

4. *Malt-dust*, including the dust from the malt-kilns, I used for two or three years to an extent sufficiently great to ascertain that the benefits produced by the use of it are considerable. It may be applied in such a quantity as to ensure *one large crop*; but on meadow-land, even when hay is at five pounds a ton, it only repays the prime cost.

The quantity which I have usually laid on has been in proportion of from fifty to sixty bushels per acre. The first coat of kiln-dust is sixpence, and of smaller dust eight pence per bushel: including the expence of carriage, and spreading this dressing on the land, it amounts to about two guineas per acre. The extra crop returned me this sum, but without profit.

5. *Soot*. Of this manure I spread eight hundred bushels over twenty acres of wheat in one year; but I could not, from the subsequent appearance of the crop, discover whether the increase in quantity was equivalent to the additional expence. However, it was evidently of some use; but to what extent would require more than bare inspection to ascertain. By way of comparison, some of the ridges were left without soot: they were at harvest scarcely to be distinguished from the rest; but where the soot lay in larger quantity than ordinary, as was the case in the places at which the loads had been shot from the carts, the superior vegetation was very distinctly marked. I have, on the whole, formed the same opinion with respect to this species of manure as I have already stated in regard to malt-dust, namely, that it returns the cost price with very little profit.

* Society of Arts, 1799. p. 231, addressed to the Secretary.

6. *Soap-makers' waste.* I have tried only one load of this manure on a few roods of ground in four of my meadows. It has not produced the least effect, although it is now three years since it was laid on. Soap-makers' waste, potash, and barilla, are probably held in too much esteem as preparers of the food of plants, by philosophical chemists, of whom it might be wished that a little *practice* were combined with their *theoretical* ideas on the science of agriculture, that they would try their spurious theories by the test of experiment, before they publish them to the world.

I am further induced to consider this kind of dressing for land as of much less utility than is generally imagined, from having been informed by Mr. Ruffel, junior, that his father, who is a soap-maker of great respectability, at Paris Garden, has used the waste of his own manufactory on his farms in Essex and Kent (in the latter one a clay soil), without discovering that it was of any material benefit to the land; and that he has consequently discontinued the use of it.

The experiments made by major Valley, as reported in the eighth volume of papers published by the Bath Society of Agriculture, seem also to prove that Dr. Hunter's food of plants does not answer *any* of the purposes for which it has been so highly extolled; but, on the contrary, that it is really *hurtful* to corn crops.

7. *Sweepings of London streets.* I have used several hundred loads of this manure on grass-land, and have found it to be of considerable service to the succeeding crops. I have usually laid it in large heaps, and mixed with it a small quantity of horse-dung: and in this state it generates a little heat, though less than might be wished, which helps to decompose or rot the mixture: when thus prepared, it has been spread on the land in proportion of ten or twelve loads per acre.

8. *The soil of privies.* Within the last four or five years this manure has been spread on my land to the expence of about 100l. The proportion from two to four loads per acre. The effect produced by it was *astounding fertility*; so much so, as to induce me to be of opinion that it exceeds every other kind of manure that can be brought into competition with it, at least for the first year after it is laid on. In the second it is of some service, but in the third its effects very nearly, or entirely cease. From these premises I draw this conclusion, that for land in good condition the application of two loads per acre per annum will continue it in that state for any length of time. And also, that land which has been much exhausted might be restored by laying on four or five loads per acre; after which, a repetition of two loads annually would be found sufficient to keep it in the highest degree of fertility.

9. *Farm-yard dung.* This, when it had been once turned, and become about three-fourths rotten, I have used in the proportion of about *thirteen or fourteen loads* per acre; and found it much less effective for *one* year, than *three loads* of night-soil. I believe, that even a load and a half of soil would have been equal to the foregoing quantity of dung. In the *second* year I could not perceive any difference between the dung and the soil.

In the last volume of the Transactions of the Society, page 168, a crop of wheat, amount-

ing to 56 bushels per acre, is said to have been raised by Mr. Henry Harper of Lancashire; which is so much above the general average, that Mr. Harper was at a loss how to account for it. I am inclined to think, that the *night-soil* contained in the mixture with which he dressed the clove was the cause of this *wonderful effect*.

He mentions, that the quantity of manure (consisting of *night-soil*, coal-ashes, and sweepings of streets) was eighty tons; and that the clove contains eleven acres. The proportion per acre was therefore something more than seven tons. He does not say what part of this proportion was night-soil; but it was probably not less than four tons; a quantity which, as I have before observed, is sufficient of itself to produce one immense crop.

In short, it appears to me, that nature following her general system of reproduction, prepares this matter in the most perfect manner, for the purpose of feeding vegetables, and raising them to the very highest pitch of excellence. And it is certain, that herbage growing under these circumstances is capable of fattening the *largest* cattle in less time than any other.

The importance of this kind of manure being so evident, I am sure the society will feel equally with me the most poignant regret, when they take into their consideration, that ninety-nine parts in every hundred of this valuable article is constantly and most *absurdly* carried by the sewers and drains into the rivers, and thereby totally lost to the purposes of agriculture, for which it is so admirably adapted.

In Britain alone, the quantity of the manure and of urine which is annually thus wasted is astonishingly great; probably not less than five millions of cart-loads: worth to the farmers two millions and a half; and to the community five millions of pounds sterling per annum.

III.

*Description of a New Escapement for Clocks. By Mr. SIMON GOODRICH.**

FIG. 1. Plate XV. represents a front view of the crank escapement.

Fig. 2, a side view of ditto.

The same letters of reference are put to the corresponding parts in each figure.

A A A A, the back plate of a clock. B, the crank, fixed to the end of C, the arbor of the last pinion of the train. D D, two small wire chains, composed of two links only, attached to the crank B, by means of two collets, with plenty of liberty upon it. These chains are fastened to E E, two small springs screwed to F F F, a bar that goes across, and is fastened in the middle to G G, the crutch with which H H, the pendulum, is suspended in the common way. I I, are two screws going through the bar F; the ends of which being

* Society of Arts, 1799, p. 329.

made to act against the springs E E, serve to adjust them to a proper distance and degree of strength; and by that means easily to put the pendulum in proper beat. In fig. 1, the pendulum is partly represented by dotted lines, in order to shew the crank, &c.

The advantages of this escapement are, 1st, simplicity of construction; and consequently cheapness. It can easily be adopted by the manufacturers of the common wooden Dutch clocks, as the whole escapement can be formed of a few pieces of wire bent till properly adjusted, without even the necessity of the adjusting screws I I. Therefore the benefits of this invention may extend even to the poorer part of the public.

2^{dly}, Total silence when going: clocks being excluded by many persons to whom and from many places where useful, upon account of their noise.

3^{dly}, Keeping good time, arising from this principle, that the crank always impels the pendulum in the same manner; that is, as it passes through the semicircle under its centre always one way; and as it passes through the semicircle over its centre always the other way; and it recurs in the same situation it set out from, after these two impulses; which being easily made equal by means of the adjusting screws, notwithstanding the small difference in length of the levers acted upon, all the succeeding impulses must also correspond with these, and be equal and regular, as far as regards the escapement itself. In other escapements, if any irregularity exists, or is worn in any one or more of the teeth or pins in the scape-wheel and the pallets, or inclined planes they act against, the impulse given to the pendulum cannot be equal and regular; and the higher the number of the scape-wheel, the greater this chance of imperfection.

4^{thly}, Durability and great certainty of continuance. This escapement does not require oil; which, as it becomes clammy and thick by the dust, is the frequent and well-known cause of the stopping of others: it does not wear itself untrue and out of order. The friction of the collets upon the crank must be very small, as the acting surface is small; and as the intensity of the impulse upon the pendulum is in proportion less in this, as it acts upon a longer lever than in others; also, at the time that the chief force is exerted, as the crank passes over and under its centre, there is very little turning of the collets upon it: so that I judge this escapement, properly made, stands a fair chance of lasting as long as the train itself, without being out of order; which I understand is far from being the state of others.

The ingenious part of the public can no doubt improve upon this application. Of the various methods I have tried, this appears to me to be the best for general use. I can send the Society drawings * of the other methods which I have tried, if by this they should judge them worthy their notice. In the one, the crank, besides its own, can be applied with all the advantages of a dead beat; and in the other, of a detached escapement: but they will not have the advantage of silence.

I was induced to apply this motion to a ready-made clock, in order to try, by the same machine, a point of which I had some doubt; namely, that perhaps, on account of the greater velocity of the acting part, this method might require more power than the common. The

* The drawings here alluded to, as also a model of another escapement by Mr. Goodrich, are reserved in the Society's collection.

experiment proved in favour of the crank; though with the common escapement, well adjusted, I kept the pendulum in motion for some days, with as small a power as I could with the crank; yet the same small power with the common escapement was not sufficient to maintain the motion so long as it was with this crank. The weight here used for this eight-day piece, which has kept it going for about six months without once failing, is $4\frac{1}{2}$ lb. It has in it an allowance of more than half for the loss of power from foulness, or other accidents; for three pound was sufficient to work the clock for several weeks when first put together.

IV.

Description of the Furnace of POTT.

TO MR. NICHOLSON.

SIR,

MUCH has been said on the theory and construction of chemical furnaces, and many ingenious contrivances have been made on that head, especially for philosophical purposes and experiments in the small way; yet, to my knowledge, there is none by which in so simple a manner, and moderately small size, the heat can be increased to such intensity as by Pott's furnace.—I mean *Pott* the famous Prussian chemist, which, though his researches on the more simple earths and stones were chiefly confined to their degree of fusibility, or also of their being fire-proof, may nevertheless be considered as the father and forerunner of those admirable analyses of mineral substances, for which the present period is indebted, after *Margraf* and *Gellert*, to *Gerhard*, *Achard*, *D'Arcet*, and especially to *Klaproth* and *Vauquelin*.

That furnace, it seems, has been long since forgotten; and perhaps it may be of service to the chemical public to be again put in mind of it. On this account I send a drawing, and translation of the description, which *Pott* himself published of his furnace. If you are of the same opinion, and think it may be agreeable to the readers of your valuable and useful Journal, you are requested to insert it.

Sir, your humble obedient servant,

N. N.

15th October, 1799.

An Account of Pott's Furnace, given by himself.

When I affirmed in my treatise on the *Lithogeoognosia**, that the culinary and common

* John Henry Pott's *Lithogeoognosia*, or chemical enquiries into the nature of the common more simple stones and earths, etc. 4to. Berlin, 1784, Continuat. 2d. By the shape of this furnace, it appears that its inside, after being coated, has the form of the cavity of an egg, or nearly elliptical, which was the favourite shape with *Boerhaave*, for furnaces intended to produce a strong heat.

fires

fires are not sufficiently strong for those operations and fusions which I have described in that book, I meant to comprize in that assertion, not only the furnaces of the medical chemists, but even those of the manufacturers of glass and porcelain. For in those furnaces, though the fire be kept up several days, several substances can never be fused, which in my furnace I could bring into the state of fusion by a fire maintained for *two hours* only: such as the *Oriental* and *Bohemian garnets*, and even *hyacinths*.

My furnace is nearly of the same construction as that which Becher has described in his *Tripode Hermetico*, page 32. The body of the furnace, A, fig. 3, plate XV. is made of strong sheet-iron, that it may better resist the action of the fire. Its inside is coated with crude white clay, which is worked up, by means of bullocks' blood, with an equal portion of baked clay.

B is likewise constructed of sheet-iron, and internally coated in the same manner as A, upon which it is placed. Through the small door D the coal is put in. Into the iron tube G, serving as a flue, another, H, of the same material is inserted, which at least should be six feet long. The longer this pipe is, the stronger is the action of the fire; and it should be fastened in the chimney by means of an iron chain, to prevent it from overturning the furnace by its weight.

If it be desired to increase the force of the fire, another pipe, C, must be joined to the door of the ash-pit, E, in such a manner, that the other extremity of C, ending in the form of a funnel, may reach beyond the window, in order that the cold air may be conveyed to the furnace from a considerable distance.

The charcoal, to be employed in this furnace, should be of the size of a hen's or goose's egg: larger or smaller pieces are improper, and may be used for other purposes. The furnace must be filled with the fuel nearly to its top, in order that the crucible be surrounded on all sides by burning coals, and that the fire may thus exert its whole power. Nor must it be forgotten to supply fresh coals at least every eight minutes, or oftener, and to shut the door every time quickly and exactly. In this manner, any substance whose nature is susceptible of fusion, may be fused within the time of two hours.

V.

*Process used to separate the Mucilage from Linseed Oil; Methods of dissolving Copal for Varnish, &c. By Mr. TIMOTHY SHELDRAKE.**

IHAD read in some book, the title of which I do not recollect, that linseed-oil might be purified by shaking it with water, which would imbibe the impurities, and leave the oil more purified. I tried this experiment by shaking linseed-oil with warm water, and was surprized to find they did not separate, but remained united in the form of emulsion. I then boiled them together, and found their tendency to separate diminish. As it is the known pro-

* Society of Arts, 1799, page 283.

perty of gums, or mucilage, to keep oil and water united in this state, I was induced to suspect the preservation of mucilage in linseed-oil.

I had read in Dossie a method of preparing what he calls *fat oil*. It is effected by placing linseed-oil in a shallow vessel exposed to the heat of the sun, and stirring it frequently; in a certain time it loses its property of drying, thickens, and acquires a degree of tenacity that makes it proper for a size, or cement, for gilders, &c. A similar substance is alluded to by Leonardo da Vinci. (Philos. Journal, II. 90.)

Taking the existence of mucilage in this oil for granted, I conjectured that the alteration produced in its texture by Dossie's process, arose from the evaporation of some of its principles, and the more intimate union of the rest, in consequence of that evaporation: to verify this conjecture I tried the following experiment. I filled a half-pint phial full of linseed-oil, corked and tied it securely over with a bladder. This I exposed to the heat of the sun in summer, during the whole day: after it had remained a few days in this situation, the upper part of the phial was covered with drops, similar to those produced by holding a wet bottle to dry before the fire. I then shook it well, which made the contents look muddy, and set it to rest again. After a time it became clear, and a portion of transparent liquor, like water, lay at the bottom. I then repeated the shaking and setting it to rest, till no additional quantity of this fluid was separated.

By trying this experiment repeatedly upon oils procured from different places, I found that some oils afforded much more mucilage than others. From some I separated a third part of mucilage; from others a pint would not afford more than a table-spoonful, and sometimes less. Whether this difference in the result was radically in the oils, or from a difference in the processes conducted by means so variable as the heat of the sun, I am not able to ascertain.

Upon trying the same experiment with nut and poppy oil, I found the same result, but in a different degree. The average quantity afforded by nut-oil was, I believe, not more than a third part of the average of linseed-oil, and the average of the poppy-oil was not so much as a sixth. In some cases, particularly of the poppy-oil, I did not obtain any.

The colour of the oil always diminished as the mucilage was abstracted; but the mucilage was always as colourless as water. It is a question I will not pretend to decide, whether the colour of these oils depends upon the presence of the mucilage, or upon any other principle which is destroyed by the action of light. I have, in some instances, had the oil as colourless as water,

This decomposition of the oils, if it may be so called, is curious, as proving the existence of mucilage in them; but as it is very troublesome, may it not be advisable to prefer those which have naturally the least mucilage in their composition?

After pouring the oils from the mucilage, I put several quantities of the latter together, and found them mix without any difficulty. I mixed this mucilage with water, and found it unite with it in any proportion, without becoming turbid. I laid it upon plates of metal, exposing

exposing them to dry in the sun and before the fire, and when dry, washed them with a sponge and water; but it shewed no tendency to dissolve; though while in a liquid state it seemed to possess all the properties of a gum.

*As the processes by which I dissolved the amber and copal to make the oil varnishes are to be found in many books, and as it will be better for artists to purchase than attempt to make those varnishes, it can scarcely be thought necessary to detail those processes here; but as I believe the methods by which I dissolved the copal in spirit of turpentine and spirit of wine are not known, I shall now subjoin them.

To dissolve Copal in Spirit of Turpentine.

N.B. Whatever quantity is to be dissolved should be put into a glass vessel, capable of containing at least four times as much, and it should be high in proportion to its breadth.

Reduce two ounces of copal to small pieces, and put them into a proper vessel. Mix a pint of spirit of turpentine with one-eighth of spirit of sal ammoniac; shake them well together; put them to the copal, cork the glass, and tie it over with a string or wire, making a small hole through the cork. Set the glass in a sand-heat, so regulated as to make the contents boil as quickly as possible, but so gently that the bubbles may be counted as they rise from the bottom. The same heat must be kept up exactly, till the solution is complete.

It requires the most accurate attention to succeed in this operation. After the spirits are mixed, they should be put to the copal, and the necessary degree of heat be given as soon as possible. It should likewise be kept up with the utmost regularity. If the heat abates, or if the spirits boil quicker than is directed, the solution will immediately stop, and it will afterwards be in vain to proceed with the same materials; but if properly managed, the spirit of sal armoniac will be seen gradually to descend from the mixture, and attack the copal, which swells and dissolves, except a very small quantity which remains undissolved.

It is of much consequence that the vessel should not be opened, till some time after it has been perfectly cold. It has twice happened to me on uncorking the vessel, when it was not warm enough to affect the hand, that the whole of the contents were blown with violence against the cieling. It is likewise important that the spirit of turpentine should be of the best quality. I have never succeeded with that which is sold at the colour-shops; but whenever I procured my spirits at Apothecaries-hall, I have dissolved the copal by the process I have described without difficulty.

This varnish is of a rich deep colour when viewed in the bottle, but seems to give no colour to the pictures it is laid on; if left in the damp, it remains sacky, as it is called, a long time; but if kept in a warm room, or placed in the sun, it dries as well as any other turpentine-varnish; and when dry, it appears to be as durable as any other solution of copal.

To dissolve Copal in Alcohol.

Dissolve half an ounce of camphor in a pint of alcohol; put it in a circulating glass, and add four ounces of copal in small pieces; set it in a sand-heat, so regulated that the bubbles
may

may be counted as they rise from the bottom ; and continue the same heat till the solution is completed.

Camphor acts more powerfully upon copal than any substance that I have tried. If copal is finely powdered, and a small quantity of dry camphor rubbed with it in the mortar, the whole becomes, in a few minutes, a tough coherent mass. The process above described will dissolve more copal than the menstruum will retain when cold. The most economical method will, therefore, be to set the vessel which contains the solution by for a few days ; and when it is perfectly settled, pour off the clear varnish, and leave the residuum for a future operation.

This is the brightest solution of copal that I have seen ; it is an excellent varnish for pictures, and may, perhaps, be found to be an improvement in fine Japan works ; as the stoves used in drying those articles may drive off the camphor entirely, and leave the copal pure and colourless on the work.

N.B. Copal will dissolve in spirit of turpentine by the addition of camphor with the same facility, but not in the same quantity, as in alcohol.

At the time I determined to lay the preceding papers before the society, I conceived that the quick and certain manner in which the vehicle dried was one of its advantages : but as that circumstance has been objected to, and in some cases really is a disadvantage, I have since endeavoured to remove that objection by the following process.

Put a pint of nut or poppy oil into a large earthen vessel ; make it boil gently upon a slow fire ; put in, by degrees, two ounces of ceruse, and stir it continually till the whole is dissolved.

Have ready a pint of the copal-oil-varnish heated in a separate vessel ; pour this by degrees into the hot oil, and stir them together till all the spirit of turpentine is dissipated ; let it then be set by till cold, when it will be for use.

It is obvious that as this is a compound of the copal-varnish with the least exceptionable of the drying oils, it will partake of the properties of each of its component parts. It gives less brightness and durability to colours than the varnish will, but more than oil : but as it certainly may be used in painting in the same manner as any other drying oil, and gives more durability and brightness to colours than they can derive from any other oil, it is not unreasonable to suppose that it will prove an advantageous vehicle.

I have mentioned specific quantities of the ingredients ; but it is easy to see that the relative proportions may be varied according as it is required to dry faster or slower. It must be remarked too, that wherever the mixture is to be made, both the ingredients should be hot ; because if either of them is cold, the mixture becomes turbid, and a part, often the whole of the copal is precipitated : but this inconvenience is avoided by mixing and boiling them together as I have directed. It must likewise be observed, that after some time a spontaneous alteration takes place, which diminishes, and at last destroys, the drying quality of this mixture ; and it will, therefore, be advisable to use it fresh ; or at least not to use it after it has been made more than a month, or six weeks.

Analysis

VI.

*Analysis of the Spinel. By Professor KLAPROTH. **

THE spinel appears to me to be among those precious stones which ought to be placed in the class of hyacinths of the ancients. For they do not describe its colour as being a red inclining to yellow, like the hyacinth of the moderns, but as being of a clear violet, and also of a rose colour. We read, for instance, in Pliny †, *Multum ab amethysto distat hyacinthus, tamen è vicino descendens. Differentia hæc quod ille emicans in amethysto fulgor violaceus dilutus est in hyacintho.* Epiphaius reckons five kinds of hyacinths, of which he terms the third *ῥάτιβος* ‡, and Salmasius describes its colour in these words, *inter roseum est et dilutiorem.* The modern hyacinth has been confounded with that of the ancients, no doubt, from the following passage of Pliny §, *Hyacinthos Æthiopia mittit et chrysolithos aureo colore translucentes*: but this, by a well-founded criticism, may be read thus, *Marcescens celerius nominis sui flore hyacinthus. Æthiopia mittit et chrysolithos, &c.*; in which case, the *aureus color* of the latter, which is known to be the modern topaz, will have no application to the hyacinth, and we shall have at the same time one contradiction less in Pliny.

The spinel has hitherto been classed among the rubies as the second species of that genus, the true ruby being the first. But since Romé de l'Isle || has shewn the different crystallization of these two stones, and has shewn that the figure of the ruby, of which Pegu is the country, is an elongated double hexahedral pyramid, modern mineralogists have classed it among the sapphires as a red variety, and have considered the spinel as a particular genus; a distinction which the differences of hardness and specific gravity appear also to point out.

In doubtful cases, the fracture may determine the species. The spinel has a foliated fracture, the plates being disposed in three different directions; but the sapphire has a conchoidal fracture on all sides **.

Chemical analysis is always the most decisive. But the scarcity of the ruby in its natural state, crystallized in the hexahedral pyramid, or the red sapphire, leaves us little reason to hope for success in this respect.

The primitive figure of the spinel is an octahedron, or double tetrahedral pyramid. This crystallization is rather frequently found in a perfect state, but much oftener with some con-

* *Beitrag zur chemischen Kenntniss*, vol. II.

† *Lib* 37, c. 9.

‡ From the Arabian word, signifying the colour of red chalk. See Joan de Laet de Gemmis et Lapid. Lug. Bat. 1647.

§ *Loc. citato.*

|| *Crystallographie*, I. 213.

** *Mineralogy of Estner*, II. sect. 1. p. 96, 97.

derable variations. All these different crystallizations are very carefully described by the Abbé Eftner*.

The colour of this stone is also much varied, and passes through all the gradations of red. This variety of colour has caused lapidaries to distinguish it by different names, such as the ruby, almandin, spinel, balais, rubicel.

The red colour of this precious stone is not only very fixed in the fire, but it even becomes deeper by careful ignition of the pale varieties. According to the testimony of Julius Scaliger†, the inhabitants of Zealand are very well acquainted with the method of profiting by this, which is, perhaps, the only reason why we so seldom observe in the rough ruby the fine and beautiful colour of those which have been cut.

With regard to their colour, those spinels are the most rare which are beautifully transparent and colourless, such as the perfect octahedron which Mr. Macie of London preserves in his collection; in the next place are those of a sapphire-blue, such as that of Mr. F. Greville; and next the green, as that possessed by Mr. Hawkins, &c. This stone affords a new proof that the colour of precious stones is a very inferior character for determining their classification.

I found the specific gravity of the spinel in select crystals to be from 3,570 to 3,590.

Though I have published an analysis of the spinel several years ago‡, there are nevertheless several matters which were not, at that time, well known, and appeared to me to require a new course of experiments. The event has shewn me that, in my former work, I had overlooked a substance not suspected to exist in this stone, namely, magnesia. Among other analyses made to determine the quantity of this new principle, I shall here give that which appeared to me to deserve the most confidence.

A. One hundred grains of rough spinel of Zealand, in select crystals, were pounded in a steel-mortar, and then reduced to an impalpable powder by grinding them in a mortar of flint with a little water. The powder being then dried and slightly ignited was found to have gained nine grains by erosion from the mortar.

B. On this powder I poured 960 grains of muriatic acid, and set them to digest. After the acid was evaporated nearly to dryness, I diluted the mass with water, and poured the whole on a filtre. The acid liquor was yellow. I supersaturated it with ammoniac, which threw down brown flocks of iron, that weighed one quarter of a grain after ignition.

C. The liquor separated from this precipitate was evaporated, saturated with muriatic acid, and mixed with oxalate of potash. A precipitate of oxalate of lime fell down, which was carefully collected, placed in a hole in charcoal, and ignited by the blow-pipe. It afforded three quarters of a grain of pure calcareous earth. When this was dissolved in the nitric acid, and mixed with the sulphuric, it afforded sulphate of lime.

* Mineralogy of Eftner, p. 73, et. seq.

† Exercit. CXVIII.

‡ Beobacht und entdek, &c. Berlin, 1789, vol. III. page 336.

D. The powder thus extracted by muriatic acid, was put into the silver crucible, with ten times its weight of a solution of caustic potash, of which the alkali constituted half the weight. The mixture was evaporated to dryness on a sand bath, and afterwards ignited for an hour. The mass diffused in boiling water, left on the filtre a residue of a fawn colour, which, dried in the air, weighed 54 grains.

E. These 54 grains again treated with caustic potash, and the mass diffused in water, left a residue in very fine powder, which dried in the air weighed 43 grains.

F. The alkaline solutions D and E, which were of a yellow colour, were mixed together, saturated with sulphuric acid, and the precipitate which was formed redissolved with a slight excess of acid. The carbonate of potash occasioned, in this solution, a very voluminous precipitate, which, after having been well washed, was redissolved in the sulphuric acid. The solution resembled a mucilage: by exposure to heat, it was perfectly liquified, and deposited a white powder, which collected, washed, and dried in the air, weighed 95 grains. The acid which had been separated was set aside.

G. I slightly ignited these 95 grains, with three times their weight of caustic potash. The whole being afterwards diluted with water and filtered, a very small portion of residue was left, which, after washing, was dissolved in the sulphuric acid, except a few flocks.

H. The part dissolved by the potash G, was precipitated by sulphuric acid, and redissolved in excess of acid, and afterwards precipitated hot by carbonate of potash. The precipitate being well washed, was again dissolved in sulphuric acid.

I. The sulphuric solutions F, G, H, were evaporated. The liquor having assumed the gelatinous form, indicated that filix had been separated. It was therefore diluted with much water, then concentrated, and the filix collected on the filtre.

K. I evaporated the acid solution of the sulphate. After having added the acetite of potash to obtain crystals*, a perfectly pure alum was formed; but the solution having assumed a greenish colour towards the end, I added prussiate of potash. A slight blue precipitate was afforded, in which the oxide of iron might be estimated at one fourth of a grain. The solution, cleared of iron, was decomposed by the carbonate of potash. The precipitate being again dissolved in sulphuric acid, afforded to the end perfectly pure crystals of alum, which were added to the former.

L. I then undertook the decomposition of the 43 grains, E, and which remained insoluble in the potash. Having put them into diluted nitrous acid, they were dissolved, and left only a small portion of filix. The solution, after having been separated from this last, was mixed with a small portion of the acetite of potash, and left to spontaneous evaporation. Some more small crystals of alum were formed, after which the solution afforded only crystals of sulphate of magnesia.

* In order to add to the sulphate of alumine the quantity of potash necessary for its crystallization, I at present employ the acetite of potash. By this means I avoid decomposing part of the alum (which might be already formed) by adding more potash than necessary.

M. To separate the sulphate of magnesia from the sulphate of alumine, I strongly ignited the whole in a porcelain crucible for half an hour; after which I diffused the saline mass in water, and filtered it. The alumine thus separated, was dissolved in the sulphuric acid, and crystallized in alum.

N. The solution of pure sulphate of magnesia was precipitated hot by the carbonate of potash. The carbonate of magnesia washed and dried, weighed $20\frac{1}{2}$ grains; but after strong ignition, there remained only $8\frac{1}{4}$ grains of pure magnesia.

O. The waters used in the washings (of which that afforded by the decomposition of the sulphate of alumine F, had preserved its yellow colour), were all evaporated to dryness. The saline mass, diffused in water, again let fall a little earth, which I added to the small quantity of flocks remaining after the experiment G. This being urged by a red heat with caustic potash, and treated with the sulphuric acid, was decomposed into silex and alumine.

P. All the alum obtained in the experiments K, L, M, and O, which weighed 665 grains, was dissolved in water, and the boiling solution decomposed by carbonate of potash. The alumine washed with boiling water, and dried by a gentle heat, weighed 221 grains. But after being purified by digestion in acetous acid, and saturating this last with ammoniac, it was again well washed, and strongly ignited for a quarter of an hour. Its weight was then only $74\frac{1}{2}$ grains.

Q. The silex of the experiments I, L, and O, collected and ignited for half an hour, weighed $24\frac{1}{2}$ grains. Deducting the nine grains of increase afforded by the mortar A, there will remain $15\frac{1}{2}$ grains for the part afforded by the spinel.

This analysis, therefore, gives for one hundred parts of the spinel :

Alumine (P)	-	-	-	-	-	-	-	74,50
Silex (Q)	-	-	-	-	-	-	-	15,50
Magnesia (F)	-	-	-	-	-	-	-	8,25
Oxide of iron	{ (B)	1,25 }	-	-	-	-	-	1,50
	{ (K)	0,25 }	-	-	-	-	-	
Calcareous earth (C)	-	-	-	-	-	-	-	75
								<hr/> 100,50

The increase of half a grain, which is here found contrary to the rule, since there must always necessarily be some loss, can only arise from the dryness of the stone in its natural state, which cannot be given to its separate parts even by strong ignition.

I must here mention a phenomenon which occurred in another analysis of the spinel, which guided me to the preceding, and of which the description may not be totally useless. After having treated the spinel with potash and the muriatic acid, the earth precipitated from the muriate by ammoniac was thrown into a solution of caustic potash. The residue was dissolved in the muriatic acid, and after having separated a small portion of silex, the solution was concentrated by evaporation.

At

At the end of several days small groups of crystals were separated, composed of small flat tetrahedral prisms, most of them joined two and two, in the form of St. Andrew's crosses. Others were joined, to the number of three or more, in the form of a cross.

They were very soluble in water. When decomposed by ammoniac, the precipitate appeared of a light brown, because contaminated by a small portion of oxide of iron. This being dissolved in the sulphuric acid, and the new combination concentrated, the crystals of alum were first separated; after which the liquor afforded only crystals of magnesia, in tetrahedral prisms.

This crystallization of the muriate of magnesia containing alumine appeared to me to be very remarkable.

VII.

*Verbal Process of the Conversion of Soft Iron into Cast-steel, by Means of the Diamond. By
CITIZEN GUYTON.**

THE class may recollect the account † I gave of the great experiment of the combustion of the diamond in oxygen gas, in the focus of the lens of Tŕirnhäufen, and the new truths I have deduced respecting the true nature of the diamond; of plumbago, which is its oxide in the first degree; of charcoal, which is its oxide in the second degree; and of carbonic acid, which is the product of its complete oxygenation. These experiments gave our brother the citizen Clouet the idea of seeking a new kind of additional proof, by attempting to cause soft iron to pass to the state of steel, by cementation with the diamond.

It has hitherto been considered as a decided fact, that iron does not melt but in its transition to the state of steel, or cast-iron; but in what state does the carbone enter into this combination? It may be conjectured that it is in the state of plumbago, or oxide of the first degree; because that which is separated by acids is found to possess the brilliant black colour, and incombustibility, which form its principal character. Hence it appeared proper to conclude that the carbone entered into this union in the state of oxidule; and consequently that the charcoal employed in the cementation of steel began by parting with a certain portion of its oxygen. Of this there was also an indication of considerable strength in the charcoal which had been used for this operation: as in fact it has a more brilliant aspect, and resists incineration nearly in the same manner as charcoal in the mass after strong ignition in close vessels.

* Read at the sitting of the National Institute of France, 26 Thermidor, in the year VII. and inserted in the *Annals de Chimie*, XXXI. 328, whence the present translation is made.

† *Philos. Journal*, III. 298.

But if the charcoal be really unburned in the cementation of iron, there must be a disengagement of oxygen gas. I have endeavoured to resolve this question by experiment.

I cemented small pieces of iron in a porcelain retort, which had received by a previous operation a vitreous coating, and consequently was no longer permeable by the air. These pieces of iron were surrounded on all sides by very dry powdered charcoal of beech.

The retort was placed in the reverberatory furnace with an adjutage, bearing a syphon, plunged beneath the surface of mercury, under an inverted glass vessel. A large quantity of elastic fluid was disengaged, consisting of carbonated hydrogenous gas, and carbonic acid; the latter of which was at the commencement 0,11 of the volume; towards the middle 0,13; and towards the end 0,15.

The conversion of iron into steel being but little advanced, after the fire had been continued for three hours and a half, the same iron and the same charcoal were again put into the retort, and subjected to the heat of a large furnace, urged by three bellows-pipes. There was now no more than a very small quantity of gas; but it was carbonated hydrogen, mixed with carbonic acid, with a progressive flow increase; so that the latter, which at first formed only the 0,07 of the bulk, amounted in the last portions to 0,12. The iron was now converted into steel, and the pieces were even joined together by a commencement of fusion.

It is very probable that part of the carbonic acid, collected in this operation, was formed at the expence of the remaining charcoal, with oxygen which was disengaged; but the constant presence of hydrogen leaves nothing certain in the result, but the difficulty of entirely depriving the charcoal of the last portions of water it contains.

I must here observe, that this experiment does not appear easy to be reconciled with the opinion of some chemists, that hydrogen has a greater attraction for oxygen than charcoal has; an opinion founded on the consideration that coal is precipitated in the eudiometer of Volta, when a mixture of oxygen and carbonated hydrogen gases are detonated, and the quantity of oxygen is not sufficient for saturating the two bases. I say that this elective attraction does not take place in my experiment, in which it cannot be doubted that the temperature is sufficiently elevated to produce water by the union of oxygen and hydrogen, and there is nothing in this case to give a preference of the oxygen for the carbone.

This consideration appeared to me to give a new interest to the experiment proposed by Citizen Clouet. I did not hesitate to use one of the diamonds of the polytechnic school for this purpose, pursuant to the authority I had received from the council; being persuaded that if it should disappear in this operation, by the mere exposure to an elevated temperature, in contact with iron, without the presence of air, nor any other oxygenating matter, the fact would well repay the sacrifice. Besides which, its form, colour, and irregular crystallization, rendered it of little value, even as an object of instruction.

Citizen Clouet had himself prepared a small crucible of soft iron, expressly forged out of chosen heads of nails. Its form was that of an eight-sided solid. See fig. 1. Plate XVI. and it was closed by a stopper of the same metal, well fitted, fig. 2.

This crucible was intended to be placed in a Hessian crucible, furnished with a cover,
well

well luted on. Such was the whole apparatus of the experiment; and I cannot better describe the result than by giving the verbal process which was drawn up by Citizen Clouet, Welter, and Hatchette.

Verbal Process of the Experiment, made at the Polytechnic School, the 25th Thermidor, in the Year VII. on the Conversion of Iron into Steel, by the Diamond.

The diamond employed weighed 907 milligrammes (14 grains). As it did not fill the entire capacity of the iron crucible, the remaining space was filled with filings of the same iron out of which it was formed. The crucible was closed with its iron stopper, which was driven in, to diminish as much as possible the space occupied by air.

			grammes.
The crucible and its stopper weighed together	-	-	55.8
The filings which covered the diamond	-	-	2.
			<hr/>
Total weight of iron enveloping the diamond	-	-	57.8

After having taken off the projecting part of the stopper*, the crucible was placed alone, and without the addition of any surrounding matter, in a Hessian crucible, and this in a second crucible of the same earth, but the interval between the two crucibles was filled with a siliceous sand, exempt from any mixture of iron. Lastly, the largest crucible was luted with earth, made of pounded crucibles and crude clay, and the whole was exposed about an hour to the heat of the forge furnace urged by three bellows-pipes.

When all was cold, the crucible of iron was found in the interior crucible converted into a button of cast-steel. See fig. 3. Plate XVI. It formed, together with the stopper and the filings, one simple mass, rounded, and well terminated, except a few globules which were detached, and of which the weight was only 884 milligrammes.

			grammes.
The button of cast-steel weighed	-	-	55.500
Detached globules	-	-	0.884
			<hr/>
Total weight of steel obtained			56.384, or grains.

The iron and the diamond weighed before the operation 58.707 grammes, whence it follows, that there was a loss of iron of about 2.423 grammes. This iron had given the Hessian crucible the colour of plumbago.

(Signed) *Clouet, Welter, and Hatchette.*

* This portion, as well as the remainder of the bar of which the crucible had been formed, was exhibited to the class, to shew the nature of the iron made use of.

The fusion of the iron being perfect to such a degree as to exhibit the rudiments of the most beautiful crystallization on its surface, it cannot possibly be imagined that any part of the diamond could remain unchanged in the mass, without entering into a state of intimate combination, because the difference of specific is repugnant to such a notion.

The diamond, therefore, disappeared by the attractive force exerted upon it, by virtue of the elevated temperature to which both were exposed, in the same manner as one metal disappears in its mixture or alloy with another.

Consequently, the diamond did in this experiment afford the same principle as the charcoal, because it produced the same compound with iron.

The conversion into steel is out of doubt, for the button having been laid bare by the mill of the lapidary, a drop of weak acid immediately-produced a spot of an obscure grey colour, absolutely similar to that produced on the English cast-steel, and that fused by the process of Cit. Clouet. They who have often touched steels by this kind of proof, long since pointed out by Rinman, have had occasion to remark that the spot on cast-steel*, though very perceptible, is not so black as on steel made by cementation; a circumstance which probably depends on the different state of oxidation in which they may have taken up the carbon.

Explanation of the Figures, Plate XVI.

A, fig. 1, is the plan of the iron crucible:

A——is its section.

a, fig. 2, plan of the stopper.

a, —— section of the stopper.

B C, fig. 3, button of cast-steel seen in perspective. At *e* is represented the spot formed by the nitric acid on the part laid bare and polished.

VIII.

Notice of the Experiments made by Order of the Minister of the Interior of France, relative to the Fineness of Tin or Pewter.†

THE approaching renewal of the measures of pewter‡, which are used for wine, and several other liquors, demanded a knowledge, 1. of the degree of purity which the tin ought

* From very frequent trials of steel for working uses, I have been led to adopt the contrary rule, particularly with plate-steel. There is a kind of plate-steel sold in London, at seven-pence per pound, which remains white under the same diluted acid as turns cast-steel as black as ink. The accurate reporter has, no doubt, made his trials with different products from those in our market.—N.

† Communicated to the Philomathic Society, by Cit. Coquebert, and inserted in their Bulletin, No. 30. Fructidor, an. VII.

‡ *Etain*, which word is indiscriminately used by most writers for pewter, or alloyed tin, as well as that which is pure.

to have to secure the health of the consumers; and 2. of the simple and easy method of ascertaining, at all times, the fineness of tin, without injuring the vessels composed of that material.

The members of the bureau of weights and measures (Legendre of the National Institute, Gattey, and Ch. Coquebert) proposed, in consequence, a few months ago, to the minister of the interior, a plan of experiments to be made jointly with those of the Council of Mines (Gillet, Lefevre, and Lelievre) to obtain a solution of these two questions.

These united commissions invited the assistance of Citizens Fourcroy, Vauquelin, and Dillon, and after a great number of delicate experiments made with extreme care, the result of their united labours was a number of new and interesting facts, which served to fix the opinion of the supreme administration of the republic on those points which constituted the objects of enquiry.

It is more especially a point of justice to present a summary of the facts to the Philomathic Society, as seven of the commission are among its members.

Chemical experiments have proved, 1. that tin is more easily dissolved than lead, and before it, in point of time, by the action of wine, and of vinegar; and, 2. that lead is not perceptibly oxidized in these fluids, except at the line of contact of the air and the liquor; and consequently at an extremely small surface: 3. that the newest and most acid wine in the neighbourhood of Paris dissolved no more than an inappreciable quantity of lead, after having remained eight or ten days in vessels of pewter containing 18 per cent of that metal.

4. That the effect was nearly the same with vinegar, no perceptible precipitation having been obtained, when the vessels in which wine or vinegar were suffered to remain contained full 18 per cent of lead. In proportion as the vinegar saturates itself with tin, it lets fall a small quantity of tartarite of lead; but the quantity of this precipitate is extremely small, even when the operation is performed with vessels of a large diameter and extended surface.

5. When red wine remains in tin vessels, it loses its colour, which effect is owing to the settling of the colouring matter after its combination with the oxide of tin. This deposition does not appear to contain lead. The taste of bad wine made use of in this experiment was nevertheless improved; but there is reason to think that it was rather by the precipitation of the colour, and the saturation of part of the acid of the wine, than by the presence of lead.

From these several experiments, the commissioners have concluded that the alloy of lead with tin might be admitted in vessels intended to contain wine and vinegar, in the proportion of from 15 to 18 per cent, and that there is no reason to apprehend any injury to the health of those who use such vessels.

The second part of the enquiry was directed to ascertain a process for easily determining the fineness of tin. The hydrostatic balance has long appeared to afford a most certain method, though this instrument has not been employed for such a purpose in any country. Conjectural methods were substituted which depend on the aspect or flexibility of the metal,

or other circumstances equally uncertain. Chemical analysis is, no doubt, capable of accuracy; but the processes are long and minute; besides which, they require that a portion of the vessel itself should be taken off. The examination of specific gravity has none of these inconveniences; but to make it the foundation of a legal proof it is necessary to know by experiments in what manner tin and lead, united together in different proportions, are affected in this respect. It has been suspected that the alloys of these two different metals do not exactly possess the specific gravity which would result from taking the specific gravities of each separately. But is the specific gravity greater or less than the result of computation? Do the two metals penetrate each other, or do their parts on the contrary leave a greater vacancy than before their union? Experiment alone can shew this, as the opinion of learned men differ on the subject. Kaestner, Hausen, Hahn, and even Lavoisier in his report on the art of the pewterer by Salmon, were of opinion, that penetration takes place. The common specific gravity resulting from the mixture, according to the latter, greatly exceeds what would be deduced by calculation from the volumes of the masses. Kraft alone adopted the opinion of the dilatation, in the 15th vol. of the Memoirs of Petersburg; but he supported his reasoning on one solitary fact. Those who have elsewhere treated this subject, particularly in the Memoirs of the Academy of Stockholm, have confined themselves to compute without making experiments; and consequently it became necessary to refer the whole to observation.

The purest tin and lead which could be procured were taken and mixed in different proportions, with the greatest care to mix the alloy well together, and leave no cavity within. Three series of these pieces were made, which were subjected to the hydrostatic balance, and afforded the following results.

The alloys of tin and lead have really a less specific gravity than would be indicated by calculation. Consequently, the two metals are so far from penetrating each other, that they increase their volume when mixed together.

The following is the law of this augmentation as deduced from experiment.

When the alloy of lead was in the proportion of $\frac{9}{10}$, and, consequently, the tin was $\frac{1}{10}$, the volume of the mixture was augmented, or, in other terms, the specific gravity was diminished 26 thousandth parts.

8 parts of lead and 2 of tin, augmentation of volume	40 thousandths
7 ditto of lead and 3 of tin,	- - - 48
6 ditto of lead and 4 of tin,	- - - 47
5 ditto of lead and 5 of tin,	- - - 46
4 ditto of lead and 6 of tin,	- - - 45
3 ditto of lead and 7 of tin,	- - - 43
$2\frac{1}{2}$ ditto of lead and $7\frac{1}{2}$ of tin,	- - - 39
2 ditto of lead and 8 of tin,	- - - 33
$1\frac{1}{2}$ ditto of lead and $8\frac{1}{2}$ of tin,	- - - 30

1 ditto

1 ditto of lead and 9 of tin,	-	-	-	23
$\frac{1}{2}$ ditto of lead and $9\frac{1}{2}$ of tin,	-	-	-	14

The experiments were more multiplied for those mixtures in which the proportion of tin exceeds that of lead; and more especially when the tin contained from 5 per cent of lead to 25 or 30, because these cases offer themselves most commonly in actual business.

On these data a table is formed, by means of which it is very easy to determine, by weighing a vessel in air and in water, what proportion of lead the tin may contain.

This method undoubtedly cannot shew the other metals, such as copper, zinc, bismuth, and antimony, with which the tin may be alloyed; but every one knows that these metals are mixed with the pewter in the market in very small proportions; some because they alter the colour and quality; and others, because the price being at least equal to that of tin, does not render it the interest of the tradesman to add them*.

CH. COQUEBERT.

IX.

A Chemical Examination of the Bath Waters. By G. S. GIBBES, B.M. F.R.S.†

TWO ounces of the King's-bath water experienced no change of colour when a few drops of the spirituous tincture of galls were added to it: on the addition of one drop of a solution of sulphate of iron, it became of a purple colour.

The water which served for this experiment had been carefully preserved in a well-closed bottle.

The same quantity of this water warm and fresh from the spring experienced a considerable change of colour on the addition of the tincture of galls, evidently shewing the presence of iron.

Under precisely the same circumstances, the water of the two other springs which I examined, viz. the Hot and the Cross-bath waters, produced the same appearances.

* As it cannot be expected that this abridged account should contain the various reasonings and precautions of the gentlemen of the commission, I may, perhaps, have been anticipated on the subject of the following remarks. 1. The handle, and sometimes the foot, of pewter vessels are made hollow; which construction, if not provided against by the examiner, will defeat the purpose of this proof. 2. The artist may leave cavities for the fraudulent purpose of rendering his goods lighter, and consequently of a finer apparent title than it really possesses. 3. The density of cast-metal varies according to the heat of the metal, and the coldness of the mould. 4. Most articles of pewter are hammered, which will increase the density, though not so much as the lastmentioned cause. 5. The small additions which are made to improve the hardness and colour may also change the density. It is affirmed that arsenic, in particular, is very effectual in this respect.—N.

† Communicated by the author.

From the experiments which I have made, and which I have varied in every way I could devise, as will hereafter appear, it seems that the celebrated philosopher Mayow made his experiments on the waters after they had been suffered to cool. He says, "Quod ad vitriolum, denique spectat, balneum vulgo dictum balneum crucis, item alterum præservidum nominatum, vitriolum plane nullum continere videntur; etenim si gallæ contusæ aquis thermarum dictarum infusæ fuerint; aquæ istæ colorem purpureum aut nigrum nequaquam habituræ sunt; quod tamen omnino contingeret, si thermæ istæ vitriolo imbutæ essent. Quod ad balneum regis, (sic dictum) istoc vitrioli tantillo imprægnari videtur; quippe si gallæ contusæ, ejus aquæ injiciantur, eadem colore atro-purpureo leviter tingetur." I have found upon repeated trials, that the King's-bath water will assume a purple tinge on the addition of tincture of galls, when warm, and even when it has been recently cooled, but not the least colour after it has remained cold for any length of time. It appears from the experiments which I have made, that a small portion of iron is dissolved by the carbonic acid, and that this acid by evaporating leaves the iron unsusceptible of any change of colour by the addition of galls. Mayow's observations on this circumstance confirms my opinion, and are perfectly consistent with the facts I have observed. He says, "Annotandum est autem, quod minera quædam indolis metallicæ una cum Thermarum prædictarum Scaturiginibus e Terrâ prorumpat; quæ facile in vitriolum converti potest: etenim si fabulo (quod una cum aquis Thermarum e Terrâ erumpens, in fundo balneorum reperitur) liquor quivis acidus superfundatur, idem non sine effervescentiâ fati insigni a menstruo acido corrosum, ex parte aliquâ in vitriolum convertetur; haud secus ac limaturæ ferri a liquore acido corrosæ, contingit: quippe si fabulum istoc balneorum liquore acido impregnatum, infusioni gallarum injiciatur, liquor mox colorem atro-purpureum acquireret: cum tamen, si infusio Gallarum fabulo isti recens a balneis exempto, non vero a liquore acido jam corrosio, affundatur, ea nequaquam colorem purpureum obtinebit, indicio utique manifesto, fabulum balneorum metallicum, non nisi a menstruo acido corrosum, indolem vitriolicam induere.

Advertendum est autem, quod fabulum istud thermarum aliquandiu servatum, aerique expositum sponte suâ in vitriolum commigrabit: quippe si istius modi fabulum gallarum infusioni injiciatur, aqua mox colorem atro-purpureum habitura est. Quinimo si idem lingua imponatur, sapor vitriolicus fati manifeste se prodet.

Nimirum spiritus nitro-aereus cum minerâ metallicâ, sive marchasitâ salino-sulphuræâ (e quali vitriolum confici solet), fabulo dicto admistâ tractu temporis congregitur, et effervescit; eamque tandem modo alibi ostenso, in vitriolum convertit."

Here we cannot help admiring the extent of Mayow's mind, who, without the aid of modern science, could so accurately describe the causes which produce the vitriol he speaks of. Allowing his spiritus nitro-aereus to be oxygen, his theory is perfectly consistent with all the modern discoveries; the iron, by acquiring the spiritus nitro-aereus, or oxygen, becomes ochre or rust, and is then soluble in acids; or if it (as he observes) be united with sulphur, the sulphur acquires his spiritus nitro-aereus, is converted into vitriolic acid, which combining with the iron, forms vitriol of iron, and is then affected by an infusion of galls.

That

That all this is perfectly consistent with fact, I hope to be able to shew by experiments I shall hereafter relate.

To two ounces of the King's-bath water I added a few drops of a solution of prussiate of potash, and no change of colour took place, on the addition of one drop of a solution of sulphate of iron, an evident blue colour appeared. This experiment was made on the Cross and Hot-bath water with the same results.

The hepatic water, which is composed of 16 grains of sulphuret of lime, and 10 grains of the acidulous tartarite of potash, is an excellent test for discovering metals which are held in solution. Iron precipitated by it is redissolved by the sulphuric acid. With this test, I could not perceive the least quantity of iron in any of the three waters.

As iron may be held in a state of combination in which it may resist the action of the foregoing tests, I have used the following means for ascertaining its presence.

In Professor Bergmann's table of attractions, I find that the sulphuric acid has, after the acids of sugar and tartar, the strongest attraction for iron; I have, therefore, first added a very small quantity of the sulphuric acid, and after a due time for combination, a few drops of the hepatic test: still I could not perceive the least trace of iron. This experiment was repeated with other tests with the same results. Had any iron been held in solution by means of the sulphuric acid, the hepatic test would most probably have shewn it. The smallest quantity of that acid was added, that it might not superabound with carbonate of potash. I diminished even this quantity: but carbonate of potash will produce a brown precipitate from the test, if sulphuric acid has been present; but that brown precipitate will not be redissolved by the acid.

The Bath waters do not produce any change of colour in vegetable tests, consequently they do not contain any acid or alkali in an uncombined state. I added one drop of diluted sulphuric acid to two ounces of each of the Bath waters, which, with that small quantity of acid, changed the colour of turnsole. Had there been any disengaged alkali, or earth, it would have most probably destroyed the effects therein produced.

Diluted sulphuric acid poured into two ounces of the King's-bath water, produced a separation of minute air-bubbles, which I apprehend are carbonic acid, as lime-water added to another quantity of this water caused a precipitation. The same appearances were observed in the other waters, but their quantities vary considerably.

These experiments would lead us to conclude that these waters contain carbonic acid gas, either in a disengaged state, or united to some alkaline basis. From some of the foregoing experiments, however, it appears to be in a disengaged state, as the tests did not shew the presence of any alkali, and the carbonic gas was too small for them to detect it. We shall hereafter shew that very large quantities of elastic gases arise with the waters; their heat however prevents any large quantity from uniting with them. The carbonic acid has the greatest attraction for water, and therefore the other gases are not combined with it.

When to the waters of the three baths I poured lime-water, an evident precipitation took place; which precipitation I, of course, imagined was owing to the carbonic acid gas being
united

united to the pure lime. When I added a small quantity of diluted vitriolic acid, the precipitation was redissolved, and I observed a separation of some air-bubbles. Although the Bath waters contain carbonic acid gas, they yet attract more air in cooling. This I ascertained by exposing a quantity of the water to some air in a large close vessel, the water having a communicating tube on the outside. The water in the outside tube was at first on a level with the water within, but after a little time the water sunk in the outside tube; proving that the air in the receiver had been absorbed.

The carbonate of potash produced a very copious precipitate in each of the waters of the three baths.

A solution of pure potash produces only a slight precipitation in each. A solution of pure ammonia produces also in all the waters a weak precipitate. Pure ammonia does not separate calcareous earth from its solutions. Solution of carbonate of ammonia produces a much greater precipitate in the Hot-bath water than in the other two. In Professor Bergmann's table of affinities we find that lime has a much greater attraction for the sulphuric and marine acids than the volatile alkali. Upon a solution of muriate of lime in distilled water I poured a solution of pure ammonia, and no precipitation ensued; but on adding a solution of carbonate of ammonia in distilled water, a precipitation was plainly perceptible. This last experiment shews that a double decomposition took place, in which the carbonic acid left the ammonia to unite with the lime, and the muriatic acid left the lime to unite with the ammonia.

Pure ammonia, however, produces a precipitation in the Bath waters, which shews very clearly that other earths besides lime are contained in them. Now the marine and sulphuric acids have a less attraction for ammonia than they have for magnesia; it is therefore not magnesia which is separated by the pure ammonia. As clay has a much less affinity for the marine and sulphuric acids than ammonia, it appears that clay is separated in the foregoing experiments. I poured some pure ammonia on a solution of sulphate of alumine, or common alum, and a copious precipitation ensued. In a former experiment I mentioned that a solution of pure potash did not produce so large a precipitation as the solution of the carbonate of potash. The reason is obvious; for although the pure potash would separate the sulphuric and marine acids from lime, yet that lime would be for the most part redissolved by the water, and a very sparing precipitation would ensue. This I proved more clearly by adding to the mixture of pure potash and the Bath waters more distilled water, which lessened the quantity of precipitate, though I could not by those means make it entirely disappear. This shews that other earths besides lime are contained in these waters. The precipitates entirely disappear on the addition of diluted sulphuric acid.

The oxalic acid produces a copious precipitation when added to the King's-bath and Cross-bath waters. The Hot-bath waters produce a much greater quantity of precipitate with the tests for lime than the other two. We have now proved that these waters contain a large proportion of lime, and that this lime is combined with some acid.

Having separated by means of a filtre the oxalate of lime which was formed in the foregoing

going experiment, I added to the clear fluid which came through the filtre a solution of pure ammonia, which caused a precipitation. Now from Professor Bergmann's table, and from direct experiments before related, I learn that pure ammonia has for the sulphuric and marine acids a less attraction than magnesia; but pure ammonia, as before mentioned, has a greater attraction for those acids than clay; consequently it is evident that this precipitation is clay. There appeared this separation in the three waters of the baths, but there is a great difference in the quantity of precipitate. The Hot-bath waters contain more than the King's-bath, and the King's-bath more than the Cross-bath waters.

When the waters of the several baths are warm, and fresh from the springs, there is an evident discolouration on the addition of infusion of galls, plainly indicating the presence of iron.

Two ounces of the waters fresh from the bath were mixed with a solution of sugar of lead; each produced a considerable milkiness, and the colour remained perfectly white. On the addition to each of a quarter of a grain of sulphuret of potash, an evident discolouration took place, indicating the presence of sulphur. From this, as from many other experiments, I am convinced that there is no sulphurated hydrogenous gas produced by the Bath waters.

When silver-leaf was placed in a vessel containing these waters fresh from the pumps, it remained of the same brightness and white colour, but on the addition of sulphuret of potash it assumed an almost black colour.

(To be continued.)

X.

Construction of a Lamp for Burning Tallow. By MR. WILLIAM CLOSE.

To Mr. NICHOLSON.

SIR,

THE construction of a lamp for burning tallow some time ago engaged my attention, and my endeavours were attended with some degree of success. As the contrivance is very simple, and likely to save considerable expence in the article of illumination, I have sent you a description of it, which I hope may not be unacceptable to some of your readers.

It was the perusal of the observations formerly made by yourself upon the same subject* that first induced me to make the attempt.

I am, sir, your humble servant,

Dalton, Sep. 30, 1799.

WILLIAM CLOSE.

* Philosophical Journal, I. 67.

A, represents a cup made in the form of a cone: it contains the tallow, and is supported, with the point downwards, by the thumb-screw *i*, upon the piece of iron D, which is firmly fixed into the circular wood bottom E. The widest diameter of this cup is about two inches and a half, and the diameter of a small aperture in the point is rather less than one-eighth part of an inch. This cup must be made of iron, brass, or copper, and the joint on the side closed with hard-folder.

A circular plate of iron is made to fit the widest diameter, and firmly fixed therein by the sides of the cup being turned over it a little: near one side of this circular plate, a circular hole less than one inch in diameter is made, and into it is fixed the ring *d*, which forms the mouth of the cup, and may be closed with a cork, &c.

a b represents a piece of wire which passes through a hole made in the circular piece, and through the aperture in the point of the cup. This wire is rather more than one-eighth part of an inch in diameter; it converges near the point, and exactly closes the small aperture when thrust down: it serves to regulate the descent of tallow into the cup B, according to the quantity consumed by the flame; and therefore when it is required not quite to close the aperture, it is drawn up a little, and a small spring of brass, in the inside of the cup at C, presses against it, and holds it in the place. The spring passes through the circular plate, and is fixed on the outside by a small screw.

B represents a small cup, in which the tallow is burned: it is about one inch in diameter, and about half an inch in depth. Into the bottom of this cup is soldered the tube *f*, which is about two inches and a half long, and slides into the tube *g*, which is soldered into the bottom of the cup *c*.

e represents a piece of bended wire, which supports the wick of the lamp. The ends of this piece of wire are thrust into a piece of soft wood, fitted into the tube *f*. Another tube, represented by *h*, is soldered to one side of the cup B, above the brim: the use of this tube is to contain a quantity of clean wick, and to serve for a handle to lift the tube *f* out of the socket *g* when the lamp is to be lighted.

C represents a cup to receive any tallow that may chance to run over the sides of the cup B. It is rather more than one inch deep, and two inches in diameter.

Lastly, by the help of the thumb screws *i i*, the height of the cups B, C, and A, and the distance between B and A, may be regulated at pleasure.

A wick of cotton being put into the tube *h*, and brought through the ring *e*; a quantity of tallow put into the conical cup; and the small cup filled with melted tallow; the lamp may be lighted: if the point of the cup A be raised two or three inches above the brim of the cup B, and the air in the room at rest, the tallow in the cup A will be fused in a few minutes, and if the wire *a b* be properly adjusted, a constant supply of tallow will drop to the flame.

Small particles of dust and other impurities in the tallow sometimes impede the drops, but the light continues undiminished, until all the tallow in the little cup is consumed, and the

the cup is then easily filled again by holding up the wire, after which the drops must be regulated again. To lessen this inconvenience as much as possible, nothing but clean tallow, or hogs' lard, must be burned.

Tallow and hogs' lard will burn with a very clear bright flame, of the same intensity, for a long time.

The small cup, when detached from the rest of the apparatus, will supply the different uses of a candle: it may be carried about by the tube *b*, or, occasionally, placed with the tube *f* in a small wooden stand. The tallow is not very liable to be spilled.

Every time before the lamp is lighted, a new portion of wick must be drawn through the ring. This is not easily effected when the tallow is cold; therefore, to avoid this trouble, it is the best method always to draw up the wick immediately before the flame is extinguished; for which purpose, small forceps, made of one piece of bended iron, will be most convenient.

Lastly, it may be necessary to remark, that after the lamp is lighted, when the tallow in both cups is cold, the tallow in the small cup must be broken and stirred up, that a sufficient quantity may be fused immediately to supply the flame; and the sooner to fuse the tallow in the conical cup, the flame should be raised near to its point, and when there is a strong current of air in the room, in that situation it ought to remain*.

XI.

Report made to the Institute of Sciences and Arts (at Paris), on the 29 Prarial, in the Seventh Year (June 17, 1799), in the Name of the Class of Physical and Mathematical Sciences, on the Measure of the Meridian of France, and the Results which have been deduced to determine the new Metrical System.*

(Concluded from p. 324.)

MEASURES of surface are easily deduced from the determination of the length of the metre, which is the basis of the whole system. But this is not the case with measures of

* Count Rumford assured me some time ago, that tallow will burn very well in the common fountain lamp, provided the basin, or receptacle into which the tallow flows, is sufficiently capacious to hold enough of the material to support the combustion till the tallow in the reservoir is fixed. The principal inconvenience I found on trying this, was the interception of a large quantity of light by the basin itself, which, nevertheless, I think may be almost entirely succeeded, though perhaps not quite as perfectly as in a candle. Tallow sold retail at the price of candles costs ten pence a pound, which in the middling-six will burn above 40 hours. Such an illumination will therefore cost less than one farthing an hour. I do not know the price, consumption, and quality of oil, but estimate it at about half that of tallow. Some better information respecting both shall be given in our next number.—N.

† Abridged from the Journal de Physique. Fructidor, An 7.

VOL. III.—NOVEMBER 1799.

3 B

weight.

weight. The delicate and laborious task of determining the unity of weight was entrusted by the National Institute to Lefevre Gineau, together with Fabroni of Florence; and a special commission was afterwards employed in examining all the registers of observation and experiment, and verifying all the computations. The unity of weight must necessarily consist of some solid, the magnitude of which shall be determined from the lineal measures of the system, and the material of which shall be such as can at all times be procured of one and the same uniform density. The magnitude of the solid adopted for this purpose was the cube of the decimeter, and the substance itself distilled water. Hence the experiment was reduced to determining the relation between the new unity of weight and those of the ancient system, by an actual experiment with such a solid of water. Experimental philosophers are aware that there are two methods of doing this; that is to say, by actually weighing the water contained in a vessel of known dimensions, or otherwise by finding the weight of the quantity of water displaced by a solid, of which the dimensions must also be known. The latter method was preferred, and with justice, on the present occasion.

A hollow cylinder of brass, internally supported by edge-bar-work, was constructed by C. Fortin, together with a guage capable of ascertaining longitudinal measures, to the precision of the four thousandth part of a line of the ancient measure. On the bases of this cylinder were drawn twelve diameters, intersecting each other at equal angles, and each diameter lying in the same plane with a correspondent diameter upon the opposite base. Three concentric circles were drawn on each base, also corresponding by pairs with each other. The points of intersection, including the centre itself, were consequently 37; and by measuring all the several distances between each of these points, and its correspondent point on the other base, the true figure of these boundaries, and mean length of the cylinder, were then deducible. Eight circles were also described on the convex surface, and twelve right lines were drawn joining the extreme points of the diameter that had been drawn on the bases. These lines and circles afforded ninety-six intersections, to every pair of which the guage being duly applied, gave the measure of forty-eight diameters. From all these measures, suitably reduced, it was found that the volume of the cylinder at the temperature of 17,6 of the centigrade thermometer was 0,0112900054 of the cubic metre, or more than eleven times the magnitude of the intended unity.

The balances used for weighing were extremely accurate. One of these, charged with rather more than two pounds poids de marc, in each basin, shews the millionth part of the weight; that is to say, one fiftieth of a grain, and it turns with one tenth of a grain when each arm is loaded with 23 pounds.

The weights were arbitrary, though nearly the intended standard, it being of more consequence that they should be adjusted to the most precise equality, than that they should agree with any definite weight. The sub-divisions were in the decimal order.

The cylinder was made hollow, in order that it might load the balance as little as possible, under a given magnitude, but it was necessary that it should be heavy enough to sink in water.

There

There was a communication between the external air and the inside of the cylinder, by means of a tube, which served to suspend it; and the weights being of the same material, the result in air was the same as it would have been in vacuo.

In order to avoid all dependence upon the relative lengths of the arms of the beam, the weighing was always performed in one and the same scale, the thing itself being first counterpoised, and afterwards taken out, in order to admit the equivalent weight in the same scale. By 53 experiments, the weight of the cylinder proved to be at a mean 11,466055 of the arbitrary unities, and the extreme difference among these experiments did not amount to 45 millionth parts of the unity. The more difficult operation of weighing the cylinder in water was repeated thirty-six times, and the mean apparent residual weight proved 0,2094190 unities with no greater absolute difference between the extremes than before.

The reporter enumerates the various corrections to which this apparent weight must be subjected. In the first place he observes, that the air supports the counterpoise; and does not support the body plunged in the water. Secondly, the apparent weight expresses not only that of the cylinder, but likewise of the air contained in its cavity. Thirdly, regard must be had to the density of the water, as governed by its temperature. The experiments were made by surrounding the vessel that contained the water with pounded ice, which kept the temperature of the water itself at three tenths of a degree of the centigrade thermometer above the freezing point; but the results were reduced to the maximum of density of water, which by another course of experiments was found to be at the fourth centigrade degree, conformably to the experiments of Deluc. Lastly, it was requisite to allow for the expansion or contraction of the brass cylinder, by the difference of temperature at the time of admeasurement, and the subsequent experiment.

After all reductions, it was found that 11,2796203 cubic decimetres of water at its maximum of density weighed 11,27, and that one single cubic decimetre of water at its maximum of density weighs 0,9992072 of the unity, which is the true kilogramme of the new metrical system.

It remained then to determine the relation between the arbitrary unity made use of, and the ancient French weights. For this purpose the ancient pile weighing fifty marcs, called the pile of Charlemagne, was examined. The whole pile repeatedly weighed was found to be equal to 12,2279475: whence it follows, that each unity is equal to 18842,9088 grains poids de marc, and that the true kilogramme, or weight, of one cubic decimetre of distilled water, taken at its maximum of density and weight in vacuo, that is to say, the unity of weight, is 18827,15 grains. The reporter adds in a note, that according to these experiments, the foot cubic (French) of distilled water, taken at its maximum of density, is 70 pounds 223 grains; and at the temperature of three tenths of a degree, it weighs 70 pounds 141 grains; and at the temperature of melting ice it weighs 70 pounds 136 grains.

The pile of Charlemagne, though very accurately made for the workmanship of the fourteenth century, at which time it is pretended that it was made, or renewed, is not accurately the same in all its parts. The mark taken as the fiftieth part of the whole pile proved to

be 0,2445589 of the unity. The hollow mark was 0,2445127; and the solid mark, 0,2444675. Whence the differences are: between the mark deduced from the whole pile and a hollow mark 0,87 grains; between the same and the solid mark, 1,87 grains; and between the solid mark, and the hollow mark, 0,85 grains. The mark which the celebrated Tillet used in his experiments on the weights of different countries, made in 1767, and inserted in the Memoirs of the Paris Academy for that year *, was different from this pile, though the reporter does not say what was the difference.

The standards presented to the Institute by this commission, were the metre in platina, equal at the temperature of melting ice to 443,296 lines of the toise of Peru, this toise being supposed to have the temperature 16 and a quarter, as has been already observed.

Besides this extraordinary standard, other standards made of iron were also presented for use on common occasions. It is recommended that the operations of adjusting measures of different metals to these standards should be performed at or near the 15th degree of the centigrade thermometer, because the subsequent variations, either to freezing or to a considerable heat, would then produce a less difference between the different kinds of metal.

The standards of weight were a kilogramme of platina intended for the legislative body, and to be preserved with the most scrupulous attention for very important occasions, and several other kilogrammes of brass, made with the same exactness, and intended for civil use. These two kilogrammes of platina and of brass being truly adjusted, are not equal in air, but only in vacuo. The difference in air is, that the brass weight is about one grain and two thirds lighter, on account of buoyancy.

Such are the standards which have been produced with great labour, from a course of observations, experiments, and deductions, capable indeed of being repeated, though not likely to be again performed by a less power than that of a public government. What might be the limit of difference between these results and others which might be had by such a repetition, can therefore be only estimated from a sedulous examination of the particulars of this arduous enterprise. It will not, however, be necessary, for the preservation of the results of Cit. Mechain and Delambre, to recur to the actual standards which have been here presented; for, as the reporter points out, it will be sufficient to render the operation of renewal more easy if the length of the simple pendulum at a known place be expressed in parts of the metre. Cit. Borda, Mechain, and Cassini, have determined this for Paris at the national observatory, with an apparatus which will be described in a memoir of Borda hereafter to be printed. The length of the simple pendulum which beats seconds at Paris was found to be 0,2549919 of the module supposed to be at the temperature of melting ice: whence it is easy to conclude that this length is 0,993827 of the metre.

* See also the First Principles of Chemistry; and my Chemical Dictionary, art. Balance.

XII.

On the Web of the Garden-Spider. By a Correspondent.

TO MR. NICHOLSON.

SIR,

YOU have, probably, often remarked the peculiar construction of the web of what, I think, is called the garden-spider. This web is composed of threads radiating from a centre, and nearly equidistant; these are intersected by others, which bear some resemblance to concentric circles. I remarked, several years since, a very striking difference between the circular and radiating thread; if the former be touched with the finger, they will adhere to it so tenaciously, as sometimes to break upon withdrawing it, although their elasticity is such that they may be extended to more than double their usual length; which, however, they instantly resume, upon quitting the body to which they adhered. On the contrary, the radiating threads appear totally destitute of the adhesive property, and never indicated the least disposition to attach themselves to any substance I presented to them. Having remarked that the insect often doubles, and sometimes triples the radiating threads; that those which serve as boundaries to the web, to which the extremities of the former are fastened, are frequently composed of seven or eight single threads united by the spider, so as to form but one (which, as well as the former, is not possessed of the adhesive property); and that those of the circular kind are always single; I was at first induced to attribute the absence of the adhesive property in the two former to their exerting this property upon each other. To see how far this idea would agree with facts, I united several of the circular threads; but upon trial, no diminution of their tendency to adhesion was perceptible. In the almost innumerable experiments I have made since this phenomenon (if it can be termed so) was first observed, the results have been uniformly the same, not only when the finger has been employed, but with every substance with which the experiment has been tried.

It appears that this remarkable difference is not occasioned, nor in any perceptible degree influenced, by the state of the atmosphere, nor by the size, nor consequently by the age of the spiders*.

I am, Sir, with great respect,

Your humble servant,

J. L.

* This fact is somewhere mentioned, but I cannot recollect the place. It is affirmed that the spider walks only on the radial threads.

In answer to the question in the postscript, requesting me to indicate an elementary chemical work containing directions for conducting the various chemical processes, descriptions of apparatus, and a full explanation of the new chemical nomenclature, I must observe, that we have no modern book which describes the processes with the minuteness and fidelity of Boerhaave, or Shaw, in their lectures; but that the elements of Chaptal, and of Lavoisier, are sound discursive treatises, written according to the modern system. Tables of the chemical nomenclature have been published in English, by Dr. Pearson and others.—N.

Pla

XIII.

*Plan of Experiments which were made in the Garden of Plants upon Sheep, and other Domestic Animals. By DAUBEUTON *.*

IF all the experiments of which I present the plan were but a project, I should be cautious of speaking of it to the Institute, because a simple project would not be worthy of their attention : but it is now a considerable time since I have been employed in these experiments. I have already made a great number, many of which have afforded results sufficiently interesting to be published. I purpose to communicate these to the Institute; and have therefore thought it necessary to describe the methods I have followed, in order that they may be enabled to judge what degree of confidence they deserve.

In the meeting of the 24th Thermidor, in the year iii. of the French republic, the professors and administrators of the National Museum of natural history gave to my disposal, for the purpose of making experiments on sheep, a piece of ground and building, which were purchased of Cit. Leger, and forms a court-yard. I have altered and appropriated it to sheep, and other domestic animals, upon which I had before made some trials, which afforded experimental facts always to be depended on, and often useful to the advancement of our knowledge. My age and infirmities having prevented me, for many years, from going to Montbard, I had suspended this work ; but the facilities with which my brethren have procured me at the museum have engaged me to resume and continue it.

During the space of eight-and-twenty years, which I have employed in the improvement of sheep, I have always seen with regret that cultivators have never employed those remedies which have been suggested for the treatment of this animal during illness, because they have been thought too expensive. In fact, they cannot be expected to expend as much in curing a diseased sheep as it would be worth if in health : their treatment must, therefore, necessarily cost much less ; for it is not possible to cure every disorder. I have, however, discovered a method of curing the most common diseases of sheep, at a much less expence. I have been obliged to suspend these researches ; but the advantages which I have since found in the museum, has enabled me once more to resume this work, which is now nearly terminated.

It has been affirmed that the goat willingly cohabits with the ewe ; and the ram with the she-goat : but a she-goat has been in my sheep-fold, near Montbard, during a number of years, with a flock of rams, without ever having yet produced young. If the goat and the ram were of the same species—if they were to couple and prove fruitful, what varieties we should see in the productions of this species ! some would be found with horns resembling both to the goat and the ram, or they would be covered with goats'-hair and wool. But the distinctive characters of these two animals have never been considered as equivocal by any one ; on the contrary, so many persons have affirmed that they breed together, and this opinion is now become so general, that it is of some interest to natural his-

* Memoirs of the National Institute of France, I. 377.

tory to ascertain the facts of this supposed cohabitation, and to learn, in case it takes place, what is the produce. For this purpose, I put a ram with a she-goat, and a ewe with a goat.

I gave an account to the Academy of Sciences, in 1779, of the experiments which I had already made, to ascertain what foods might be eaten by sheep without injury, and also that which would prove noxious. I am enabled at the Museum to extend these experiments to a great number of plants which I could not find in the district of the province of the Côte d'Or, where I have operated, during a long course of time, on the improvements of sheep.

There are convincing proofs, that the time the sheep remain folded in corn-fields, and in the meadows, greatly augments their produce; yet the practice of folding sheep on such grounds is not practised in the greater part of the provinces of the French republic. If this were done on all the fields, we should probably be able to raise a sufficient crop for the consumption of the republic; or, at least, we should not be obliged to import so great a quantity from foreign countries. I have entertained the opinion, that we might extend the custom of folding or penning sheep, if a small flock were to be so managed in the Garden of Plants; people from all the provinces, who visit that place, would then see the manner in which a park or fold is constituted, how they change the place, the cottage where the shepherd sleeps who guards it, the dog-house, &c.

They would also observe the difference between the produce of the fields, which had been thus treated, and those which had not; for to extend the use of a practice so useful, so important, and so necessary as the parking of sheep in corn-fields, and in meadows, it is necessary to employ every means which can contribute to it. I have caused very circumstantial experiments to be made in my sheep-farm, near Montbard, in the province of the Côte d'Or, on the produce of lands cultivated in this manner: I have compared the crops of fields and meadows which had been parked, with the crops of neighbouring lands on which sheep have not been fed, and with those of other lands which had neither been parked or manured, and also with lands which had only been manured without folding the sheep upon them. Incontestable proofs must necessarily be given of the advantages of parking, to induce those people to employ it who may not be convinced of the great benefits they would reap from it. I cannot too often observe, that if all the sheep which are in the republican territory were to be thus treated, it would greatly augment the quantity of our first and greatest subsistence. This consideration surely merits the particular attention of government.

It has been affirmed, that the flesh of the hogs of Siam* is more delicate and white than that of the common hogs. This animal has been variously spoken of. In some of the republican provinces where there were formerly some, there are now none; and though they are reared in other provinces, yet these animals form no part of the commerce of the dealers in wine of Paris, who can give no reason why they are not. I shall make trial of a hog of Siam, to ascertain whether they are worth being reared for common consumption.

* I suppose this to be known in England by the name of the Chinese breed.—N.

The greater part of the wild rabbits are destroyed, which is certainly productive of much benefit to agriculture; but it is difficult, at present, to find rabbits whose flesh has so good a taste, or so high a flavour. The flesh of the rabbit is nevertheless wholesome; it agrees with all constitutions; it is easy of digestion, and, consequently, good for convalescents, whose appetite it revives when the flavour is good. I shall make researches, for the sake of convalescents, and of those whose stomachs are weak, into the means of rendering the flesh of domestic rabbits as agreeable, if possible, as that of the wild rabbits; or, at least, to take away much of the expence and time required before the domestic rabbits are fit for the table, and the bad qualities which arise from the kind of food, which is more effectual to promote their growth than to give their flesh an agreeable taste. It is well known that sheep fed with lucern have their fat of a yellowish colour, and a disagreeable taste; whilst those which are fed with trefoil, have their fat also of a yellowish colour, but of a good taste. It is necessary to try other plants to know their effects with regard to the qualities of the flesh and fat of animals. I shall make a great number of these trials upon rabbits; as they will be more easy and speedy than that of sheep or kine. They will at least indicate those foods which will communicate the most desirable qualities to the flesh and fat of cattle. Rabbits increase and grow in less time; they are fed with less expence; from all which qualities they are preferable to other animals for trials of this nature. Rabbits are exposed without regret to the most dangerous experiments, because they cost so little. We do not fear to make trials with the most noxious plants on these animals, and even to repeat and modify them, in order that we may not be obliged to make more than one experiment on animals of greater value, such as the sheep, the hog, &c.

Columella has made mention of hens with five toes. This race of the species of the cock and hen exists at present. It appears that the fifth toe is a monstrosity; but there is reason rather to be surprized that this monstrosity should have been perpetuated from generation to generation, during so great a number of years, and that they should have been a constant race, well known to be of value before the time of Columella; if they have not degenerated, they surely deserve to be multiplied. I propose putting them to various trials. We know not whether the race of fowls without rumps, and consequently without tails, are not as ancient as those with five toes; but they really exist: and this also is a monstrosity. There is a race of fowls, which they call negroes, because they have the comb, the wattles, the skin, and the periosteum, of a black colour. Are there not means of making this colour disappear, or at least of weakening it? Experiments made on these fowls would be equally interesting with those with five toes, to natural history, anatomy, and physiology; particularly by the mixture of these three races with each other, and with the common race of fowls. Would this mixture vary the number of toes? Would the rump and the tail be completely, or only partly, formed? I have been informed that there have been fowls with only half a rump and tail.

There are to be seen in the Garden of Plants a small number of sheep of the Spanish race, of a superior fleece, who have been from the moment of their birth continually exposed,

posed, without shelter, to the open air, and which were produced by a ram and the ewe, who have also been from generation to generation, during a space of twenty years, exposed to the air. There are also to be seen lambs produced in the open air, who prosper better than when housed, whatever may be the rigour of the season. This example may, perhaps, induce the proprietors of sheep to suppress the use of stables. The expence of maintaining these buildings, instead of being useful to the sheep, is in a great degree hurtful to them. When there happens to be no stables in a farm, and the proprietors are not disposed to construct one, they avoid (in France) putting any sheep on that farm, not being aware that they are better in the open air than in stables, which the small flock in the Garden of Plants evidently proves. This would be of benefit, not only to individuals, but to the republic in general, since sheep form one of the principal sources of wealth.

In the experiments which I have made in my sheep-farm, near Montbard, on sheep, to ascertain what herbs they might eat without injury, and those which would prove noxious, I put two sheep into a small fold. These animals are so accustomed to be together, that a sheep who finds himself alone, becomes restless, and employs himself in searching for others, rather than in eating. I gave to these two sheep enclosed in the fold only one species of plants in the rack for their subsistence for eight days. In the Garden of Plants, I am able to try a considerably greater number of plants than in the province of the Côte d'Or, where my sheep-farm is situated; but the greater part of these plants are not sufficiently abundant to try them on two sheep for many days. I have, therefore, been obliged to divide the little folds into two, and to put only one. The sheep in each being separated only by a hurdle, they imagine themselves together, and they eat without any uneasiness with regard to company.

It appears that Linnæus was the first who thought of making trials to ascertain what plants animals eat, and those which they abstain from. He has made these experiments himself, and recommended them to his disciples. They have been published in the work entitled *Pan suecicus*; where is also to be found the method which he has followed in making these trials. I have thought it my duty to avoid his plan, as I found it to be attended with many inconveniences.

Herbs were placed before these animals, and it was too quickly concluded that they will, for the future, be agreeable to them, even though they had been compelled, after a long abstinence, to eat them. I think that these trials ought to be extended to a great number of days, provided there be a sufficient quantity of herbs of the same species to supply them with. For I have seen sheep obstinately refuse oats the first time they were presented to them, and eat them the second, with the utmost avidity, when they had an appetite.

Linnæus does not prescribe that trials should be made with herbs for feeding sheep when they are going out fasting; but when they return from pasturage, and are nearly satisfied with food. I believe this proceeding would answer, if we knew what were the herbs which sheep love best; but that is not the object of my researches: I wish to know with what

plants sheep may be fed and supported, though they eat those plants only when they are in want of such as they are accustomed to.

The manner in which I made the trials of plants upon sheep, were distinguished as follows:

1. Those plants which they eat with a good appetite.
2. Those which they eat against their inclination to appease their hunger.
3. Those which they absolutely refused to eat.
4. Those which caused them to drink more than ordinary, which is a very bad symptom in sheep.
5. Those which produced much urine.
6. Those which occasioned the colic in the stomach.
7. Those which purged.
8. Those which caused an evacuation of blood by the urinary passage.
9. Those which proved mortal.

XIV.

On the Native Iron of Peru, and on Mercury contained in Sea-Salt.

*By Professor PROUST, of Madrid.**

THE history of the discovery of native iron, by Don Rubin de Celis, is consigned in the *Annales de Chimie*. Accident procured me some small pieces, amounting to about half an ounce in all, which I was curious in employing to resolve the problem of native iron.

Before I exposed it to the agency of solvents, I examined its external characters. The most remarkable was, that it does not rust as easily as forged iron. My pieces, according to every appearance, were taken from the external surface of the great mass; but at all the places where the chissel has passed to cut them off, they preserve a whiteness and clearness which is astonishing, particularly when it is considered that they have crossed the sea, and have been kept several years in paper, which is the worst covering we know of for rusting iron.

These pieces are very ductile, forge wonderfully well, are very soft under the file, and cannot be hardened by ignition and cold water. When placed alongside of a piece of iron filed and softened to the same degree, they are whiter, and resemble steel annealed and filed clean.

I have dissolved one hundred grains in the aqueous sulphuric acid; and they produced merely 170 inches of hydrogen; whereas, at the same temperature and pressure of the atmosphere, soft iron constantly afforded 200 inches.

The solution was made with facility. Black particles are separated, but disappear towards the end of the solution; so that I could not decide that they were plumbago.

* *Journal de Physique*, VI.

In order to examine this solution, I began by hepatic water, with the intention of seeing whether any one of the metals would be separated which yield oxygen to sulphurated hydrogen, but no turbidness was produced.

It remained, therefore, to be proved, whether it contained any one of the four metals which are not precipitated by hepatic water; and beginning with manganese, I prepared my solution for the purpose of ascertaining its presence.

I first heated it with a little nitric acid, in order to carry the oxide of iron at the maximum of its oxidation. I then precipitated it gradually by the carbonate of potash, till the liquor had lost its yellow colour. It was then filtered, and the precipitation completed. A deposition of a light green colour was then made, which I soon perceived to be the carbonate of nickel. It was perfectly pure, and tinged borax of an hyacinth colour. This precipitate, redissolved in sulphuric acid, easily gave rhomboidal cubic crystals of considerable size. In this manner I obtained about 50 grains, which indicates that this native Peruvian iron contains a notable proportion of nickel.

If the carbonate of nickel of this alloy, or of any other ore whatever, be perfectly pure, as I have succeeded in obtaining it, loses the carbonic acid, becomes black, then parts with its oxygen, and at length remains on the coal well reduced to the metallic state. It is unfusible, at least with respect to my means, and very much resembles, in its colour and spongy appearance, platina obtained by the blow-pipe from the ammoniacal muriate of platina.

Nickel is perfectly attracted by the magnet, and that quite independent of iron, as I have proved by experiments on the nickel of Arragon, which I shall hereafter publish.

We may, I think, conclude, that iron alloyed with nickel in a certain proportion communicates a degree of whiteness, diminishes its disposition to rust, does not in any respect injure its ductility, if even it do not add to it; which deserves to be proved by direct experiments; and lastly, that it would be premature to determine whether this precious compound be the work of art or of nature.

On Mercury contained in Sea-Salt.

The strongest marine acid which is commonly found in the markets of France and Spain is usually made with common salt and oil of vitriol. This acid contains mercury in the state of corrosive sublimate: it arises from the mercury which is naturally mixed in sea-salt. This fact, which, at first, very much astonished the world, when Hilaire Rouelle announced it in the *Journal de Medicine*, is not mentioned in the Elements of Modern Chemistry, because no general inference could be drawn from a solitary fact. Yet mercury is also found in the salt which is consumed in Spain. I perceived it the first time by the spots of amalgam, which was left in certain silver basons in which I had purified a considerable quantity; and in this manner also it was that Rouelle was led to observe it.

The marine acid which is made by Charlar at Paris contains mercury. To this last substance

substance it is that I attribute the mercury I found in all the tin in the market, when I operated at that city. Being provided with marine acid from the same works, at the first erection of the laboratory of Segovia, I again found this amalgam at the end of the solutions of the tin of England, Mexico, and Monterey in Spain.

I remained a long time perplexed with the explanation of these facts, till chance, a short time ago, discovered to me the source of this astonishing mercury.

There is in the history of copper a peculiar kind of oxidation, which appears to me to have been hitherto unobserved. It takes place when plates of copper are kept in a bottle of marine acid full and closed. The copper, after having taken up about 17 per cent of oxygen, whereas it can receive 25 in other acids, this copper is transformed into a white muriate crystallized in tetrahedrons; which becomes violet-coloured by light, is insoluble in water, soluble in ammoniac without colouring it, and has other curious properties, of which I shall speak more fully hereafter. Being desirous of repeating this experiment with the marine acid of Paris, I found, two days afterwards, that my plates of copper were rendered white. I took them out, and examined them, and discovered without difficulty that they were changed by mercury.

To detect the existence of mercury in marine acid in a more direct way, I mixed it with hepatic water. It became immediately turbid, and afterwards deposited a black ethiops. This also is the habitude of the solutions of sublimate mercury with hepatic water.

If marine acid of the fabric of Cadahasso in La Mancha be mixed with the muriatic solution of tin which has been kept upon tin, the mixture soon becomes turbid, of a light grey colour, and will deposit mercury on a piece of gold placed at the bottom of the vessel. I have found as much as two grains in the pound of this acid.

However little may be the utility of quoting the ancient chemists, at this day, I shall nevertheless offer some passages which appear to shew that it was known that mercury is contained in marine salt.

Boyle found a small quantity of mercury contained in a mixture of lead and marine acid, left for a time in his laboratory. "This was so much the less surprising to me," says he, "as the principal ingredient was marine acid." On the Production of Principles. *De Product. Princip.* p. 55.

Does not this passage prove that the existence of mercury in common salt was not a new fact to Boyle?

Athanasius Kircher affirms, p. 316 of one of his treatises, of which I forgot the title when I wrote this note, that mercury is obtained from sea-salt. In his time, the system of mercurification, and the mercuries of metals, were much in vogue.

Beccher, *Phys. sub.* p. 205, obtained mercury from a mixture of sea-salt and clay. He affirms, page 456, that he was assured that mercury might be augmented by means of sea-salt, and asks, on this occasion, how much sea-salt may be contained in a pound of mercury? The reverse of the question would have been better founded.

Senac, in his Chemistry, speaks also of mercury found in sea-salt. And, lastly, we find other traces

traces of mercury in sea-salt, in the writings of Tachenius, Clavens, Beguin, in the treatise de Tribus Principiis of Glauber, &c.

If any traveller after reading this should take the trouble to observe whether the sheeting of a vessel newly sailing out of port should become silvered on certain places, particularly when the sheeting is new; or if he should take the trouble to suspend a plate of gold in the water, and observe its changes; he might perhaps flatter himself with affording at his return a new article to natural history, respecting the marine acid. Who can affirm whether the destruction of (copper) sheetings, which is sometimes so rapid, and still unknown as to its cause, may not depend upon mercury in greater abundance in some seas than in others?

XV.

*Note concerning the Earthquake in Peru, 1797. By M. CAVANILLES.**

THERE are more than sixteen volcanos, of which the internal parts are in a state of incessant agitation, and from which dense vapours, and often flames, are emitted, either by the crater or by lateral openings. In the most profound calms, noises and dreadful roarings are often heard, announcing the approach of earthquakes, to which this country is particularly subject. Since the year 1791, this noise was frequently heard in the neighbourhood of the mountain, called Tunguraqua. Antonia Pineda, and Nee, naturalists in the expedition round the world, examined the sides of this volcano, the lava of which was hardened by the internal heat of the mountain, as well as by the rays of the sun, and were much alarmed at the horrid noises and extreme heat of the place. Pineda, that excellent philosopher, whose premature death is still a subject of bitter regret to the scientific world, predicted that a terrible eruption was preparing in the mountain of Tunguraqua, and the event confirmed his suspicions. On the fourth of February, 1797, at three quarters after seven in the morning, the summit of the mountain being uncommonly free from vapours, the bowels of the volcano were agitated by frequent shocks, and the adjacent chains, or ridges, were dilacerated or burst in such a manner, that in the space of four minutes an immense country was overthrown by an undulatory motion. History has never related the effects of an earthquake so extraordinary; nor did ever a phenomenon of this kind produce greater mischiefs, or destroy a greater number of the human race! A number of towns and villages were destroyed in an instant. Some, among which were Riobamba, Quero, Pelileo, Patate, and Pillaro, were buried under the ruins of the neighbouring mountains: others, in the jurisdiction of Harnbata, Latacunga, Guaranda, Riobamba, and Alausi, were totally destroyed. Others suffered prodigiously by the gulphs which were formed, and the reflux of rivers intercepted in

* J. de Phys. VI. 231.

their course by dams of earth. Others again, after the agitation of frequent shocks, remained in a state so ruinous as to threaten their immediate destruction. Sixteen thousand men are reckoned to have perished in the first and subsequent shocks. At ten in the morning, and at four in the evening of the same day (Feb. 4), after a dreadful noise the earth shook with additional force, and continued to tremble, though with less effect, during the whole of the months of February and March. On the fifth of April, at three quarters past two in the morning, the villages already in part destroyed were again so strongly agitated as to have completed this overthrow. These shocks were felt through country extending 140 leagues from east to west; that is to say, from the sea coast to the River of Napo, and doubtless still farther; for we are little acquainted with those parts which are inhabited by the natives, and from N.E. to S.W. from Popajan to Piura, which is reckoned 170 leagues. (The volcano of Tunguragua had before occasioned an earthquake in 1557.) But in the centre of this district lies the part which is totally destroyed, to the extent of 40 leagues north and south from Guarandam to Machache, and 20 leagues from east to west.

But as if this earthquake had not been sufficient to ruin a country of such fertility, riches, and population, there was another misfortune prepared for the sufferers which had never before been known. The earth opened and formed immense gulphs; the summits of the mountains rolled down into the vallies, and from their bursten flanks there issued a quantity of fetid water, so immense that in a short time it filled vallies one thousand feet wide, and six hundred feet deep. It covered the villages, the edifices, and the inhabitants; closing the mouths of the springs of fine waters, and condensing by drying in a few days into a very hard earthy paste, which intercepted the course of rivers, causing them to return or deviate for 87 days, and converted what was formerly dry land into lakes.

During these earthquakes many of the most extraordinary events happened, which will no doubt be recorded in the page of history. I shall here relate only two. At the very instant of the earthquake, the lake Quirrotoa, near the village Infiloc, in the jurisdiction of Lacatunga, took fire, and its vapours suffocated the herds which fed in the neighbouring lands. Near the town of Pallileo stood a large mountain, named Moya, which being overthrown in an instant, vomited a river of the thick and fetid matter, which covered and completed the destruction of that wretched town. The naturalist will hereafter find in these desolated tracts objects worthy of his researches. Specimens of the minerals and earths of Tunguragua have been sent to Spain; but these will not suffice to indicate the causes of these astonishing phenomena. For this purpose it will be necessary to visit the ground itself, where this conflict of the elements took place, and has left the ruinous marks of its energy.

ACCOUNTS OF BOOKS.

ELEMENS d'une Typographie, &c. Elements of a Typography, which reduces that at present in use to one third; and a writing which gains near three fourths upon the common (French) writing: both applicable to all languages, preserving all the grammatical principles, and the riches of those which are printed in the Roman character, and written by the letters used in France; established on simple principles easy to be comprehended; demonstrated by clear and precise rules, of which the perfect theory may be learned in less than a day, and of which the exercise will enable a skilful hand to follow the speech of an orator. Paris, printed for the Author, A. Pront, No. 249, Rue de la Harpe; and sold by F s and Defenne. One volume octavo, about 200 pages, with 47 plates of examples, price 18 franks. Copies on vellum paper, 36 franks: and there are a few copies in large vellum paper, quarto, price 73 franks, in which the examples, words, and detached signs, are made with the pen.

The learned A. M. Millin, author of the *Magasin Encyclopédique* *, gives a long account of this work, and speaks highly in its favour, but, as he justly remarks, a work of this kind may be read and studied, but cannot be abridged. This is so far the case in his account, as well as the extracts he gives from the report made to the National Institute of France, that I must confess that though my curiosity is excited by the importance of the subject, and the general character of this performance, yet I am far from being satisfied with the obscure notion, conveyed, of the means by which the desirable purposes mentioned in the title are said to have been accomplished. The Reporter to the Institute, whose name is not mentioned, informs us, that "the consonants and the vowels are so identified in the processes of this invention, that two, three, four, and even five characters form only one, without any confusion in their values. Whence it constantly happens that a single figure, which demands no more time for its description than a single letter of common writing, and occupies no more space, but frequently less, is employed to represent two characters, often three, and sometimes four and five:" to which Cit. Millin adds, that this condensation proceeds even as far as eight.

Citizen Pront makes use of 118 characters: 25 to represent the capitals of the alphabet; 25 for the smaller letters; 32 for the compound vowels, &c.*

The written characters do not essentially differ from those used for printing. The chief difference consists in the ligatures, or methods of joining in the former.

As the condensation is stated to be as three to one, the fatigue of the eye must be only one third part as much as with the ordinary characters.

But the most remarkable peculiarities of the new theory of this author is (says C.

* Vol. III. 200.

† This &c. is in C. Millin's account, otherwise I should have mentioned the disposal of the rest.

Millin),

Millin), that though this method is extremely concise, easy to be acquired, and rapid in the execution, he has succeeded in retaining every syllable, vowel, point, mark, and even accent.

The gain of two-thirds of the time employed in writing is certainly very extraordinary. Professional men in the law have more occasion to employ copyists than those of any other business. Their work is reckoned by the sheet consisting of seventy-two words. Common copyists can write eighteen or nineteen sheets in an hour, with the words at full length; and it is reckoned speedy writing to complete twenty-four in the same time. The most speedy writer I ever employed as an amanuensis could write thirty sheets from dictation in the hour: but I never employed him so regularly as to know whether he could continue for days at that rate. I myself write at the rate of twenty-seven, and I have had several assistants who have written at about the same speed. Out of three short-hand writers I have employed, the swiftest did not write more than four or five and forty sheets in the hour, and that not very correct; which may be reckoned barely twice as swift as the common writers. I suppose the professed short-hand writers in our courts do much more; but they undoubtedly have many single characters, and abridgments of whole sentences, and common forms which must add to their dispatch. Deliberate reading so as to pronounce every syllable with extreme precision, or clear oratorical matter, is measured by about 120 sheets in an hour:—distinct colloquial reading by about 160 sheets. From the performance of the short-hand writers, compared to the extent to which they abridge their words, I have no doubt but that we form our common letters in less time than they do their simple characters, and that this difference arises from our greater practice. Hence it should seem as if, for common purposes at least, it would be better to abridge in the usual characters than in those of short-hand.

I have sent for this work, which may probably deserve further notice when I receive it.

Histoire des Mathematiques, &c. History of the Mathematics, &c. a new Edition, considerably augmented, and continued to the present Time. By J. F. Montucla, of the National Institute of France, 2 vols. 4to. 1465 pages, type Cicero.

Cit. Lalande has given a short account of this well-known and esteemed work in the *Mag. Encyclop.* III. 257. The first edition appeared in 1758, and the author has employed himself, during the long series of years elapsed since that time, in increasing and perfecting it: but his employment in the public buildings did not leave him time enough to attend to the second edition. In 1792, the author being disgusted with the circumstances of the times, was tempted to renounce it: but Lalande, and the bookseller Panckoucke, urged him to proceed; and in 1794, the work went to press. These two first volumes bring the subject, as in the first edition, to the end of the last century; but the two others will carry it to the end of the present. Half the third volume is already printed, and there is reason to hope that both will appear, notwithstanding the great age of the author, who was born on the 5th September, 1725. This edition is considerably augmented; the number of letters in the first edition, being to prove in the present as 232 to 336.

M. Simon Goodrich's Application of a Crank to answer the purposes of an Escapement in Clocks.

Philos. Journal Vol. III. Pl. XI. engraving p. 96c.

Fig. 1.

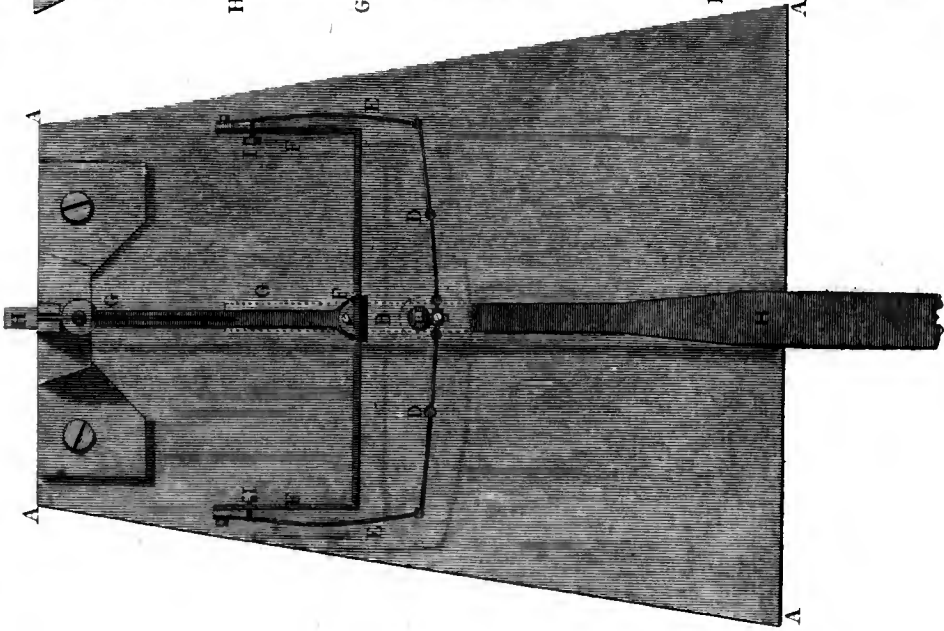


Fig. 2.

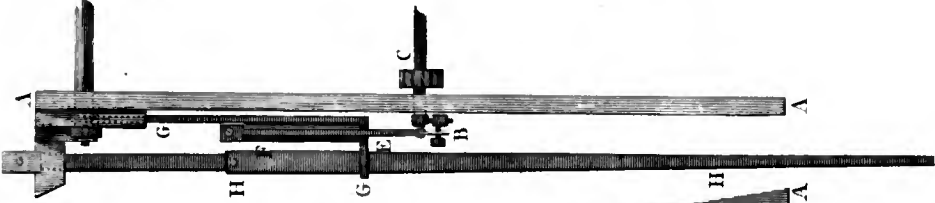
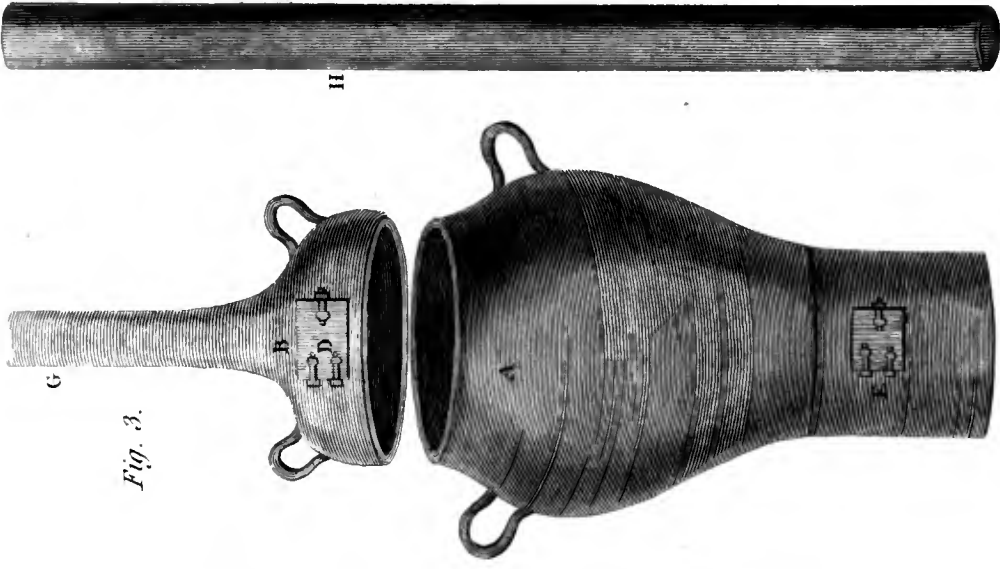


Fig. 3.



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Steel made with the Diamond.

Fig. 1.

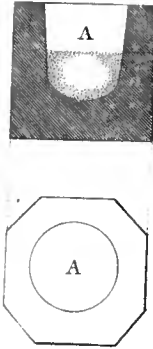


Fig. 2.

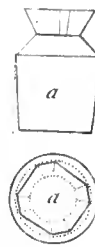


Fig. 3.

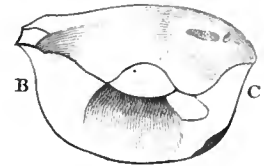
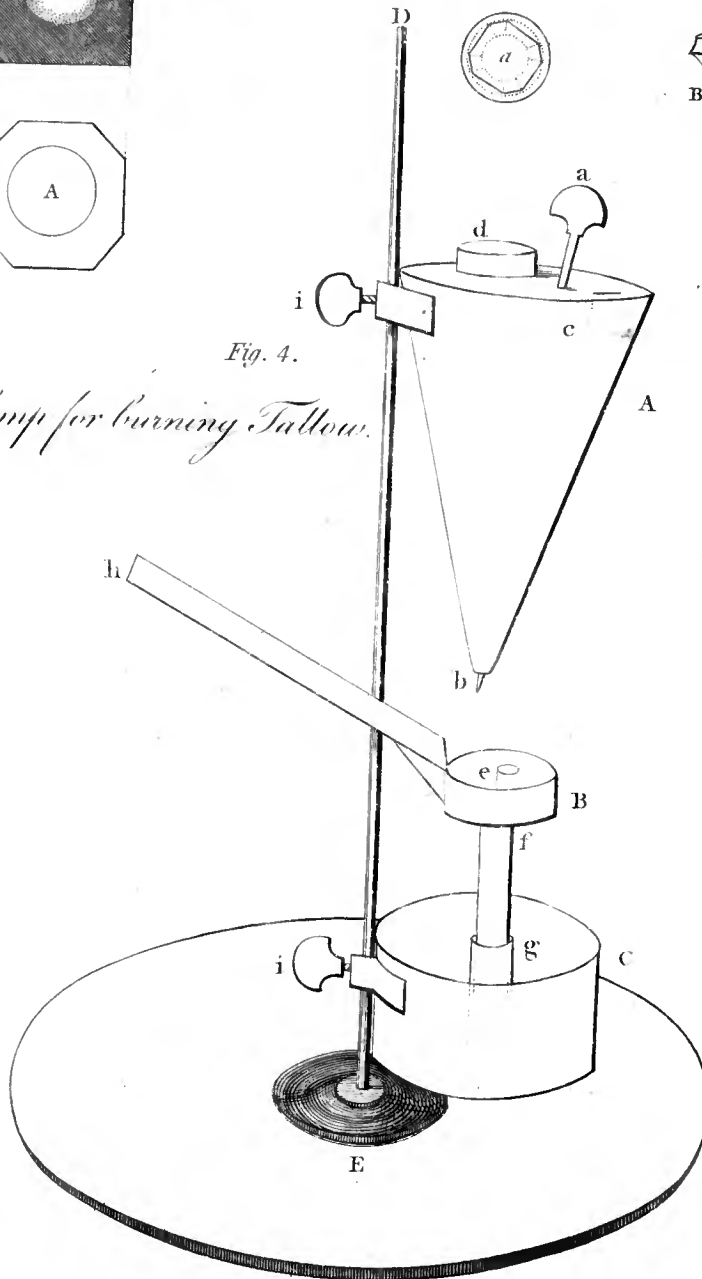


Fig. 4.

Lamp for burning Tallow.





A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

DECEMBER 1799.

ARTICLE I.

An Inquiry concerning the Weight ascribed to Heat. By BENJAMIN COUNT OF RUMFORD, F. R. S. M. R. I. A. &c.*

THE various experiments which have hitherto been made with a view to determine the question so long agitated, relative to the weight which has been supposed to be gained, or to be lost, by bodies upon their being heated, are of a nature so very delicate, and are liable to so many errors, not only on account of the imperfections of the instruments made use of, but also, of those, much more difficult to appreciate, arising from the vertical currents in the atmosphere, caused by the hot or the cold body which is placed in the balance, that it is not at all surprising that opinions have been so much divided, relative to a fact so very difficult to ascertain.

It is a considerable time since I first began to meditate on this subject, and I have made many experiments with a view to its investigation; and in these experiments, I have taken all those precautions to avoid errors, which a knowledge of the various sources of them, and an earnest desire to determine a fact which I conceived to be of importance to be known, could inspire; but, though all my researches tended to con-

* Philof. Transf. 1799. page 179.

vince me more and more, that *a body acquires no additional weight upon being heated*, or rather, that heat has no effect whatever upon the weights of bodies, I have been so sensible of the delicacy of the inquiry, that I was for a long time afraid to form a decided opinion upon the subject.

Being much struck with the experiments recorded in the *Transactions* of the Royal Society, Vol. LXXV. made by Dr. *Fordyce*, upon the weight said to be acquired by water upon being frozen; and being possessed of an excellent balance, belonging to *His most Serene Highness the Elector Palatine Duke of Bavaria*; early in the beginning of the winter of the year 1787,—as soon as the cold was sufficiently intense for my purpose,—I set about to repeat those experiments, in order to convince myself whether the very extraordinary fact related, might be depended on; and, with a view to removing, as far as was in my power, every source of error and deception, I proceeded in the following manner.

Having provided a number of glass bottles, of the form and size of what in England is called a Florence flask,—blown as thin as possible,—and of the same shape and dimensions, I chose out from amongst them two, which, after using every method I could imagine of comparing them together, appeared to be so much alike as hardly to be distinguished.

Into one of these bottles, which I shall call A, I put 4107,86 grains Troy of pure distilled water, which filled it about half full; and into the other, B, I put an equal weight of weak spirit of wine; and, sealing both the bottles hermetically, and washing them, and wiping them perfectly clean and dry on the outside, I suspended them to the arms of the balance, and placed the balance in a large room, which for some weeks had been regularly heated every day by a German stove, and in which the air was kept up to the temperature of 61° of *Fahrenheit's* thermometer, with very little variation. Having suffered the bottles, with their contents, to remain in this situation till I conceived they must have acquired the temperature of the circumambient air, I wiped them afresh, with a very clean dry cambric handkerchief, and brought them into the most exact equilibrium possible, by attaching a small piece of very fine silver wire to the arm of the balance to which the bottle which was the lightest was suspended.

Having suffered the apparatus to remain in this situation about twelve hours longer, and finding no alteration in the relative weights of the bottles,—they continuing all this time to be in the most perfect equilibrium,—I now removed them into a large uninhabited room, fronting the north, in which the air, which was very quiet, was at the temperature of 29°, F; the air without doors being at the same time at 27°; and, going out of the room, and locking the door after me, I suffered the bottles to remain forty-eight hours, undisturbed, in this cold situation, attached to the arms of the balance as before.

At the expiration of that time, I entered the room,—using the utmost caution not to disturb the balance,—when, to my great surprize, I found that the bottle A very sensibly preponderated.

The water which this bottle contained was completely frozen into one solid body of ice; but the spirit of wine, in the bottle B, showed no signs of freezing.

I now very cautiously restored the equilibrium, by adding small pieces of the very fine wire of which gold lace is made, to the arm of the balance to which the bottle B was suspended, when I found that the bottle A had augmented its weight by $\frac{35}{10000}$ part of its whole weight at the beginning of the experiment; the weight of the bottle with its contents having been 4811.23 grains Troy, (the bottle weighing 703.37 grains, and the water 4107.86 grains,) and it requiring now $\frac{134}{10000}$ parts of a grain, added to the opposite arms of the balance, to counterbalance it.

Having had occasion just at this time to write to my friend, Sir Charles Blagden, upon another subject, I added a postscript to my letter, giving him a short account of this experiment, and telling him how “*very contrary to my expectation*” the result of it had turned out; but I soon after found that I had been too hasty in my communication. Sir Charles, in his answer to my letter, expressed doubts respecting the fact; but, before his letter had reached me, I had learned from my own experience, how very dangerous it is, in philosophical investigations, to draw conclusions from single experiments.

Having removed the balance, with the two bottles attached to it, from the cold into the warm room, (which still remained at the temperature of 61°), the ice in the bottle A gradually thawed; and, being at length totally reduced to water, and this water having acquired the temperature of the surrounding air, the two bottles, after being wiped perfectly clean and dry, were found to weigh as at the beginning of the experiment, before the water was frozen.

This experiment being repeated, gave nearly the same result, the water appearing, when frozen, to be heavier than in its fluid state; but, some irregularity in the manner in which the water lost the additional weight which it had appeared to acquire upon being frozen, when it was afterwards thawed, as also a sensible difference in the quantities of weight apparently acquired in the different experiments, led me to suspect, that the experiment could not be depended on for deciding the fact in question; I therefore set about to repeat it, with some variations and improvements; but, before I give an account of my further investigations relative to this subject, it may not be amiss to mention the method I pursued for discovering whether the appearances mentioned in the foregoing experiments might not arise from the imperfections of my balance; and it may likewise be proper to give an account, in this place, of an intermediate experiment

which I made, with a view to discover, by a shorter route, and in a manner less exceptionable than that above mentioned, whether bodies actually lose, or acquire, any weight, upon acquiring an additional quantity of latent heat.

My suspicions respecting the accuracy of the balance arose from a knowledge,—which I acquired from the maker of it,—of the manner in which it was constructed.

The three principal points of the balance having been determined, as nearly as possible, by measurement, the axes of motion were firmly fixed in their places, in a right line, and the beam being afterwards finished, and its two arms brought to be in equilibrio, the balance was proved by suspending weights, which before were known to be exactly equal, to the ends of its arms.

If with these weights the balance remained in equilibrio, it was considered as a proof that the beam was just; but, if one arm was found to preponderate, the other was gradually lengthened, by beating it upon an anvil, until the difference of the lengths of the arms was reduced to nothing, or until equal weights, suspended to the two arms, remained in equilibrio; care being taken, before each trial, to bring the two ends of the beam to be in equilibrio, by reducing, with the file, the arm which had been lengthened.

Though, in this method of constructing balances, the most perfect equality in the lengths of the arms may be obtained, and consequently the greatest possible accuracy, when used at a time when the temperature of the air is the same as when the balance was made, yet, as it may happen, that in order to bring the arms of the balance to be of the same length, one of them may be much more hammered than the other, I suspected it might be possible that the texture of the metal forming the two arms might be rendered so far different, by this operation, as to occasion a difference in their expansions with heat; and that this difference might occasion a sensible error in the balance, when, being charged with a great weight, it should be exposed to a considerable change of temperature.

To determine whether the apparent augmentation of weight, in the experiments above related, arose in any degree from this cause, I had only to repeat the experiment, causing the two bottles A and B to change places upon the arms of the balance; but, as I had already found a sensible difference in the results of different repetitions of the same experiment, made as nearly as possible under the same circumstances, and as it was above all things of importance to ascertain the accuracy of my balance, I preferred making a particular experiment for that purpose.

My first idea was, to suspend to the arms of the balance, by very fine wires, two equal globes of glass, filled with mercury, and, suffering them to remain in my room till they should have acquired the known temperature of the air in it, to have removed them

them afterward into the cold, and to have seen if they still remained in equilibrio, under such difference of temperature; but, considering the obstinacy with which moisture adheres to the surface of glass, and being afraid that, somehow or other, notwithstanding all my precautions, one of the globes might acquire or retain more of it than the other, and that by that means its apparent weight might be increased; and having found by a former experiment, of which I have already had the honour of communicating an account to the Royal Society, that the gilt surfaces of metals do not attract moisture; instead of the glass globes filled with mercury, I made use of two equal solid globes of brass, well gilt and burnished, which I suspended to the arms of the balance, by fine gold wires.

These globes, which weighed 4975 grains each, being wiped perfectly clean, and having acquired the temperature of (61°) of my room, in which they were exposed more than twenty-four hours, were brought into the most scrupulous equilibrium, and were then removed, attached to the arms of the balance, into a room in which the air was at the temperature of 26° , where they were left all night.

The result of this trial furnished the most satisfactory proof of the accuracy of the balance; for, upon entering the room, I found the equilibrium as perfect as at the beginning of the experiment.

Having thus removed my doubts respecting the accuracy of my balance, I now resumed my investigations relative to the augmentation of weight which fluids have been said to acquire upon being congealed.

In the experiments which I had made, I had, as I then imagined, guarded as much as possible against every source of error and deception. The bottles being of the same size, neither any occasional alteration in the pressure of the atmosphere during the experiment, nor the necessary and unavoidable difference in the densities of the air in the hot and in the cold rooms in which they were weighed, could affect their apparent weights; and their shapes and their quantities of surface being the same, and as they remained for such a considerable length of time in the heat and cold to which they were exposed, I flattered myself that the quantities of moisture remaining attached to their surfaces, could not be different as sensibly to effect the results of the experiments. —But, in regard to this last circumstance, I afterwards found reason to conclude that my opinion was erroneous.

Admitting the fact stated by Dr. Fordyce,—(and which my experiments had hitherto rather tended to corroborate than to contradict,)—I could not conceive any other cause for the augmentation of the apparent weight of water, upon its being frozen, than the loss of so great a proportion of its latent heat as that fluid is known to evolve when it congeals; and I concluded, that if the loss of latent heat added to the weight of

of one body, it must of necessity produce the same effect on another, and consequently that the augmentation of the quantity of latent heat must,—in all bodies,—and in all cases,—diminish their apparent weights.

To determine whether this is actually the case or not, I made the following experiment.

Having provided two bottles, as nearly alike as possible, and in all respects similar to those made use of in the experiments above mentioned, into one of them I put 4012,46 grains of water, and into the other an equal weight of mercury; and, sealing them hermetically, and suspending them to the arms of the balance, I suffered them to acquire the temperature of my room, 61°; then, bringing them into a perfect equilibrium with each other, I removed them into a room in which the air was at a temperature of 34°, where they remained twenty-four hours.—But there was not the least appearance of either of them acquiring, or losing any weight.

Here it is very certain, that the quantity of heat lost by the water, must have been very considerably greater than that lost by the mercury; the specific quantities of latent heat in water and in mercury, having been determined to be to each other as 1000, to 33; but this difference in the quantities of heat lost, produced no sensible difference on the weights of the fluids in question.

Had any difference of weight really existed, had it been no more than *one millionth* part of the weight of either of the fluids, I should certainly have discovered it; and, had it amounted to so much as $\frac{1}{788888}$ part of that weight, I should have been able to have measured it; so sensible, and so very accurate, is the balance which I used in these experiments.

I was now much confirmed in my suspicions, that the apparent augmentation of the weight of the water upon its being frozen, in the experiments before related, arose from some accidental cause; but I was not able to conceive what that cause could possibly be, unless it were, either a greater quantity of moisture attached to the external surface of the bottle which contained the water, than to the surface of that containing the spirits of wine,—or some vertical current or currents of air, caused by the bottles or one of them not being exactly of the temperature of the surrounding atmosphere.

Though I had foreseen, and, as I thought, guarded sufficiently against, these accidents,—by making use of bottles of the same size and form,—and which were blown of the same kind of glass,—and at the same time,—and by suffering the bottles, in the experiments, to remain for so considerable a length of time exposed to the different degrees of heat and of cold, which alternately they were made to acquire; yet, as I did not know the relative conducting powers of ice and of spirit of wine with respect to heat; or, in other words, the degrees of facility or difficulty with which they acquire the temperature

temperature of the medium in which they are exposed;—or the time taken up in that operation; and, consequently, was not *absolutely certain* as to the equality of the temperatures of the contents of the bottles at the time when their weights were compared, I determined now to repeat the experiments, with such variations as should put the matter in question out of all doubt.

I was the more anxious to assure myself of the real temperatures of the bottles and of their contents, as any difference in their temperatures might vitiate the experiment, not only by causing unequal currents in the air, but also, by causing, at the same time, a greater or less quantity of moisture to remain attached to the glafs.

To remedy these evils, and also to render the experiment more striking and satisfactory in other respects, I proceeded in the following manner.

Having provided three bottles, A, B, and C, as nearly alike as possible, and resembling in all respects those already described; into the first, A, I put 4214,28 grains of water, and a small thermometer, made on purpose for the experiment, and suspended in the bottle in such a manner that its bulb remained in the middle of the mass of water; into the second bottle, B, I put a like weight of spirit of wine, with a like thermometer; and, into the bottle, C, I put an equal weight of mercury.

These bottles, being all hermetically sealed, were placed in a large room, in a corner far removed from the doors and windows, and where the air appeared to be perfectly quiet; and, being suffered to remain in this situation more than twenty-four hours, the heat of the room (61°) being kept up all that time with as little variation as possible, and the contents of the bottles A and B appearing, by their inclosed thermometers, to be exactly at the same temperature, the bottles were all wiped with a very clean, dry cambric handkerchief; and, being afterwards suffered to remain exposed to the free air of the room a couple of hours longer, in order that any inequalities in the quantities of heat,—or of the moisture attached to their surfaces,—which might have been occasioned by the wiping, might be corrected by the operation of the atmosphere by which they were surrounded, they were all weighed, and were brought into the most exact equilibrium with each other, by means of small pieces of very fine silver wire, attached to necks of those of the bottles which were the lightest,

This being done, the bottles were all removed into a room in which the air was at 30° ; where they were suffered to remain, perfectly at rest and undisturbed, forty-eight hours; the bottles A and B being suspended to the arms of the balance, and the bottle C suspended, at an equal height, to the arm of a stand constructed for that purpose, and placed as near the balance as possible, and a very sensible thermometer suspended by the side of it.

At the end of forty-eight hours,—during which time the apparatus was left in this situation,—I entered the room, opening the door very gently, for fear of disturbing the balance;

lance; when I had the pleasure to find the three thermometers,---viz. that in the bottle A, (which was now inclosed in a solid cake of ice,) that in the bottle B, and that suspended in the open air of the room, all standing at the same point, 29° F, and the bottles A and B *remaining in the most perfect equilibrium*.

To assure myself that the play of the balance was free, I now approached it very gently, and caused it to vibrate; and I had the satisfaction to find, not only that it moved with the utmost freedom, but also, when its vibration ceased, that it rested precisely at the point from which it had set out.

I now removed the bottle B from the balance, and put the bottle C in its place; and I found that *that* likewise remained of the same apparent weight as at the beginning of the experiment, being in the same perfect equilibrium with the bottle A as at first.

I afterwards removed the whole apparatus into a warm room, and, causing the ice in the bottle A to thaw, and suffering the three bottles to remain till they and their contents had acquired the exact temperature of the surrounding air, I wiped them very clean, and, comparing them together, I found their weights remained unaltered.

This experiment I afterwards repeated several times, and always with precisely the same result; the water, *in no instance*, appearing to gain, or to lose, the least weight, upon being frozen, or upon being thawed; neither were the relative weights of the fluids in either of the other bottles in the least changed, by the various degrees of heat, and of cold, to which they were exposed.

If the bottles were weighed at a time when their contents were not *precisely of the same temperature*, they would frequently appear to have gained, or to have lost, something of their weights: but this doubtless arose from the vertical currents which they caused in the atmosphere, upon being heated or cooled in it: or to unequal quantities of moisture attached to the surfaces of the bottles;—or to both these causes operating together.

As I knew that the conducting power of mercury, with respect to heat, was considerably greater than either that of water, or that of spirit of wine, while its capacity for receiving heat is much less than that of either of them, I did not think it necessary to inclose a thermometer in the bottle C, which contained the mercury; for it was evident, that when the contents of the other two bottles should appear, by their thermometers, to have arrived at the temperature of the medium in which they were exposed, the contents of the bottle C could not fail to have acquired it also, and even to have arrived at it before them; for, the time taken up in the heating or in the cooling of any body, is, *ceteris paribus*, as the capacity of the body to receive and retain heat, *directly*, and as its conducting power, *inversely*.

The bottles were suspended to the balance by silver wires, about two inches long, with hooks at the ends of them; and, in removing and changing the bottles, I took care not

to touch the glafs. I likewise avoided, upon all occasions, and particularly in the cold room, coming near the balance with my breath, or touching it, or any part of the apparatus, with my naked hands.

Having determined that water does not acquire or lose any weight, upon being changed from a state of *fluidity* to that of *ice*, and *vice versa*, I shall now take my final leave of, a subject which has long occupied me, and which has cost me much pains and trouble; being fully convinced, (from the results of the above-mentioned experiments,) that if heat be in fact a *substance*, or matter,---a fluid *sui generis*, as has been supposed, which, passing from one body to another, and being accumulated, is the immediate cause of the phænomena we observe in heated bodies, (of which, however, I cannot help entertaining doubts,) it must be something so infinitely rare, even in its most condensed state, as to baffle all our attempts to discover its gravity. And, if the opinion which has been adopted by many of our ablest philosophers, that heat is nothing more than an intestine vibratory motion of the constituent parts of heated bodies, should be well founded, it is clear that the weights of bodies can in no wise be affected by such motion.

It is, no doubt, upon the supposition that heat is a substance distinct from the heated body, and which is accumulated in it, that all the experiments which have been undertaken, with a view to determine the weight which bodies have been supposed to gain, or lose, upon being heated or cooled, have been made; and, upon this supposition (but without, however, adopting it entirely, as I do not conceive it to be sufficiently proved,) all my researches have been directed.

The experiments with *water*, and with *ice*, were made in a manner which I take to be perfectly unexceptionable; in which no foreign cause whatever could affect the results of them; and the quantity of heat which water is known to part with, upon being frozen, is so considerable, that if this loss has no effect upon its apparent weight, it may be presumed that we shall never be able to contrive an experiment by which we can render the weight of heat sensible.

Water, upon being frozen, has been found to lose a quantity of heat amounting to 140 degrees of *Fahrenheit's* thermometer; or, which is the same thing, the heat which a given quantity of water, previously cooled to the temperature of freezing, actually loses, upon being changed to ice, if it were to be inbibed and retained by an equal quantity of water, at the given temperature, (that of freezing,) would heat it 140 degrees, or would raise it to the temperature of $(32^{\circ} + 140)$ 172° of *Fahrenheit's* thermometer, which is only 40° short of that of boiling water; consequently, any given quantity of water, at the temperature of freezing, upon being actually frozen, loses almost as much heat as, added to it, would be sufficient to make it boil.

It is clear, therefore, that the difference in the quantities of heat contained by the

water in its fluid state, and heated to the temperature of 61° F, and by the ice, in the experiments before mentioned, was, *at least*, nearly equal to that between water in a state of boiling, and the same at the temperature of freezing.

But this quantity of heat will appear much more considerable, when we consider the great capacity of water to contain heat, and the great apparent effect which the heat that water loses upon being frozen would produce, were it to be imbibed by, or communicated to, any body whose power of receiving and retaining heat is much less.

The capacity of water to receive and retain heat,—or what has been called its specific quantity of latent heat,—has been found to be to that of gold as 1000 to 50,—or as 20 to 1; consequently, the heat which any given quantity of water loses upon being frozen, were it to be communicated to an equal weight of gold, at the temperature of freezing, the gold, instead of being heated 172 degrees, would be heated $140 \times 20 = 2800$ degrees, or, would be raised to a *bright red heat*.

It appears therefore to be clearly proved, by my experiments, that a quantity of heat equal to that which 4214 grains (or about $9\frac{3}{4}$ oz.) of gold would require to heat it from the temperature of freezing water to be *red hot*, has no sensible effect upon a balance capable of indicating so small a variation of weight as that of $\frac{1}{1000000}$ part of the body in question; and, if the weight of gold is neither augmented nor lessened by *one millionth part*, upon being heated from the point of *freezing water* to that of a *bright red heat*, I think we may very safely conclude, that ALL ATTEMPTS TO DISCOVER ANY EFFECT OF HEAT UPON THE APPARENT WEIGHTS OF BODIES, WILL BE FRUITLESS.

II.

On the Plants used by the ancient People of Europe, to poison their Arrows. By C. CH. COQUEBERT.*

ALL nations that subsist by hunting have sought for quick poisons among vegetables, in which they could dip their arrows, in order to kill with more certainty the animals which constituted their food.

Historians in general have neglected to acquaint us with the plants which our ancestors, the half-savage inhabitants of Europe, used for that purpose, in remote ages. I have met accidentally with passages in two Spanish works, which throw much light on this interesting subject.

The first of these works is entitled *Sinopsis stirpium indigenarum Arragoniæ*, printed 1779, and whose author is only designed by the initial letters C. A. R. native of Sarra-

* Soc. Philomath. Feb. 1798. No. II.

gosa. This author quotes a manuscript of *Cienfuegos*, his countryman, who in 1618 wrote on the botany of Arragon, and in which he relates that in his time the Spanish huntsmen still used to poison their arrows; that the poison in which they dipped them was so speedy in its effect, that the least wound inflicted on the animal was sufficient to secure the huntsman his game. The vegetable from which they prepared it was the *Veratrum album*, (white hellebore) a plant very common on the fields of the Alpine mountains. There was, however, some skill required in preparing the confectio of the veratrum for that use; for *Cienfuegos* adds, that the King of Spain had at that time a huntsman wonderfully skilled in that art.

The second work, from which I have received some information, is the history of the war of Grenada, under *Philip* the second, by *Mendoza*. This author, whom the Spaniards highly esteem for the purity of his diction, the impartiality which distinguishes his writings, and the extent of his knowledge, says that the poison used by the huntsmen, even in his time (at the beginning of the seventeenth century), was prepared on the mountains of Bejar and Guadarrama, with the black hellebore, called in that part of Spain *el zumo de vedegambre*. The extract made from it was of a brown red. Another poisonous native plant was used for the same purpose on the lofty mountains of the kingdom of Grenada, which is called *Yerva* by the inhabitants, a word which denotes an excellent or eminently useful plant. It is the *aconitum lycoctonum*, which grows like the veratrum, on the high mountains.

The animals wounded by these poisoned arrows are affected in the same manner as those which have been wounded by the hellebore, or aconite. The symptoms are a sudden extreme weakness, shivering cold, numbness and blindness, with foam at the mouth, and convulsions of the diaphragm. *Mendoza* affirms, that two plants, which he denotes only by the Spanish names of *Membrillo* and *Retama*, of whose signification I am ignorant, are successfully used as counter-poisons.

After having read those two passages, I was desirous to see what *Haller* says of the plants in his *Historia stirpium indigenarum Helvetiæ*; or rather, in the French translation, he has given of that part of *Vicat* which relates to the properties of plants.

If, says he, the poison of the veratrum accidentally penetrates to the blood-vessels, without loss of its strength, death is the immediate consequence, even though by a very slight wound. This was observed at the time when the ancient Portuguese used to poison their arrows with the juice of that plant. *Mathiolum* has confirmed this observation by his experiments. When death is thus produced, the progress of putrefaction is so rapid that the flesh of the animal becomes soft as soon as it has ceased to breathe. *Guilquadinus* also mentions the poison the Spaniards prepared with this plant.

Two drams of the root of the veratrum, in decoction, injected into the veins of an animal, have excited immediate convulsions and vomiting, which were followed by death, and soon afterwards by a state of flaccidity.

The spirituous infusion, according to *Haller*, has more effect than the aqueous infusion, and the latter more the decoction and extract. It is to be supposed then that the virtue of the plant resides in the volatile parts which boiling evaporates.

At the article of the black hellebore (*helleborus viridis*, L.) *Haller* also affirms, that that plant is used to poison arrows. He quotes *Monardus*, who mentions of a hen which died, after a fibre of black hellebore had been passed through her comb. It is difficult, however, to ascribe so deleterious an action to that hellebore; for ever since the time of *Columella* the root has been used to make fetons for sheep, which they usually pass through the skin of the neck. It produces suppuration.

With regard to the aconites, I find in *Haller's* works, relative to that kind which *Linne* calls *aconitum cammarum*, that its juice having casually, and in a very small quantity, entered a wound, the accident was followed by cardialgia, fainting, swelling, and at length a gangrene.

It appears from these facts, that the three plants I have mentioned, particularly the veratrum, were used by the ancient inhabitants of Europe to poison their arrows; and that the use of fire-arms alone has abolished that of the poison, used by the Spaniards as late as the last century.

III.

Experiments to determine the quantity of tanning principle and Gallic acid, contained in the Barks of various Trees. By GEORGE BIGGIN, Esq.*

THE bark of trees contains the astringent principle called gallic acid, and also that principle which has a peculiar affinity to the matter of skin, and which, from the use to which it is applied, is called the tanning principle. But in the present mode of tanning, bark is applied in *mafs* to the skins; consequently *both* principles are applied. It remains for examination whether both principles are useful in the process of tanning; for if they are not both useful, probably *one* is detrimental.

To a Nobleman whose zeal on every occasion by which the sciences or arts may receive illustration or improvement, is eminently conspicuous, and to whose public energy, as well as private friendship, I feel myself much indebted, to his Grace the Duke of Bedford I owe the means of prosecuting some experiments on this subject. His Grace, by collecting a variety of barks at Woburn, gave me an opportunity of making

* *Philos. Transf.* 1799. p. 299.

some experiments to ascertain the quantity of tanning principle and gallic acid, each bark contained. For that purpose I made use of the following methods, according to the principles laid down by Mr. Seguin.

By dissolving an ounce of common glue in two pounds of boiling water, I procured a mucilaginous liquor, which as it contains the matter of skin in solution, is a test for the tanning principle. By a saturated solution of sulphate of iron, I obtained a test for the Gallic acid.

I then took one pound of the bark I meant to try, ground as for the use of tanners, and divided it into five parts, each part being put into an earthen vessel. To one part of this bark I added two pounds of water, and infused them for one hour. Thus I procured an infusion of bark which I poured on the second part of the bark, and this strengthened infusion again on the third part, and so on to the fifth. But as a certain portion of the infusion will remain attached to the *wood* of the bark, after the infusion is poured or drawn off, I added a third pound of water to the first part, and then followed up the infusion on the several parts till the three pounds of water, or so much of them as could be separated from the bark, were united in the fifth vessel, from which I generally obtained about one pint of strong infusion of bark.*

To a certain quantity of this infusion, I added a given measure of the solution of glue; which formed an immediate precipitate, that may be separated from the infusion, by filtering paper. When dried it is a substance, formed by the chemical union of the matter of skin with the tanning principle, and is in fact a powder of leather. By saturating the infusion with the solution of glue, the whole of the tanning principle may be separated by precipitation.

FOR THE GALLIC ACID.

To the pound of bark left in the earthen vessel and already deprived of its tanning principle by these *quick* infusions, I added a *given* quantity of water to procure a strong infusion of the gallic acid which requires a longer time (say 48 hours). This infusion when obtained pure† affords little signs of the presence of the tanning principle when tried by the test of the solution of glue; but with the solution of sulphate of iron it gives a strong black colour (the common black dye), which differs in density according to the quality of the bark; this may be further proved by boiling a skain of worsted in the dye, by which the gradations of colour will be very perceptibly demonstrated.

* The specific gravity of this infusion was ascertained by an hydrometer, whose gradations are inverse to those of a spirit hydrometer.

† It is hardly possible from the intimate connection of the two principles, to separate them entirely by infusion; in the infusion of tanning principle there will always exist a little gallic acid; and in an infusion of gallic acid a little tanning principle will commonly be present, unless the infusion of gallic acid is very weak, and procured by a third or fourth watering.

Having

Having thus obtained a point of comparison by making a similar infusion under similar circumstances of any bark or vegetable substance, and paying strict attention to the specific gravity of the infusion, the quantity of precipitate of leather, and the density of colour produced by given quantities of one or the other test, the result will be a comparative statement of the respective powers of any bark or vegetable substance. This comparative statement I conceive to be sufficient for all commercial purposes.

As oak bark is the usual substance employed in the trade of tanning; if a quantity of tanning principle is found to be contained in any other bark or vegetable, the commercial utility of that bark or vegetable may be determined by comparing its quantity of tanning principle and price with those of oak bark.

For an accurate chemical analysis I have tried a variety of acids and simple and compound affinities; and have pursued the above experiments at the same time that I was employed on some in dying, I found the muriate of tin (the method of using which is described by Mr. Proust, in the *Annales de Chimie*) very convenient. A solution of it being added to the infusion of bark, forms a precipitate with the tanning principle, leaving the gallic acid suspended; the precipitate is of a fawn colour; and is composed of tanning principles and oxidated tin.

By these means, I have been enabled to form a comparative scale of barks; which, however, I do not produce as accurate. Oak bark in its present state, as procured for commercial purposes, differs very much in quality from accidental circumstances; the season of the year in which it is collected occasions a still more important difference; consequently the scale now produced must be very imperfect; but I am of opinion that by the pursuits of scientific men, who may be inclined to investigate this subject more fully, a very accurate scale may hereafter be found.

In the following scale I have taken sumach as the most powerful in the comparative statement; leaving, however, a few degrees for a *supposed maximum of tanning principle*, which I reckon 20.

Scale of Barks.

Bark of	Gallic acid by colour.	Tanning principle by hydrometer.	Tanning principle (in grains) from half a pint of infusion and an ounce of solution of glue.
Elm*	7	2,1	28
Oak cut in winter	8	2,1	30
Horse chestnut	6	2,2	30
Beech	7	2,4	31

* The infusion of elm was so loaded with mucilage that it was with difficulty I could separate the tanning principle, or try the specific gravity.

Willow (boughs)	-	-	8	2,4	31
Elder	-	-	4	3,0	41
Elm tree	-	-	8	4,0	58
Willow (trunk)	-	-	9	4,0	52
Sycamore	-	-	6	4,1	53
Birch	-	-	4	4,1	54
Cherry tree	-	-	8	4,2	59
Sallow	-	-	8	4,6	59
Mountain ash	-	-	8	4,7	60
Poplar	-	-	8	6,0	76
Hazel	-	-	9	6,3	79
Ash	-	-	10	6,6	82
Spanish chestnut	-	-	10	9,0	98
Smooth oak	-	-	10	9,2	104
Oak cut in Spring	-	-	10	9,6	108
Huntingdon or Leicester willow	-	-	10	10,1	109
Sumach	-	-	14	16,2	158

It is to be observed, that the barks do not keep any respective proportion in the quality of gallic acid and tanning principle contained in each; which is an evidence of the distinctness of principle, and may, perhaps, open a new field of saving oak bark in dyeing, as the willows, fallow, ash, and others, produce a very fine black. It is also worthy of observation, that the quantities of gallic acid and tanning principle do not differ in *equal* proportions between the winter and spring felled oaks. This fact may lead to the discrimination of the proper time for cutting; which is, probably, when the sap has completely filled and dilated that part of the vegetable intended for use. This will make a difference in the season of cutting oak, elm, and other trees, shrubs, &c. Leaves should be taken when arrived at their full size, and then dried under cover; for as the tanning principle is so soluble, and the substance that contains it so thin (in a leaf), the dew alone might dissolve it.

Finally, as the gallic acid does not seem to combine with the matter of skin, and as its astringency will corrugate the surface, we may, I think, conclude that its presence in tanning is not only useless but detrimental.

IV.

On the Plumb Line and Spirit Level. By M. CHEZY.*

A WEIGHT suspended by a long slender and flexible thread, and acted upon by no other force than that of gravity, will stretch the string into a right line perpendicular to the horizon; or which is the same thing, the plane to which this line is perpendicular is considered as the horizon itself, or a parallel to that circle.

Such a weight affords, therefore, a very simple and easy method of determining the level at all places. But this method is not without its inconveniences, when the utmost precision is required, and in levelling this precision is often necessary to be had.

It is scarcely possible to use a thread, or wire, much finer than one fourth part of the thickness of an hair. If to this be given a length of ten feet from the point of suspension to the mark intended to be intersected by the wire for the purpose of ascertaining the situation of any instrument; this instrument will be inconvenient, on account of its magnitude, and of the agitation which the least movement of the air will produce, notwithstanding the guard commonly made use of. If the wire be short or thick, the precision will be less, and even in the first case it will not be very great unless the most scrupulous attention of a skilful observer be applied to insure it. To judge of this degree of accuracy the diameter of a hair was measured, and it was found that thirty or thirty-five were required to cover the space of one line of the Royal Paris foot; that is to say, the thickness of an hair is about $\frac{1}{35}$ or $\frac{2}{1088}$. The fine or arc of one second to a radius of ten feet is 0,0069816 of a line, or nearly 0,007, or the $\frac{1}{4}$ part of the thickness of a hair; a quantity, so small, as easily to elude common observation, and much less than the derangement which the slightest current of air can produce in the wire.

The same force of gravity which stretches the thread by means of a weight, likewise renders the surface of fluids horizontal. If any very fluid liquor, such as spirit of wine, be included in the cavity of a glass tube, so as not to occupy its whole capacity, the empty space will be constantly at the upper part of the tube; that is to say, at one of the ends if its position be not horizontal, or in the middle if it be so; and by a very slight inclination the bubble will be removed to a great distance from the middle of the tube. A tube of this kind is considered as being very useful to ascertain the position of the horizon, with great accuracy; and, for this purpose, it is very successfully used under the name of the spirit level.

These levels are commonly made of glass tubes in the state they are obtained at the glass-house. Of these the straightest and most regular are selected and examined, by filling

* On a former occasion, when speaking of these Instruments, (Philos. Journal I. 134.) I forbore to enter upon the methods of grinding the Spirit Level; but having since met with the present valuable memoir I have thought it desirable to treat the subject more fully.—N.

them nearly with spirit of wine, and ascertaining by trial that side at which the bubble moves most regularly, by equal inclinations of the instrument upon a stage, called the bubble trier, which is provided with a micrometer screw, for that purpose. The most regular side is chosen for the upper part of the instrument, the others being of little consequence to its perfection. Spirit of wine is used because it does not freeze, and is more fluid than water. Ether is better because still more fluid (1). The tube and the bubble must be of considerable length. The longer the bubble the more sensible (2) it is to the smallest inclination. A very small bubble is scarcely sensible, appears as if attached to the glass and moves but slowly.

In the use of a level of this kind, constructed by *Sieur Langlois*, it was remarked, that when it was properly set, in the cool of the morning, was no longer so in the middle of the day, when the weather became hot; and that when it was again rectified for the middle of the day it became false in the evening, after the heat had diminished. The bubble was much longer in cold than in hot weather, and when longer it was too much so, and could not be kept in the middle of the tube; but stood a little on the one or the other side, tho' the inclination was precisely the same. These defects were small, and such as claim the notice of careful observers only; but they appeared of too much consequence not to produce a wish to remedy them (3). It was observed, that they arose from irregularities in the interior surface of the tube; and by examining a great number of tubes, selected for levels of the same kind, there was reason to conclude that all these levels would have more or less of the same defects, because there was not one tube of a regular figure within. They were at best no otherwise cylindrical than plates of glass from the glass-house can be said to be plane before they are ground. The irregularities were easily discernable.

It was, therefore, concluded, that it would be advisable to grind the inner surfaces of the tubes, and give them a regular cylindrical or rather spindle form, of which the two opposite sides should correspond with portions of circles of very long radius. To accomplish this, a rod of iron was taken, of twice the length of the glass tube, and on the middle of this rod was fixed a stout tube of copper (*cuivre*) of the same length as the tube of glass, and nearly equal in diameter to the bore. The rod was fixed between the centres of a lathe, and the glass gently rubbed on the copper cylinder, with fine emery and water, causing it to move through its whole length. The glass was held by the middle, in order that it might be equally ground, and was from time to time shifted on its axis, as was also the copper cylinder, in order that the wear might be every where alike. The operation had scarcely commenced, before the tube broke; and several others experienced the same misfortune, though they had been well annealed. It

was supposed that the emery which became fixed in the copper* might contribute to split the glass, each grain continuing its impression with the same point, in the same right line, which in some instances might be as well disposed to cut the glass as a diamond. A cylinder of glass was substituted, instead of the copper, and the emery rolling itself on the surface of the last, instead of fixing itself, had better success; so that every part of the circumference of the tube and the cylinder touched each other through their whole length. The same operation was continued, using finer and finer emery to smooth the tube, and prepare it for polishing; after which the tube and cylinder having been well washed, thin paper was passed round the cylinder, and the paper was very equally covered with a small quantity of Venice Tripoli. The tube was then replaced and rubbed as before, till it had acquired a polish†.

A level thus ground, may be either of the proper sensibility, or be too much or too little sensible. It will be too sluggish, if before grinding, exclusive of the irregularities of the tube, its diameter should much exceed in the middle of the length the diameter of the extremities; or it will be too sensible if this diameter should not sufficiently exceed the other; or lastly, if the middle diameter be smaller than that of the extremes, the bubble will be incapable of continuing in the middle, but will, in every case, either run to one or the other end, or be divided into two parts.

To correct these defects, and to give the instrument the required degree of perfection, it is proper to examine its figure before the grinding is entirely finished. For this purpose, after cleaning it well, a sufficient quantity of spirit of wine must be put into it; and secured by a cork at each end. The tube must then be placed on the forks or Y's of a bubble trier, and its sensibility, or the magnitude and regularity of the space run over by the bubble by equal changes of the micrometer screw, must be ascertained. If the run or spaces passed over be too great (5) they may be rendered smaller by grinding the tube on a shorter cylinder; but if they be too short, they may, on the contrary, be enlarged, by grinding on a longer cylinder. It is necessary, therefore, to be provided with a number of these cylinders of the same diameter, but of different lengths, which it is advisable to bring to a first figure, by grinding them in a hollow half cylinder of brass. By means of these it will be easy to regulate the tube of the level to any required degree of sensibility, after which the tube may be very quickly smoothed and polished.

* I am informed that our artists use brass (*cuiivre jaune*) without this inconvenience. It is probable that the copper being defended by the emery bedded in its face did not itself grind away so as to fit the glass during the operation, and therefore broke it. This property of copper is well known to glass cutters and other artists. A drill, consisting of a very thin copper tube will, with emery, cut a hole through glass very readily; and a small circular plate of the same metal, no thicker than a card, will cut through a piece of glass or the hardest file, without any perceptible wear of the copper itself.—N.

† Our artists find the polishing rather noxious than beneficial, and have, therefore, discontinued it.—N.

The

The level which was thus ground is one foot in length; and the cylinder on which it was first worked is of the same length. When it was finished it was found to be too sensible. It was, therefore, worked on another cylinder of between nine and ten inches long, which diminished its sensibility so far, that the bubble, which is nine inches and four lines long, at the temperature of 16° of Reaumur, above freezing is carried from the middle of the tube exactly one line for every second of a degree of inclination. This degree of sensibility was thought sufficient; but any greater degree which may be required may be obtained by the process here described.

It may be remarked that a glass tube is very subject to be split by grinding its inner surface; the same tube will not be endangered by grinding its external surface even with coarse emery; and when once the polish of the inside is ground off, the danger is over, and coarser emery may be used without fear. Thick glass is more subject to this misfortune than thinner. The coarsest emery made use of in grinding the tube here spoken of was sufficiently fine to employ one minute in descending through the height of three inches in water.

Annotations by the Author of the preceding Memoir.

(1.) If the ether be not well rectified it is subject to two great inconveniences in this use. If the tube be very slightly agitated, the ether divides itself into several bubbles, which employ a considerable time before they unite. In the second place, as this ether is decomposed in the course of time, it deposits very small drops of oil which adhere to the tube, stop the motion of the bubble, and render the level very faulty. The ether is besides more fluid when rectified and freed from a saponaceous matter which causes its bad effects*.

(2.) A level is said to be more sensible the smaller the inclination required to produce a given change in the position of the bubble†.

Let E M F, fig. 1. pl. xvii. represent the internal irregularity of a tube. It may easily be understood that when the elevation of temperature dilates the liquor in the tube,

* Our artists use ether, and find that the precautions here mentioned are very necessary.—N.

† And the more quickly and rapidly it returns to its first situation when the micrometer screw is suddenly returned to its zero. These three qualities do not constantly accompany each other in common levels.—It is not immediately obvious why a long bubble should be more sensible, as the run is exactly the same whatever may be the size of the vacant space. To explain this let A H B I, fig. 5; plate xviii. represent a hollow ring, filled with fluid to A and B on a level with the center C; then if the vertical point H be inclined to either side, the surfaces A and B will have an apparent run, along the tube, of exactly the same quantity. And the same would follow if the tube were filled to E and D, or to G and F. But the actual rise or perpendicular elevation of the fluid required to pass over a given arc will be as the sine of half the arc left empty, or half the length of the bubble, and consequently the momentum by which the cohesive attraction at the edges of the bubble is to be overcome will be in the same proportion. The longer bubble will therefore move more briskly and settle more accurately to its station.—N.

and contracts the bubble, this last may occupy the space AB precisely in the middle of the tube, and the instrument being then rectified, will indicate the level. But when it becomes cold, the liquor being condensed, the bubble will occupy the space CD , which is no longer in the middle of the tube. A good instrument ought always to shew the level when the bubble is in the middle of the tube, which is the rule followed in practice; it is therefore subject to error when the tube is irregular. Irregularities, not discernable to the sight, are of much consequence to the station of the bubble. These may vary without limit and produce a correspondent number of variations in the bubble itself, which will in some cases be too sensible, and in others not enough so, will be much affected by the same degrees of inclination in some cases, and not at all in others, or it may stand to the right or left end of the tube without being capable of fixing itself in the middle.

(4.) The length of the radius of curvature of the inside of the tube depends on its sensibility, or rather this sensibility is in proportion to the length of the radius. The radius of curvature of the surface of the earth is likewise of some consequence when the sensibility is very great.

The sensibility of a level may be expressed by the space run over by the bubble in the tube, divided by the degree of inclination which has occasioned the derangement; or if the degree of inclination be supposed to be a constant quantity, as for example one second, the sensibility will be simply as the space passed over.

Let AB , fig. 2, plate xviii. represent this space; AC the radius of the earth, and AD the radius of curvature, Ab , of the inside of the tube W when the tube is inclined so as to cause the bubble to run over the space AB , its radius bD will apply itself upon BC the same as AD was before applied upon AC . The angle DBC (or $D b C$, because the space bB may be considered as nothing on account of the extreme minuteness of the angle bAB) will be the angle of the inclination of the tube, which we suppose to be constantly one second. The angle ACB will be known if the radius of the earth and the space AB be known. The angle ADB is constantly the sum of the two external angles, and may also be known if AD and AB be ascertained. The angles ADB , ACB , being subtended by the same arc, will be to each other inversely as the two radii AD , AC . Of three quantities, AC , AD , AB , two being known, the third may also be found.

Let $AB = a$, $AD = r$, $AC = R$, the angle $CBD = b$, the angle $ACB = m$, and the angle $ADB = n$. If the value of r be sought, every thing else being known, we shall have $b + m : m :: R : r = \frac{mR}{b + m}$. If therefore a were one line m being nearly $\frac{1''}{13760}$ we shall have $r = 238$ toises nearly.

If the value of a be sought, or, which is the same thing, the value of m , the rest being

being known, we shall have $R : r :: b + m : m = \frac{r b}{R - r}$. If we make $r = R$, m and a will become infinite, if we make $r = \frac{R}{2}$ we shall have $m = b$, and consequently, $a = 16$ toises nearly. If r be made $= 50000$ toises, placing the tube in the direction of the meridian, we shall have at the Equator $m = 0,015616''$ and $a = 0,2461749$ toises, and at the pole $m = 0,015353''$, and $a = 0,2461187$ toises.

If, lastly, the value of R be sought, the angle $A D B$ being known, we shall have $n - b : r :: n : R = \frac{n r}{n - b}$. This determination will always be very uncertain, on account of the extreme smallness of $A B$. We have seen in the last preceding example that a difference of $\frac{1}{24}$ part of a line in $A B$ produces a difference of about 55000 toises in $A C$, though we have supposed a very great degree of sensibility in the tube.

(5.) In order to form a notion how this practice may increase or diminish the sensibility of a tube, let us imagine any two portions of matter whatever to be placed one upon the other, with very fine sand between them, so that they may be worn by friction; for example, two pieces of glass. If these two pieces $A B, C D$, fig. 3, of which it is unnecessary at present to consider the breadth, be of equal length and rubbed one against the other in the direction of their length, there will almost constantly be a part, either large or small, where one will overhang the other, which part consequently will not be rubbed while the other continues to be acted upon by the friction. The two pieces are precisely in the same situation, because the parts $B D, A C$, which are less rubbed than the middle, are equal to each other; each of these pieces is therefore equally disposed to become hollow in the middle, which, however, cannot happen, because it is impossible they should wear except at the places which touch, and they tend by grinding to touch each other every where in a right line or a circle, which are the only lines that in the different positions here supposed can touch each other every where. This is evidently impossible, provided both should become hollow at the same time, and the one cannot become hollow sooner than the other.

But if the two pieces be unequal in length, as in fig. 4, the longest of the two will constantly project beyond the other; and if the shorter one do project, the longer will project still more at the same time. It will therefore be more disposed to wear away and become hollow in the middle. This concavity or curvature will increase the longer the grinding is continued, while the shorter piece will become convex in a circular line; both having the same radius in order that they may constantly touch throughout*.

(6.) In

* The above reasoning is not accurate to the extreme of strictness. The figure of surfaces ground upon each other is liable to vary from partial expansions, if the work be carelessly touched by the hand, and in equal pieces

(6.) In order to obtain very fine emery, and of different degrees of fineness, the sieve is not sufficient, but water must be used. After having well ground the coarse emery on an iron plate with a muller of the same metal, it is thrown into a vessel which must be rather wider at bottom than at top, by a gradual increase. The vessel must then be filled with clear water so that it may stand eight or ten inches above the emery. The whole is then to be strongly agitated with an iron spatula; after which it must be left to settle for an hour. The emery falls to the bottom, but the water remains turbid, being charged with emery or some other extremely fine and light matter. Into this water must be plunged the shortest branch of a syphon filled with clear water to the depth of four inches below the surface of the water, holding the other extremity of the syphon closed with the finger, which is afterwards withdrawn that the water may flow through the syphon without stirring the vessel or agitating the emery which is at the bottom. The water thus drawn off, is to be received in another large vessel, and the first vessel is to be again filled, and its contents agitated as before. The same operation is to be repeated till the water passes clear through the syphon. The powder which has passed over with the water and was received in the second vessel is too fine to be usefully employed in grinding glafs. The vessel being emptied and cleared, the same operations are to be repeated, with this difference only, that instead of leaving the water to settle for an hour, no longer time is employed than half an hour, and when the water passes clear through the syphon, the operation is discontinued, and the emery obtained by subsidence from the water, and carefully defended from all impurities, is reserved under the denomination of emery of half an hour.

The same operations being repeated, allowing only a quarter of an hour for the subsidence, affords an emery, which is indeed fine, but less so than the preceding, and is to be reserved under the denomination of emery of a quarter of an hour.

By similar processes emery may be obtained of half a quarter of an hour, of four, of two, of one, of one half, or one quarter of a minute. To measure the half or quarter of a minute, a pendulum beating seconds (or a weight suspended by a string of 39,2 English inches) may be used. The oscillations are to be counted from the moment of the agitation, and at the instant of the thirtieth or fifteenth oscillation, the finger being withdrawn from the mouth of the syphon, suffers the water to flow. The sieve may be used for coarser emery.

of metal it is well known that the figure does not continue strait unless particular care be taken not only to avoid partial pressure and expansion, but likewise to shift them with respect to each other, so that the pieces may alternately be uppermost. These and other precautions which are sufficiently known to those who make metallic speculums are also necessary to be attended to in the construction of levels.—N.

A Chemical Examination of the Bath Waters. By GEORGE SMITH GIBBES, M.D.F.R.S.

(Continued from p. 363.)

ONE hundred and ten ounces of the King's Bath water left after evaporation to dryness eighty grains of solid matter. I evaporated this water in a new tin vessel, and I found that the solid substance adhered strongly to the sides of the vessel. I made use of an ivory knife to detach this substance, and I observed that the knife was thereby considerably abraded. This circumstance induced me to examine very particularly the nature of the products, and I found that a large proportion remained insoluble after I had subjected them to the action of the three strong mineral acids. To this insoluble substance, I added above a thousand times its quantity of distilled water; still I perceived that it remained unchanged. This precipitate descended rapidly to the bottom of the vessel. I separated it by a filtre from the water, and I found it to possess no saline or earthy taste.

In order to procure a larger quantity of this substance, I evaporated 168 ounces of the King's Bath water nearly to dryness. On this residuum I poured a considerable quantity of nitric acid, and I left it to stand for above an hour, I then added a large quantity of boiling water, from which a white precipitate fell rapidly to the bottom of the vessel. I filtered the liquor, and I found, after carefully drying the substance left on the filtre, that it weighed twenty grains. The oxalic acid does not decompose it, neither was its quantity diminished by boiling it a considerable time in distilled water.

Professor *Bergman* observes, in his *Analysis of Mineral Waters*, that the portion, when the other ingredients are separated, which resists the action of a sufficient quantity of marine acid is silicious earth, which may be farther determined by the blow-pipe; for this earth, when added to the mineral alkali in fusion, unites with it with a violent effervescence, and is thereby totally dissolved.

Mr. *Kirwan* says, that the general method of discovering the silicious earth, is to evaporate a large quantity of water nearly to dryness, then to supersaturate and re-dissolve all that may have been precipitated by adding a sufficiency of nitrous or vitriolic acids, and then evaporate to dryness. If then the dry mass be once more re-dissolved in water, and filtered, the silicious earth will remain on the filtre. It is distinguished by its insolubility in most acids, and its vitrescibility with two parts soda.

I exposed six grains of this substance mixed with double its weight of soda, in a small platina crucible to a very strong heat which I urged with a pair of double bellows, and
I found

I found that it acquired a vitreous appearance after having suffered a very considerable effervescence similar to that in making glass. I have already mentioned that this substance resisted the action of the marine, nitric and vitriolic acids.

From the foregoing experiments, I am led to believe, that this insoluble substance is silicious earth; I have, however, been very cautious and exact in my experiments to ascertain it, because no one who has hitherto analysed these waters, has mentioned any thing concerning its presence in them. It is a fact generally admitted, that water is capable of holding in solution a very small portion of silicious earth, but under some circumstances, a considerable quantity as in the Geyser and Rykum waters, so admirably proved by Dr. Black. The uniform temperature of the Bath waters would lead us to conclude, that they acquire their heat from a continued and powerful operation in the bowels of the earth. The length of time that they have been known to pour forth their hot streams, would induce us to think that the cause is uniform, and very deeply situated in the bowels of the earth. I beg leave to offer a conjecture respecting that regularity of their temperature; which is, that at some great depth in the earth they are at a very high temperature, and that in coming up to the surface of the earth, their temperature is lowered to the degree they are found to possess. Thus they appear to me to be analogous to the Geyser in Iceland, and that at a certain depth they would be found to have the same appearances. That they are not of the nature of common springs of water is evident to me from their not being affected by the vicissitudes of weather, or by the alternation of wet and dry seasons. As I do not find that there is any portion of disengaged alkali in these waters, and as I cannot discover any substance which can render silicious earth soluble in them, I must hazard the following conjecture respecting its state of combination. I suppose that at a considerable depth these waters are of a temperature capable of dissolving many substances, which water is incapable of, at any ordinary degree of heat, and among them silicious earth. This earth would be therefore extremely divided, and sufficiently so I apprehend, for a part to remain suspended, or even dissolved by them, when they arrive at the surface of the earth. On evaporation, I suppose, these minute particles to coalesce sufficiently to prevent any fresh union with water. There is a fact which corroborates my opinion; which is that after a few years, so great is the deposition of a fine sand in the reservoir, that they are obliged to clear it away to prevent the pumps, &c. from being obstructed.

(To be concluded in our next.)

VI.

*Account of the New Gazometer of Citizen SEGUIN.**

CITIZEN Seguin has contrived a Gazometer, or Instrument for measuring the gases, which he proposes to substitute to the Gazometer of *Lavoisier*, and of which the aim is to dispense with the corrections required to be made for the variations of the barometer, during the course of the experiment. By this new Gazometer the elastic fluid is subjected to an invariable pressure, by an artificial regulated pressure, substituted instead of that of the atmosphere. This pressure is effected by means of a quantity of water introduced at pleasure into the reservoirs appropriated to contain the gases.

The instrument is composed of four reservoirs. The first performs the same office with regard to the second as the reservoir of a fountain lamp; that is to say, it obviates the necessity of filling it as often as would otherwise be the case.—The second transmits the water into the third, to produce the required degree of compression.—The third receives one of the gases and communicates with the fourth, in which the mixture of the gases, subjected to the same degree of pressure, is made.—Each reservoir has a kind of gage or level, which shows the proportion of water and gas within. The first reservoir communicates with a bottle, which shews the state of its contents. A tube or level open at top, and communicating below with the second reservoir, shews the height of the water within.—A level communicating with a third vessel, above as well as below, that is to say, with the part filled with gas, as well as that which contains the water; likewise shews the relative quantities occupied by the two fluids respectively in the vessel. A cock, of which the tube communicates with the last mentioned level, serves to empty this vessel, by suffering the water to flow out when gas is required to be introduced into this third reservoir.—Three tubes or levels are adapted to the fourth vessel. The first placed in the middle, communicates at the same time with the parts of this vessel which respectively contain gas and water. It shews the relative proportions of water and gas as they exist in the reservoir. Another tube communicating at top with the tube leading from the third reservoir, and at bottom with the part occupied by water in the fourth, shews the degree of pressure exercised by the condensed gas upon the water of the reservoirs, and stands lower than the first level.—The third tube communicates at bottom with the fourth reservoir, but is open at top. It shews the elevation to which water can be raised by the pressure exerted by the gas in the fourth vessel. It, therefore, stands higher than the first vessel by the same quantity as the first stands higher than the second. The author calls these gages by the names of the real level, the level of pressure, and the level of reaction.

* Communicated to the National Institute of France, and abridged by Hallé, in the Bulletin of the Philomath. Society, No. 10, page 75.

This fourth reservoir also receives the water it contains from the second vessel by a tube for that purpose. It receives the gas from the third by a bended tube, plunged beneath the water it contains, through which the gas passes by a cap, like that of a watering pot.

Graduated semicircles, into the description of which Mr. *Hallé* does not enter, are employed, to shew the precise state of the contained fluids.

In Plate 17, the numbers 1, 2, 3, 4 represent the reservoirs. A is the pipe through which gas is conveyed into the third reservoir; BBB is the bended tube through which the same gas is conveyed from the third reservoir into the fourth; C the perforated cap through which the gas is passed beneath the water of the fourth reservoir. D, the tube through which the other gas is conveyed into the fourth reservoir, to be mixed with the first—a, pipe of communication to convey water from the first reservoir to the second; b, pipe of communication from the external air of the second reservoir to the top of the first; c, bottle communicating with the first reservoir; d, pipe of communication from the bottle to the first reservoir; e, bended tube plunged in the water of the second reservoir; f, syphon which delivers water from the bottle into a small cap suspended from the neck of the bottle, which bottle itself is supported by a connection with the first reservoir; g, pipe through which water is transmitted from the second reservoir to the third; h, pipe through which water passes from the third reservoir to the fourth; k, cock for discharging the water of the three reservoirs; l, level for the second reservoir; m, level for the third reservoir; n, real level for the fourth reservoir; o, level of pressure for the fourth reservoir; p, level of reaction for the same reservoir; q, graduated semicircles to shew the state of the contained fluids.

VII.

*Instructions for making Red Crayons. By A. F. LOMET.**

IT is not without difficulty that Crayons of a good quality can be obtained for drawing-schools, particularly at a distance from the metropolis. The native ochre sawed into pieces, which are commonly used, is almost always hard and of an uneven and gravelly consistence; so that the outlines in drawing, for which it is used, cannot be made either with the softness or the precision requisite to produce the desired effect. The only good crayons that can be procured have been hitherto manufactured in Paris exclusively, where they have been long sold at a very high price. The best are known by the name of crayons *de pâte du citâ. Desmarest*, who was probably the inventor. None

* *Annales de Chimie* xxx, 293.

of those who have written on the composition of those crayons, having mentioned the quantity of ingredients which ought to be employed, I have made a series of experiments on all the combinations possible to be made with the substances proper for that fabrication. I have rejected those products that have not answered the purpose of my enquiries, and I here offer those processes which have afforded satisfactory results.

These crayons are composed of soft ochre, which is an oxide or bog ore of iron, containing a mixture of earth, of the nature of the clays, which we called hematite. They incorporate it with some binding substance, such as lime, glue, or rosin, mixed sometimes with soap, to take off the hardness of the composition. Instead of the ochre, called Sanguine in French; the other ochres, such as those known by the name of brown-red, or colcothar of vitriol may be used. In this case it is necessary to chose them soft to the touch, and of a lively colour; those that are intended for sale being often mixed with clay; which gives them a yellowish and dull colour, and ought consequently to be avoided.

I have made the experiment of incorporating those substances with the white of eggs and the albumen of the blood; but the crayons were not of a good quality. The softest ochre should be taken and ground with pure water on a marble, as practised with the colours used for painting, observing to moisten it no more than may be absolutely necessary, to facilitate the sliding of the muller.

When the operation of grinding in the large way becomes difficult and too expensive, another method may be practised to divide the colouring matters. After pounding they are sifted through a silk sieve, then diluted with much water in tubs, and after strong agitation, left to settle for a few minutes; that is to say, for the time sufficient to suffer the coarsest parts to fall to the bottom. The water highly charged with the finest particles is then drawn off, and left to settle for four and twenty hours, when the clear water is also decanted off. A very fine powder is thus obtained. The coarse remainder is to be ground and treated in the same manner till the whole mass be reduced to a state of extreme division.

The gum, the glue, or the soap intended to give a sufficient degree of solidity to the crayons, are to be separately dissolved, and the solutions worked up with the grinded ochre. The mixture is then dried by exposing it to the sun, or before a fire by a very moderate heat, taking care to stir it often till the paste has acquired the consistence of butter.

The crayons are then to be fashioned or moulded, which may be done in two ways. The first consists in spreading the paste upon a board, in which a number of channels or half round grooves are made, of an indeterminate length, but of a depth and width proportioned to the size of the crayons intended to be manufactured. The second method, which is the best, consists in pressing the paste through the pipe of a syringe;

whose orifice is equal to the size of these crayons. The pieces when moulded in this manner, are left to dry. This desiccation ought to be performed slowly, and in the shade, to prevent cracks, which would be formed, if this precaution were not attended to.

When the pieces are dried, they are to be divided into lengths of two inches (or five centimetres) each; the edges are rounded off and they are roughly pointed. They must then be scraped to remove a hard external surface which they acquire during their drying, and would prevent their marking.

A slight coating of oil must be smeared over the wooden moulds, to prevent the taste from adhering to the sides.

Gum arabic and isinglass are to be preferably used. The gum and soap may be dissolved in water; but the isinglass must be first cut into small pieces, afterwards put into warm water and dissolved on a water-bath. These solutions must be sufficiently liquid to pass through a hair-sieve, and leave their impurities behind.

It is not without difficulty that the paste incorporates with the solution of glue. Both must be warm, and the mixture must be made over the fire at the boiling water heat.

The paste must be well mixed together before it is put into the moulds, in order that it may be uniformly incorporated with the solution, and leave no hard parts. It would be better to work it with the painter's muller, and to grind it for a short time on the stone, before it is put into the moulds.

It is only in the crayons which contain gum that soap can be admitted. None of the experiments in which glue and soap have been used together have ever succeeded; and it must be so, because the excess of the alkali of the soap acting on the gelatine destroys its adhesive quality.

As those crayons, into the corporation of which soap enters, are found to afford a darker shade, it seems that this combination takes a portion of the oxygen from the red oxide of iron, and renders it brown by bringing it nearer to the state of the martial ethiops. I have observed that all the pastes prepared with the oxide of iron, even if with mere water, become brown on their exterior surface during the drying. This effect takes place in a more evident manner when they are exposed to the sun; and it appears to arise from the light taking a portion of the oxygen to the oxide of iron. I shall hereafter return to the chemical properties of these kinds of preparations, but my present purpose is only to describe those processes of fabrication in which I have constantly succeeded, in order that they may be repeated with success.

The crayons composed after these directions have all the good qualities which can be required; they do not cost one fourth of the price of those in the shops; but it must be noticed that their composition requires great exactness in the quantity prescribed, be-
cause

cause the least alteration occasions a considerable difference in the quality of the paste. It is particularly necessary to guard against those errors which may happen from inevitable diminution, during the time of the manipulation. The best way to prevent this, will be to ascertain by experiments, the quantity of water and grinded ochre which the solutions may contain before mixing them.

By means of the quantities stated in the following table for each of those kinds of crayons, it will be easy to know the proportional quantities of gum, glue, or soap, which may be employed for a determinate weight of ochre or red oxide of iron.

Substances to be employed, with their quantities and results.

No. 1.				These crayons are very friable, but they may be used for large drawings. These contain the least possible quantity of gum. If less be used they will not have sufficient consistence to be of any use.
Dry ochre, or red oxide of iron	10.	gram.		
Dry gum arabic	-	-	1.311 gram.	
No. 2.				Soft crayons, rather friable, and excellent for large drawings.
Ochre, &c.	-	-	10. gram.	
Gum	-	-	0.363 gram.	
No. 3.				Smooth and solid crayons, fit for common use.
Ochre	-	-	10. gram.	
Gum	-	-	0.415 gram.	
Or still better with	-	-	0.441 gram.	
No. 4.				Soft firm crayons, for drawings which require delicacy and precision.
Ochre	-	-	10. gram.	
Gum	-	-	0.467 gram.	
No. 5.				Very firm crayons, fit for small drawings, which require to be highly finished.
Ochre	-	-	10. gram.	
Gum	-	-	0.519 gram.	
No. 6.				Crayons almost too hard to be used. This is the maximum of gum that can be employed in their composition.
Ochre	.	.	10. gram.	
Gum	.	.	0.571 gram.	
No. 7.				These crayons are of a darker colour than the others mentioned; they are of a very hard consistence, and soft to the touch; but all crayons into whose composition any soap enters, have the defect of making strokes which shine too much when retouched. None of my experiments with soap succeeded. These crayons very much resemble those of the composition of C ₂ Desmarestt.
Ochre	.	.	10. gram.	
Gum	.	.	0.380 gram.	
White dried Soap	.	.	0.519 gram.	

Chemical

VIII.

Chemical Experiments and Observations on the Production of Sugar, and an useful Syrup from indigeneous Plants. By SIGISMUND FREDERIC HERMSTADT.

(Concluded from page 339.)

h. Experiments with the Beet-root, or Mangold (Beta cicla alba), instituted for the purpose of making Sugar from it.

THE late professor *Maggraf** made experiments fifty years ago with several species of rapes, in order to ascertain whether it be possible to extract from them a sugar fit for use. Among several examined rapes and other species of roots he was particularly successful, according to his statement, in preparing an useful sugar and likewise a good syrup from the *Beta cicla*. From 2lb. of fresh roots he obtained $\frac{1}{2}$ lb. of dry matter, and this last yielded to him, by extraction with alcohol, one half ounce of good sugar.

Relying on that experiment, I had a *Scheffel*† of those rapes, namely, the *white mangold* measured and weighed. It held 130 rapes, weighing 68 pounds. They were then mashed and pressed; the residue from the press was infused in luke-warm water, and pressed again. Forty quarts of liquor were thus obtained; which was evaporated to a third part, then mixed with 20 quarts of fresh lime-water, and boiled for half an hour. The juice, which had now a yellow wine-colour, being refrigerated, filtered, and reduced by inspissation to a syrup like consistence, yielded six pounds of an agreeable, brownish-yellow, and transparent syrup. I poured one part of this syrup into a rather deep glass dish, in which a number of glass rods were fixed; and in that state I exposed the whole in a moderately warm place to spontaneous evaporation for the space of sixteen weeks; after which time I found the glass-sticks covered with crystals of sugar, of the size of from a lentil to a pea, and resembling yellow sugar-candy. Therefore, as the *Scheffel* of these mangolds sells for sixteen groschen ‡, and since the other expences in producing the syrup may be estimated at the highest at four groschen, the pound of such *mangold-syrup* will cost three and $\frac{1}{4}$ groschen, which sufficiently evinces its use as a substitute for the common syrup from sugar. In the same manner it is practicable, though by a rather slow process, to produce useful sugar from it, which at the

* *Experiences chimiques dans le dessein de tirer un véritable sucre de diverses plants, qui naissent dans nos contrées:—in the Memoires de l'academie des sciences, de Berlin, pour l'année 1747. Also in his Chemischen Schriften part ii. page 70—86. Berlin 1767.*

† The Berlin *Scheffel* holds 2604 french cubic inches, old style. The *English bushel* holds 1801, do. And 61,2 Berlin *scheffels* are equal to 88 $\frac{1}{2}$ English bushels.—Transl.

‡ Sixteen *Berlin Groschen* make about 2 shillings sterl. so that the pound of mangold syrup at 3 $\frac{1}{4}$ groschen will cost about five-pence halfpenny.—Transl.

present high price of that article, would be cheap enough; though dearer than sugar from the maple, with which last it agrees pretty well, as to its goodness or quality.

i. *Experiments made with the Runkelrübe* (*Beta vulgaris altissima*. *Beta cicla altissima*, according to Jacquin), *for the purpose of making Sugar.*

The runkelrübe is like the preceding, very common; so much so, that both species are employed in Thuringen, and in the environs of Brandenburg for feeding cattle. It differs from the foregoing in its very juicy state and sweet taste.

The runkel-rapes, which I made use of for my experiments, were cultivated at *Schoneberg*, near Berlin, in a moderately good soil on the estates of the privy-counsellor Mr. *Noldechen*. When they are closely examined, they offer to view a great variety of appearance. On the outside they are all of them covered with a red rind; but internally some are perfectly white, some are variegated with white and red circles, and some are distinguished from the others by light and deep-red rings. Others again are thick and knobby, while others are thin and long, like beet-roots. I am unable to determine whether this external difference depends on real varieties, or on the various quality of the soil in which they grow. Mine were all taken from the same kind of ground. In *Thuringen*, my native country, where those rapes, as far as my remembrance reaches, are cultivated as food for cattle, it is probably owing to the goodness of the soil that they appear larger and more abundant in juice, than here. Their size and richness in liquor are, besides, promoted there by hoeing and covering their protuberant part with earth, and also by not lopping off their leaves, but preserving the vegetation upon them till the time of reaping; though in another point of view it would furnish a good support to cattle. Such rapes as have been cropped or cut, are externally distinguishable from those that have not. The first are less smooth and have a larger crown, on which all the sproutings for new leaves are observable, the second are more even, and possess a smaller crown.*

One *Scheffel*, of these runkelrübes, held 112 pieces of various size, and weighed 125 pounds. After they were separated from the crown and outer rind, they were grated, and the pulp, from which a portion of juice spontaneously drained off, was strongly pressed. I thus obtained from this scheffel of rapes 24 quarts of a violet-coloured and very sweet liquor. I first reduced it without any addition, to a third part by boiling, which by experience I find to be the best method. During this process a

* These rapes are remarkable for their extremely great quantity of juice. I had 5 of them cut into thin slices without paring them, and suffered them to dry upon a chamber-stove; after which only 17 ounces of a dry mass remained, exhibiting a taste similar to that of liquorice; which attracted moisture from the air. Hence one pound of fresh Runkelrübe affords only two ounces dry matter.

vast quantity of albuminous matter was separated. After refrigeration, the liquor was run through a woollen cloth, then mixed with 24 quarts of lime-water, and boiled together with it for half an hour. All the juice now became a clear fluid of a yellow colour of wine, and there was still separated a large quantity of impurity in the form of scum. I suffered it again to cool, passed it through flannel, and when the whole was inspissated, it yielded me eight pounds of a well flavoured syrup.

In the same method as was used with the white rape, I exposed one part of this syrup to gentle exhalation, in a rather deep glass-bowl with glass-rods placed in it. After the lapse of eight weeks I obtained by this method a real brown-yellow sugar, crystallized around the sticks and resembling sugar-candy. But as in this syrup, as well as in the preceding, the crystallization goes on very slowly, I cannot well ascertain at present what quantity of dry sugar may be conveniently produced from it.—However, as this syrup is more agreeable to the taste than the common one, and because all the expences being deducted, one pound of it does not amount to above one groschen ($1\frac{1}{2}$ Engl.) on this double account it may, like that from the mangold, be used instead of common syrup.*

k, Experiments with the Beet-root (Beta rubra. Beta cicla rubra, according to Jacquin.) for extracting Sugar from it.

The possibility of making genuine sugar from the beet-root, has already been proved by *Marggraf*. (lococit.) On this account I subjected it to the same treatment as the former species. From one *scheffel* of beet-roots I obtained $6\frac{1}{4}$ lb. of syrup, of an harsh, disagreeable taste, and of much inferior goodness to those produced from the white mangold and the Runkelrübe. This unpleasant additional taste might probably be destroyed by suitable management. I have not yet attempted to extract sugar from it. *Marggraf* obtained from one pound of fresh beet-roots, 12 ounces of a dry mass, and 16 ounces of the dried roots, afforded him $2\frac{1}{2}$ drams of sugar.

* When at another time, I had heated a portion of the liquor from the Runkelrübe with milk of lime, (*lime diluted with water to the consistence of milk*) instead of lime-water, I added rather too much; in consequence of which, the whole fluid acquired a taste resembling that of acetite of lead. And by subsequent gentle evaporation there crystallized from it a large portion of malat of lime; from which circumstance it follows that, this rape contains much malic acid, which on one hand renders the crystallization of the sugar difficult, and on the other hand, possessing itself a sweetish taste, it increases the quantity of the syrup procured from the rape. To conclude, on continuing my experiments on this head, I find I succeeded best in separating the sugar, when I suffered the syrup to evaporate slowly at a temperature of 70° *Reaum.* stirring it round now and then, and once only at a time. A larger portion of grained sugar is then deposited at the bottom of the vessel; which cannot however, unless by repeated solutions and crystallizations be produced in the state of sugar, which shall continue dry. I have not yet tried, what may be the habitudes of this rape-sugar, when treated under a layer of clay.

l. Experiments

l. Experiments with Carrots, (Daucus carotta Lin).

Carrots when cut small and boiled with water, produce a sweet liquor, by the evaporation of which the country-people usually prepare a sweetish juice under the name of carrot-juice. They either eat it spread upon bread, or use it to sweeten their food. It is also sold by them to grocers, who mix it with the common sugar-syrup. But as the common juice from carrots is too much contaminated with mucilaginous parts to admit of being used as syrup, and has besides a disagreeable taste, I treated it in the following method.

One *scheffel* full of carrots was cleared of their external thin rinds, then rubbed small on a grater, and the juice expressed. This liquor was clarified by boiling with some whites of eggs, and reduced to the consistence of a syrup. In this manner I procured 6½ lb. of syrup, not unpleasantly tasted, but much inferior in quality to the syrups obtained from the mangold and runkelrübe. Nor did I find any means of obtaining a true sugar from it. Alcohol extracts from it a substance, very much similar to manna.

m. Experiments instituted with Turneps (Brassica rapa), for the purpose of making Sugar.

I caused twelve turneps to be peeled and separated from their crowns, and then to be reduced to a pulp by means of a grater. The pulpy mass exhibited a sweet, agreeable, yet slightly sharp taste, and yielded after proper expression a colourless, sweet juice. When clarified with a little white of eggs, the clear juice was strained through a woollen cloth, and thickened to a syrup. It was then of an agreeable taste, which, though inferior to the syrups produced from the mangold and runkelrübe, deserves to be ranked with the common syrup of sugar. By another experiment I found that one *scheffel* holds 125 of these rapes, and weighs 116 pounds; and that eight pounds of syrup may be obtained from it. Twelve weeks were elapsed when I found sugar crystals had shooted; which, however, were very brown, and could not be separated but with difficulty from the other remaining fluid.

n. Experiments with the Rape, or Cole-wort, (Brassica napobrassica).

The quantity of juice contained in this rape, as well as the agreeable taste of its juice, induced me to examine it for sugar and syrup. Sixty of them weighing together 123 pounds, and were freed from their external coat, and then finely grated and pressed. They yielded 22 quarts of colourless juice, which was agreeably sweet, but had an additional somewhat sharp taste like radishes. I suffered it to boil up twice, during which operation a great quantity of flocculent matter separated, and the liquor became as clear as water. After cooling it was filtered, diluted with 20 quarts of freshly pre-

pared lime-water and gently boiled down. The sharp radish-like flavor was driven off during the boiling, and I obtained at last ten pounds of a transparent brown-yellow syrup, in every respect nearly resembling the syrup obtained from the black birch (d,) and exhibiting the same foreign taste. It might be profitably used instead of the common brown syrup. For the purpose of producing sugar from this syrup, I exposed a portion of it to slow evaporation in a glass vessel, in which some sticks were placed; and after some weeks small saccharine crystals were formed, the quantity of which I was unable to ascertain. Yet, this rape being dearer than the white mangold and runkelrübe, and yielding a much less agreeable syrup, than even the turnep itself, it must, in this respect, always give way to those three species.

o. Experiments with the Skirret. (Sium Sifarum, Lin.)

Marggraf (loco citato) obtained from one pound of fresh skirrets $4\frac{1}{2}$ ounces of dried extract, and from $\frac{1}{2}$ lb. of dried roots he obtained three drams of genuine sugar. My experiments were this time only directed towards producing a syrup that might be fit for use; because the extraction of dry sugar, as was observed by *Marggraf* himself, is subject to many difficulties, on account of the many farinaceous parts these roots contain. As these roots, from their smallness, are not easy to be grated, I had one *scheffel* of them, weighing 26 pounds, bruised, with the addition of cold water in a stone mortar, and the pulp afterwards pressed. The residue was again moistened with cold water, and pressed a second time, by which management I obtained a turbid sweet juice. I left this standing for eight days in a cold place, during which a great portion of mealy substance fell down, and the liquor became clear. It was then clarified with a small portion of white of eggs, and afterwards inspissated; and thus it afforded five pounds of a pleasant, light-brown syrup. It is evident, therefore, that these roots are much too dear to be employed in making a cheap syrup.

p. Experiments with Parsneps.

(*Pastinaca sativa*, Lin.)

The particular sweetness of this root, which in other respects is very ligneous, induced me to subject it to similar experiments, though *Marggraf* had already observed, that only an inconsiderable quantity of sugar can be procured from it.

With this intention I treated one *scheffel* of the roots, weighing 24 pounds, exactly in the same manner as the skirrets. The syrup amounted to $5\frac{1}{2}$ pound weight, of an agreeable flavour, but did not thoroughly lose the particular taste of the parsnep.

If my other occupations had permitted, I should not have neglected to ascertain the relative proportions of real sugar contained in a certain determinate quantity of the substances subjected to examination. Such an attempt was at that time out of my power,

and

and I must defer the experiments I mean to make for this purpose to another opportunity.

Appendix to the Experiments for producing Sugar from indigenous materials. By SIGIS. FRED. HERMBSTADT.*

In the continuation of my experiments on the runkelrübes, I endeavoured to ascertain the quantity of real sugar, which is obtainable from a certain quantity of the rape. With this view I weighed accurately three pounds three ounces of the syrup obtained by my second experiment, which I poured into a conical vessel of tinned copper, and left it standing in a temperature of from 65 to 70° Reaum. for slow evaporation. After the expiration of eight hours a crust of grain or crystals of sugar was formed on the surface, which after 24 hours was nearly one third of an inch thick. This was pushed down, and sank in the liquid syrup to the bottom. Two days afterwards a new crust was formed, and again pushed down. I repeated this operation till a pellicle appeared on the surface of the remaining syrup, which was not crystalline, but merely tenacious. This pellicle indicated that all the crystallizable sugar was now separated from the fluid; which was also confirmed by the taste of the residual syrup, which was less sweet and more mucilaginous. At this period I transferred the whole of the crystallized sugar, together with the remaining fluid syrup into small sugar-loaf moulds, which I kept for the space of eight days in a temperature of 30° Reaum. During this time all the liquid syrup was drawn off, and the sugar remained nearly in a dry state. The whole operation lasted 36 days, and the raw sugar, thus obtained, was somewhat moist within, though it did not liquefy in the open vessel in which it was left during three weeks. Its weight was two pounds two ounces. The syrup, which spontaneously drained off, amounted to 12 ounces. Hence of the 3lb. and 3 ounces of syrup made use of, only 5 ounces of watery fluid evaporated. According to this experiment, the *scheffel* of these runkelrübes would afford about 5½lb. of brown raw sugar, and 1¾lb. of a syrup, not quite ill-tasted, and which if not used as syrup, may with great profit serve to make very good brandy by distillation. It does not signify whether those runkelrübes be cultivated by a particular method or no; for the sugar and syrup were obtained from them in the state in which they grew, near Schoenberg on a moderately good ground. At present I am occupied with the refining of my raw sugar, thus procured, in order to ascertain what quantities of the finer species of sugar, and how much syrup I may receive from it.

HERMBSTADT.

* Likewise from the Neu. Schr. d. Gesell. d. naturforsch. Freunde zu Berlin. page 450—52.

IX.

On the silent Escapement of Mr. Goodrich. By a CORRESPONDENT.

TO Mr. NICHOLSON.

SIR,

I WAS much pleased with the simple escapement of Mr. *Goodrich*, described in your last number, and in common with the respectable Society for the Encouragement of Arts, and yourself, and I doubt not the Inventor also, thought it as new as it is ingenious. But as I have since met with the same Invention, I send you the following notice, and suppose you will join me in opinion that Incidents respecting Improvements in the Arts have a claim for insertion in your miscellany.

In the Volume of *Machines et Inventions approuvées par l'Academie royale des Sciences*, tome viii. page 325, there is an account, with an engraving, of a Crank Escapement, invented in the year 1746, by *L'abbé Soumille*, and a Report explaining its qualities and advantages at length, by Messrs. *Hamel* and *Camus*.

I am, Sir, your obliged servant,

ONE OF THE PUBLIC.

London, 12th Nov. 1799.

X.

*Results of various Experiments for determining the Quantity of Action which Men can afford by their Daily Work, according to the different manners in which they employ their Strength. By Citizen COULOMB.**

TO give a clear and precise account of this interesting memoir, it is in the first place necessary to fix the meaning of the words *quantity of action*.

The effect which results from the mechanical labour of men can always be reduced to the ascending motion of an heavy body. The velocity with which such a body ascends, would soon be destroyed; if the cause that produced it were to discontinue its action; and it is necessary that the man should make a continual effort to keep it up. Here, therefore, are two quantities capable of numerical enunciation; the velocity which is the number of metres or unities of space uniformly passed over, during the

* Communicated to the National Institute of France, and abridged by *Prony*, in the Bulletin de la Soc. Philomath. No. 16. Messidor, Aug. 6.

unity of time, and the effort which has for its expression and measure a certain number of kilograms or unities of weight. The produce of these two numbers represents and measures the action; and this product multiplied by a third number, which is the time, during which the action continues, gives the quantity of action or total effect resulting the work, which is, therefore, an object capable of being measured and subjected to computation.

These notions being established, the fundamental object of research is the comparison of the work with the *fatigue*, which is the necessary consequence. Any determinate quantity of action (or the number which represents it) may be afforded by an infinity of different combinations of the values of numbers, of which the product serves as a measure; and these combinations depend on the various manners in which the force of men is employed. Is the fatigue equal in all cases for equal quantities of action, or does it vary when in different circumstances the numbers are varied which represent the velocity, the time, and the effort; so as, nevertheless, to preserve the same constant product? *Daniel Bernoulli*, and other celebrated authors have adopted the former opinion; but *Cit. Coulomb* shews that they were deceived, and by refuting, by proofs drawn from reasonings and experiment, an opinion, supported by names so respectable, he has rendered a great service to the scientific application of mechanics.

But though fatigue be not simply proportioned to the quantity of action, it is one of its functions; that is to say, the formula which represents it ought to include in some manner the velocity, the effort, and the time. It is known by the theory of mathematical analysis that there must, therefore, exist a certain relation between these three things, such as that a given effect may be produced with the least fatigue; or which is the same thing, that when the fatigue is the same the quantity of effect or total action may be a maximum. This is the problem which the author has proposed to solve, and in which he has considered the various methods of employing the force of men.

In the first place he examines the quantity of action which men can produce, when, during a day, they mount a set of steps or stairs, either with or without a burthen. The experiments he mentions on this subject immediately prove the falsity of the opinion of *Bernoulli*. He found that the quantity of action of a man who mounts without a burthen, having his own body to raise only, is double that of a man loaded with a weight of 68 kilogrammes ($149\frac{1}{2}$ lb. avoirdupois,) both continuing at work for a day. Hence it is strikingly observable, how much, with equal fatigue and time, the total or absolute effort may obtain different values by varying the combinations of effort and of velocity.

But the word effect here denotes the total quantity of labour employed to raise, not only the burthen, but the weight of the man himself; and what is of the greatest importance to consider, is, *the useful effect*; that is to say, the total effect, deducting the value

value which represents the transport of the weight of the body of the man. This total effect is the greatest possible when the man ascends without a burthen; but the *useful effect* is then nothing; It is also nothing if the man be loaded so much as to be scarcely capable of moving: and, consequently, there exists between these two limits a value of the load, such, that the useful effect is the greatest possible. Citizen *Coulomb* supposes that the loss of quantity of action is proportional to the load (which hypothesis is confirmed by experience) whence he obtains an equation, which treated according to the rules of maxima and minima, gives 53 kilograms (117lb, avoirdupois) for the weight with which the man ought to be loaded, in order to produce, during one day, by ascending stairs (un escalier) the greatest useful effect; and the quantity of action which results from this determination, which has for its value 56 kilograms, raised through one kilometer, does not sensibly differ from the results of experience. But this method of working is attended with a loss of three fourths of the total action of men, and consequently costs four times as much as work, in which, after having mounted a set of steps without any burthen, the man should suffer himself to fall by any means, so as to raise a weight nearly equal to that of his own body.

The author afterwards examines the work of men walking on an horizontal path, with or without a load. This method is here similar to the preceding, and affords similar results. The greatest quantity of action takes place when the men walk without being loaded; and is to that of men walking under a load of 58 kilograms, nearly as 7 to 4. The weight which a man ought to carry, in order to produce the greatest *useful effect*, namely, that effect in which the quantity of action relative to the carrying his own weight is deducted from the total effect is 50.4 kilogrammes (or 112 pounds, avoirdupois.)

There is a particular case which always obtains with respect to burthens carried in towns; namely, that in which the men, after having carried their load, return unloaded for a new burthen. The weight they should carry in this case, to produce the greatest effect, is 61.25 kilograms, or nearly 135 pounds avoirdupois. The quantity of useful action in this case, compared with that of a man who walks freely and without a load, is nearly as 1 to 5, that is to say, he employs to pure loss $\frac{4}{5}$ of his power.

The author afterwards successively treats of the cases in which men convey burthens on a wheel-barrow, or raise a weight for driving piles, or turn a handle. Under each article he gives the absolute and comparative results, comparing each kind of labour to the other methods of employing the forces of man. He finds that by causing him to mount a set of steps freely and without burthen, his quantity of action is at least double what he affords in all these other methods of employing his strength. The limits of this abstract will not permit us to follow him to a greater extent, for which reason we shall confine ourselves, in order to give a notion of his method to what we have

have said of man walking in an inclined or horizontal plane. His memoir is concluded by the consideration of the labour of men employed to cultivate the ground. He has found by experience, that the whole quantity of action thus afforded during a day amounts to 100 kilograms (220lb.) elevated to one kilometer.

Afterwards comparing this work with that of men employed to carry burthens up an ascent or steps, or at the pile engine, he finds a loss of about $\frac{1}{8}$ part only of the quantity of action which may be neglected in researches of this kind.

The author takes great care to warn readers against experiments of too short duration, and in several places speaks of the error to which we are exposed by making experiments with men of more than common strength. The mean results have also a relation to the climate. "I have caused," says the author, "extensive works to be executed by the troops at Martinico; where the thermometer is seldom lower than twenty degrees; I have executed works of the same kind by the troops in France; and I can affirm that under the fourteenth degree of latitude, where men are almost always covered with perspiration, they are not capable of performing half the work they could perform in our climate."

XI.

*New Researches into the Affinities which the Earths exert upon each other in the humid and in the dry way. By Citizen GUYTON.**

IT was not even suspected in the earlier times of chemistry that an elective attraction could be exerted between two pure earths. Their union was thought to be the consequence of a fortuitous aggregation, a simple adhesion referable neither to elective attraction nor the equilibrium of composition. But when it was observed that two earths infusible singly in our furnaces, such as flint and lime, were fused together with considerable facility into an homogeneous matter, of which the principles could no more be separated unless by chemical methods; it became necessary to admit of a mutual action which became active as a certain temperature, that is to say, a true chemical affinity.

The first step being made, it seems as if chemists would not have delayed to multiply their proofs by direct experiments; but it is nevertheless true, that very few facts are yet established in the dry way, and that it begins to be found necessary to interrogate nature in the humid way, of which the processes are so much the more important, as they more nearly approach the very methods used by nature to form these earthy compounds, which

* Extracted from a Memoir, read at the sitting of the National Institute of France on the 16 prairial last, (June 3, 1799,) inserted in the *Annales de Chimie* xxxi. 246.

consisting of the elements themselves in different proportions, exhibit such a variety of properties.

These considerations have determined me to present the results of my experiments in the humid way to the institution. I am persuaded that they will also conduce to a proper examination of the result of the dry processes under the true point of view.

§. 1. *Experiments in the Humid way.*

If two solutions, each formed of the same solvent and a different substance, be mixed there can be no change of equilibrium and decomposition, unless the two substances held in solution in the same fluid, exert upon each other an attraction more powerful than that which the solvent exerts upon them; and if these substances unite in the concrete form it will be an unequivocal proof, not only of an attraction capable of producing combination, but likewise an *elective attraction*, which destroys one compound in order to form another.

Such are the reasons according to which I have conducted my experiments. The results are as follow.

Experiment 1. I mixed ten centilitres of lime-water with two centilitres of barytic water, the specific gravity of the latter being 1,138. Clouds were speedily formed which fell to the bottom.

It might be expected that the lime-water might contain sulphuric acid which is so often contained in waters and in lime-stones mixed with gypsum. That which I used was previously effayed by the muriate of barytes which produced no change.

Experiment 2. I dissolved pure alumine in a solution of pot-ash. On the other hand I prepared the solution of filix in the same salt by the usual methods. Here were two earths separately combined with the same solvent. The two fluids after filtration were very clear and moderately concentrated; the former had a scarcely perceptible yellow tinge; the latter was faintly green.

I mixed equal parts of each. As soon as the fluids came into contact, a brownish zone was formed, which by agitation diffused itself through the mass, and gave it a fawn colour.

The mixture did not appear to undergo any change for near an hour, though it was agitated with a spatula of glass, but at the end of that time the whole mass was whitish, opaque, and of the consistence of a thick jelly.

This experiment as well as the foregoing was repeated in the chemical course of the second division of the polytechnic school on the 29th ventose of the last year, and presented absolutely the same appearances.

It is not possible to obtain a more direct proof that earths, of the number of those in which there is the least reason to suspect alkaline properties, are capable even in the
humid

humid way and in pot-ash, to exercise upon each other an elective attraction, which exceeds that which unites either of them to the alkaline solvent.

Experiment 3. The action of lime on flint in the humid way, is demonstrated by the precipitation which lime-water effects in the solution of flint in pot-ash. The precipitate is a true combination of flint and lime as M. *Gadolin* has announced from having seen the experiment performed by me three years ago in my course * at the Polytechnic School.

An earlier observation had before pointed out the truth to us, namely, that glasses, into the composition of which too great a quantity of lime had been admitted, and which are in consequence soluble in acids, do not afford in these solutions either a speedy, or a ready separation of the flint from the lime; but, on the contrary form crystalline depositions, which must be treated again in order to have the complete analysis.

I was desirous of ascertaining whether the same phenomenon would take place with barytes. I poured barytic water into a solution of flint in pot-ash; the mixture soon became turbid. The precipitate dried on the filtre was digested in acetous acid, which dissolved 0. 3.—It is to be observed, that the precipitate afforded by lime-water poured into the solution of flint in pot-ash when treated in the same manner with acetous acid, lost no more than 0. 1. of its weight.

Experiment 4. The action of strontian upon flint was tried in the same manner, and a decomposition also took place. But of 110 parts of this precipitate dried on the sand bath, the muriatic acid redissolved 45 in a digesting heat.

Experiment 6. The aqueous solution of strontian was mixed with that of barytes; no signs of new combination were observed.

Experiment 7. I have long since shewn that the carbonate of barytes like that of lime is soluble in excess of its own acid; and the same property has been proved to exist in the carbonate of strontian. This, therefore, afforded a new means of putting the affinities of the earths into action. The mixtures varied with this intention afforded no phenomenon, which indicated an attraction of sufficient power, either to separate them from the carbonic acid, or to unite them in the state of carbonate.

Experiment 8. These results which even by their variety, appear to characterize more particularly that unequal power of union which we call elective attraction, or affinity, induced me to make experiments on the solutions of earths in more powerful acids. They afforded me unequivocal proofs not only that there exists among the earths a tendency to unite in the humid way; but also that with regard to some of these earths, the union is such as to be capable of resisting an addition of the acid by excess.

* *Annales de Chimie* xxii. 109 and xxvii. 320.

These observations are too intimately related to the formation of stones, and the art of analysing them to admit of the principal results being neglected in this place. They will, no doubt, be thought of equal importance when they announce that the earthy salts were not decomposed, as when they shew the possibility of that effect being accomplished.

I made a mixture of equal parts of a solution of muriate of lime and of muriate of alumine. The fluid became turbid almost immediately, even before it was agitated. This effect which was first seen in the upper part, was very rapidly communicated to the lower; after which the fluid became opaque and almost gelatinous, strongly colouring blue paper, and the precipitate did not disappear but by the addition of new acid.

Experiment 9. The mixture of muriate of lime and muriate of magnesia, gave no signs of new combination.

Experiment 10. The solution of muriate of barytes, afforded at the end of three or four minutes, an abundant precipitate, which was not redissolved by excess of acid.

Experiment 11. The mixture of the solutions of muriate of lime and muriate of strontian, exhibited no change.

Experiment 12. The mixture of the solutions of muriate of magnesia and muriate of alumine, assumed at the end of a few minutes, a milky appearance; but the effect proceeded no farther, whether the solution were neutral, or with excess of acid.

Experiment 13. Muriate of magnesia and muriate of barytes, mixed in solution, afforded an abundant precipitate, which was not redissolved by the addition of more acid.

Experiment 14. Nothing of this kind was observed by mixture of the solutions of muriate of magnesia and muriate of strontian, in any proportion.

Experiment 15. If the solutions, even diluted, of muriate of barytes and muriate of alumine be mixed, the fluid becomes immediately turbid, and affords a copious precipitate.

(To be continued.)

XII.

Account of the improvements made on the Farm in the great Park of his Majesty, the King, at Windsor. By NATHANIEL KENT, Esq.*

SIR,

UPON mentioning to you some time since that there had been some practices in husbandry on his Majesty's farms under my superintendance in Windsor Great Park, which I conceived were not generally known, and upon your giving me reason to

* Addressed to the Secretary of the Society for the Encouragement of Arts, and inserted in their Transactions for the present year.

think the society for the Encouragement of Arts, &c. from its laudable desire to communicate to the public every thing that promises advantage to it, would not be unwilling to allow me a few pages in its next publication; and being indulged with his Majesty's gracious permission to state any matter that I may discretionally think proper to communicate, I am induced to lay before you a few particulars which some Gentlemen and farmers, under similar circumstances, may, perhaps, think deserving notice.

But before I enter upon any particular description of what I have to offer, it will not, perhaps, be uninteresting to the Society to know the grounds upon which his Majesty's large system of agriculture has been founded.

In the year 1791, the Great Park at Windsor, about 4000 acres, fell into his Majesty's possession. It might truly be called a rough jewel. The whole, as a natural object, was grand and beautiful, of a forest appearance; but the parts were crowded and indistinct. The soil was various, some parts clay and loam, and some sharp gravel or poor sand; a great part of the former was covered with rushes and mole-hills, and the latter with fern and moss.

About 1000 acres of the lightest part were separated from the rest at one extremity, and formed what is called the Norfolk farm; about 400 acres more at the other extremity, of a good loamy soil, were separated and called the Flemish farm; both being named from the nature of the husbandry meant to be adopted on them.

The rest (about 2,400 acres) remains still in plantations and park, and though so much reduced, yet from the improvements which have been made upon it, is now capable of carrying more stock than the whole 4000 acres did before. All the unfound wet parts have been drained by the Essex mode, so as to be rendered firm and productive of an improved herbage. The mole-hills have been levelled chiefly by dragging, and the coarse and mossy parts fined by repeated harrowing and rolling (being one of the first improvements upon park land of this description) besides which a variety of beauty has been laid open by clearing the valleys and low parts, to give a bolder effect to the woody scenes upon the higher grounds; and by making judicious openings so as to break strait lines and separate parts that were in some places too heavy and samely; so that the same extent of land has not now only a much larger appearance, but exhibits a much greater variety of ground. The truth of this every impartial person who knew the place before his Majesty caused these improvements to be made, must allow. I have only to add, that tho' prejudice may have taken up an idea that there has been too great a sacrifice of timber in effecting these improvements, truth will deny it. There has not been a tree taken down but what was either in decay or removed either to give room for the growth of others, or to set them off to greater advantage in picturesque appearance.

I come now to the object in view as before hinted, which is to state the motives which I am inclined to think induced his Majesty to adopt the farming system upon so large a

scale, and next to shew the result. These I conceive were chiefly to create useful labour for the industrious poor in the neighbourhood, and for trying experiments in agriculture to excite imitation where success might encourage it.

The Norfolk farm borders on that extensive waste called Bagshot Heath, hitherto considered too barren for cultivation, though large tracts of a similar quantity have been long since rendered useful to the community in the South-west part of Norfolk. Arable land of this description is generally managed there under a five course shift; first wheat; second turnips; third barley with seeds, which continue laid two years. But as the seeds turn to very little account after the first year, his Majesty's, which though a five course shift likewise, of one hundred acres in a shift, is upon a much improved course; as thus—first, wheat or rye; second, the irregular shift; third, turneps; fourth, barley or oats; fifth, clover.—The irregular shift, which is of great use on a light land farm, may, perhaps, want a little explanation. It is meant to be partly productive and partly preparative. Forty acres of it are sown with vetches to be fed off; forty-one sown the latter end of August with rye, for early feed the next spring for the ewes and lambs; the remaining twenty acres are planted with potatoes, and the whole comes round for turneps the next year.

From the advantage of running sheep in the park this farm has been brought surprisingly forward, considering the short time it has been cultivated, and a great part of it which produced nothing but heath and moss, and would have been dear at five shillings an acre to rent, now produces crops worth more than the original fee simple of the land.

Brevity checks me from going farther into a general description; but the following particulars may deserve notice.

The comparative advantages of the labour of horses and oxen have been for some time under the consideration of the public. His Majesty has unquestionably tried the latter upon a larger scale than any other person, as he does not work less than one hundred and eighty oxen upon his different farms, parks, and gardens, and has found them to answer so well that there is not now a horse kept. Upon the two farms and the great park two hundred are kept, including those coming on and going off. Forty are bought in every year, rising three years, and are kept as succession oxen in the park; one hundred and twenty are under work, and forty every year are fatted off, rising seven years.

The working oxen are mostly divided into teams of six, and one of the number is every day rested, so that no ox works more than five days out of the seven. This day of ease in every week, besides Sunday, is of great advantage to the animal, as he is found to do better with ordinary keep and moderate labour than he would do with high and harder labour. In short, this is the first secret to learn concerning him; for an ox will

not admit of being kept in condition like a horse, artificially, by proportionate food to proportionate labour.

These oxen are never allowed any corn, as it would prevent their fattening so kindly afterwards. Their food in summer is only a few vetches by way of bait, and the run of coarse meadows, or what is called leafows, being rough, woody pastures. In winter they have nothing but cut food, consisting of two thirds hay and one third wheat straw; and the quantity they use in 24 hours is about 24 pounds of hay and 12 of straw; and on the days of rest they range as they like in the straw-yards; for it is to be observed, that they are not confined to hot stables, but have open sheds, under which they eat their cut provender, and are generally left to their choice to go in and out. Under this management, as four oxen generally plough an acre a day, and do other work in proportion there can be no doubt but their advantage is very great over horses, and the result to the public highly beneficial.

The oxen which are brought on in succession, run the first summer in the park, and in the leafows and temporary straw yards in the winter, by which temporary straw yards I would have it understood that they are made in different places, so that the manure which they make may be as near the spot where it is wanted as possible.

The forty oxen which go off are summered in the best pasture and finished with turneps the ensuing winter. The usual way has been to draw the turneps, and to give them either stalled or in cribs placed in the yard, with plenty of straw to browse and lie upon; but last winter an experiment was tried which answered extremely well, and will be again repeated next winter; this was penning the oxen by day on the turnep land in the manner which sheep are penned, with this only difference, that the turneps were thrown up into cribs instead of being left to be trodden into the ground; and in the nights they were driven into a yard with a temporary shed, well littered with rushes, fern, and leaves, and turneps, and barley-straw given to them in cribs. They thrived very fast, and every one of them made at least eight loads of good muck in the night-yard, besides the benefit done in treading and dunging on the land in the day time, which was very great, the soil being very light.—The result of the ox system is, that charging the ox for his agistment the first year, for the value of the grafs and turneps the last year, and putting what he has in three intermediate years as an equivalent for his labour after every allowance for risk, each ox will pay at least 20 per cent. profit. In what instance does a horse produce so much?

I do not allow that the ox can be used on all soils; upon a very stony soil he cannot; nor can the horse in all places be wholly excluded from husbandry; but every occupier of a large farm may at least use some oxen to very great advantage. They are all worked at Windsor in collars, as their step is found to be much more free than when coupled together with yokes; and they are found to do their work with much greater ease in collars than in yokes, which ought every where to be exploded.

The

The different kinds of oxen are in some measure suited to the soil.—Upon the Norfolk farm, which is a light soil, the Devonshire sort are used; upon the Flemish farm, where the soil is strong and heavy, the Hertfordshire; and in the Park, where the business is carting, harrowing, and rolling, the Glamorganshire.—They are all excellent in their different stations.

It may not be improper to mention a very simple method which has been discovered of first training them to the collar, which is nothing more than putting a broad strap round their necks, and fastening one end of a cord to it, and the other to a large log of wood, and letting the ox draw it about as he feeds in his pasture for three or four days before he is put into harness, by which means he is very much brought forward in docility.

I have before observed that 20 per cent may be considered as the average profit of an ox, stating them to be bought in at 10*l.* and allowing them to sell for 25*l.* taking off 10*l.* for the two years they are not worked: but last year, beans being of very little value, they were kept longer than usual by being stall-fed with bean meal, which answered very well as they were brought to an average of nearly 30*l.* and one of them, a Glamorganshire ox, originally bought for 8*l.* and from his compact, round make, always called the little ox, thrived to such a surprising degree that he became too fat to be able to travel to Smithfield, and was therefore sold to Mr. Charlwood, a neighbouring butcher, for 47*l.*

(To be continued.)

SCIENTIFIC NEWS AND ACCOUNTS OF BOOKS.

ARTIFICIAL PYRITES.

Extract of a Letter from a Correspondent.

IF you think it worth while you may mention in your Journal an experiment I lately made, of endeavouring to procure Pyrites in a moist way. It is about two years since I first made the experiment, and only repeated it lately, to convince some of my friends, who expressed their doubts. The experiment is this.

I impregnated water very strongly with carbonic acid, and introducing some iron filings, I continued the impregnation for a day or two, and afterwards allowed the water to stand in a well corked bottle for some days, till the acid had taken up as much iron as possible. I then poured it into an aerating apparatus—threw up the hepatic gas from sulphuret of potash and sulphuric acid; and after having agitated the water till it had got a good dose of the gas, I poured the water into a large basin;—this was in the evening, and next morning when I looked at it I found it covered with a pretty thick film of a most beautiful variegated pyrites.—I had so little of it, that the only proof I had of its being this substance was, that it was ignited on its being placed on a hot poker.

This

This may, perhaps, explain the beautiful variegations we observe on some mineral springs and stagnant pools.

Wooden Bridge across the Rhine, at Schaffhausen.

Mr. Taylor, in Holborn, has published a large print of this curious product of the mechanical ability of *Grubenman*, the architect. The print contains plans of the floor and the roof, with an elevation and section of the structure, some other detailed parts and a perspective view in aqua-tinta. Instead of attempting to describe, in mere words, this combination of timber work, which formed two extended low arches, spanning in the whole 364 feet, I shall, for the entertainment of the reader, give a short historical extract from the pamphlet which accompanies the engraving, first premising that this useful monument of original genius fell a sacrifice to the fury of war, in April 1799, when it was burned by the French, after having stood upwards of forty years.

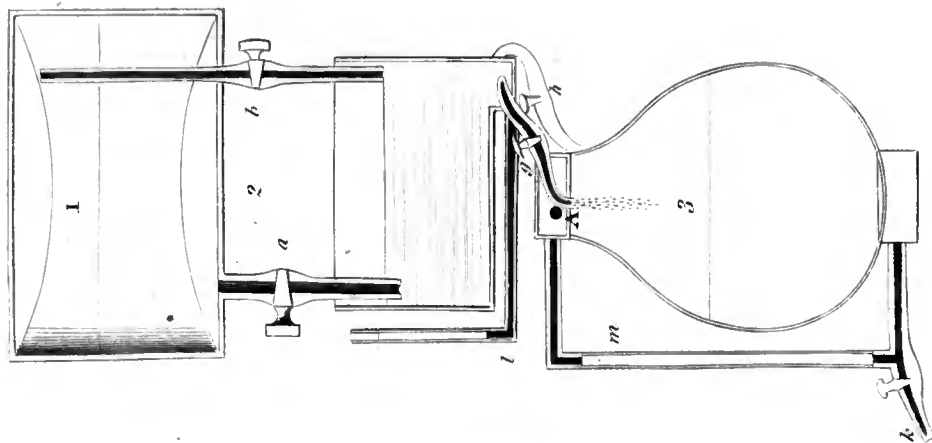
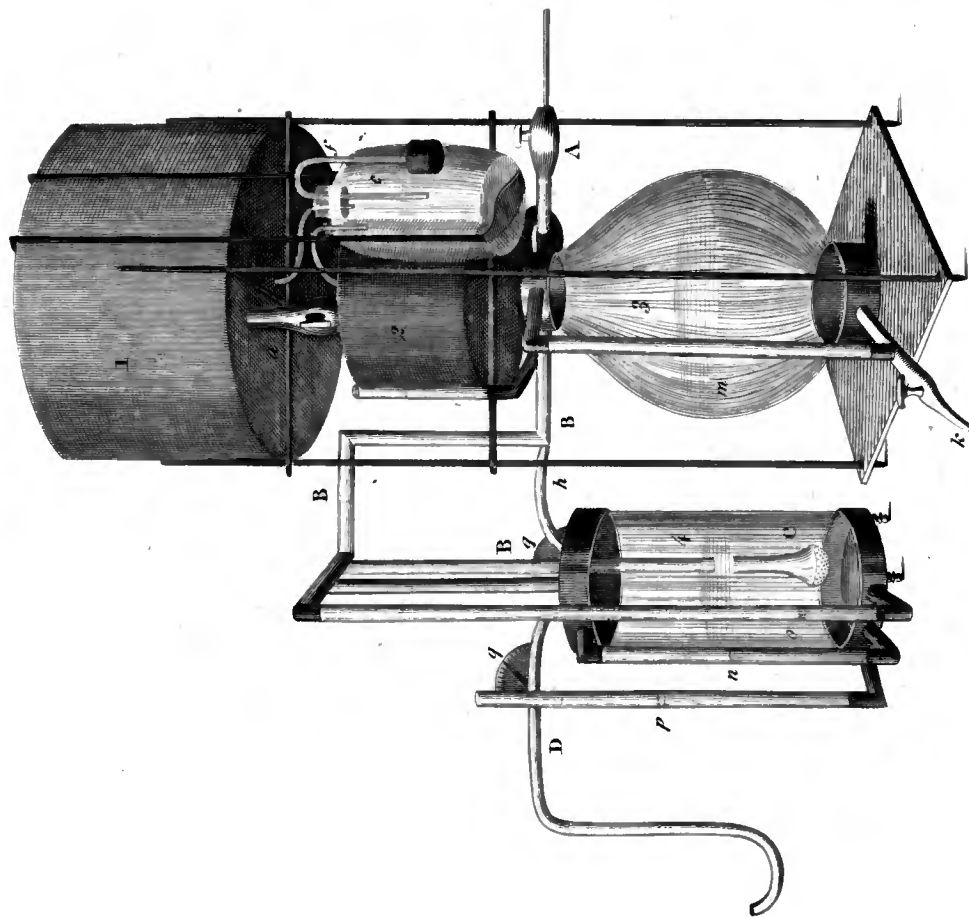
“The village of Tüfßen boasts the honor of having given birth to an excellent mechanical genius. The wooden bridges of *Ulric Grubenman* are very generally known on the Continent; that which is thrown across the Rhine, near Schaffhausen, is a fabric contemplated with astonishment by every traveller; and it is extolled in all modern works which treat of Switzerland as one of the first of the curiosities which deserve to be visited in that country. Indeed the boldness and beautiful simplicity, as well as the apparent symmetry and intrinsic strength of the wooden bridges constructed by *Grubenman* cannot be sufficiently admired. Consisting of one arch they stretch and bend as if supported by huge cables; they rock and tremble even under the feet of the passenger; and when loaded waggons pass over them, the shaking of the bridge increases to such an alarming violence that those who are unacquainted with the principle of its construction dread every moment that it will give way and plunge them in the waves.

“This sort of bridge aptly styled *hanging work* was first brought to perfection by *Ulric Grubenman*. All the wooden and stone bridges which had been laid across the Rhine, near to and at the expence of the City of Schaffhausen, being washed away by the impetuosity of that river, it became necessary in the year 1754, to erect a new one; when among the architects who delivered in their plans, *Grubenman*, then a common carpenter, of Tüfßen, presented himself with a proposal for building a bridge, which resting on no pillars in the bed of the Rhine, should be supported only by the rivers opposite banks. On producing his model for the first time to the committee appointed to examine the plans that might be offered, he was asked with a scornful smile, whether he really thought that a bridge built on the proposed principles would not break down as soon as any considerable burdens were brought in contact with it? Instead of making any answer, he, with both his feet, stepped on his little model, which bore him (though a tall and stout man) exceeding well. Impressed with this circumstance, the committee

committee more attentively considered the model of his bridge. It was completed at the close of the year 1758, and stood without receiving any injury till 1789, when a few decayed beams were replaced by new wood; since which trifling repair the bridge is as sound as ever. The same artist offered to build a similar one-arched bridge across the river Derry, in Ireland, which is 600 feet wide; but this plan was rejected."

A new System of Mineralogy, in the form of a Catalogue, after the manner of Baron Born's Systematic Catalogue of the Collection of Fossils of Mademoiselle Eleonore de Raab. By William Babington, M. D. Assistant Physician and Lecturer in Chemistry, at Guy's Hospital. Quarto, 279 pages, and Index, price 15s. in boards. Sold by Phillips and Robinsons.

My Suffrage will be unnecessary either to add to the reputation of Dr. Babington, or to support the truth that the public must be ever most essentially benefited; when men of actual research and observation compose elementary treatises, which are more commonly fabricated by mere compilation. The work refers to the Collection of Sir John St. Aubyn, Bart. to whom it is dedicated, and the author has availed himself of the best authorities for those experimental results, of which the life of no single individual is sufficient for the investigation or even the repetition. His obligations to Baron Born are expressed in the title page. Rome de Lisse has been his guide on the subject of crystallization; most of his descriptions correspond with those of Widenmann and Emmerling, and he has chiefly recurred to Kirwan for objects of chemical properties and analysis. His classes, orders, genera, and species, are founded on the chemical and his varieties on the external characters of minerals.





Method of grinding Spirit Levels.

Philos. Journal Vol. III Pl. XVIII facing p. 428

Fig. 1.

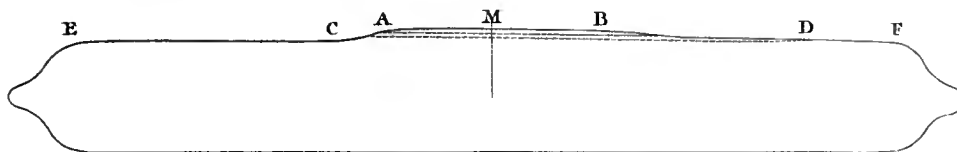


Fig. 2.

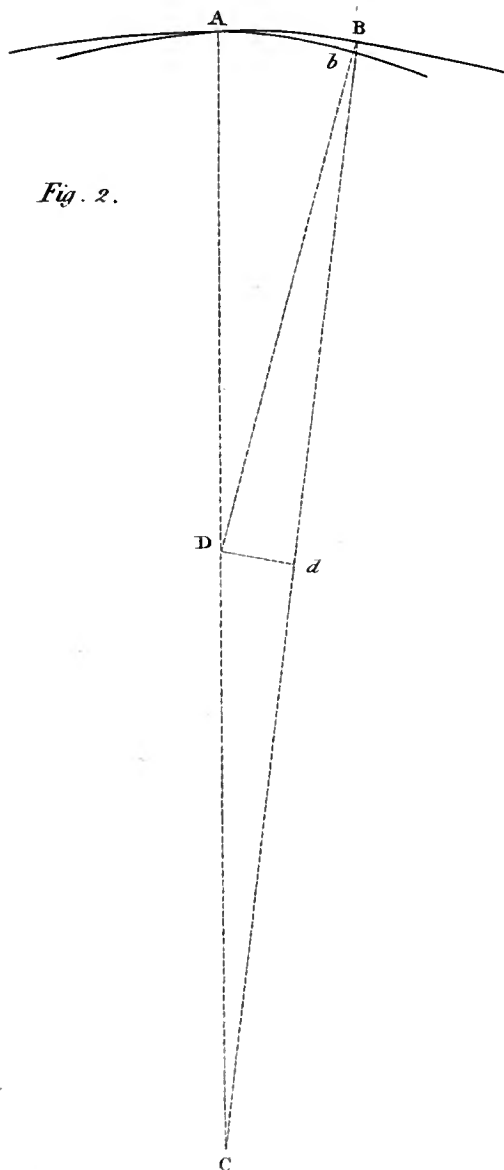


Fig. 3.

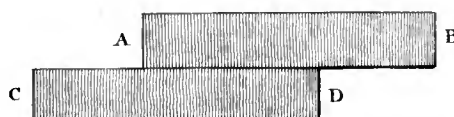


Fig. 4.

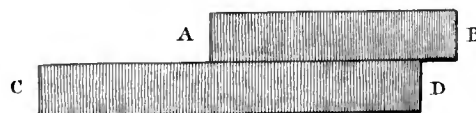
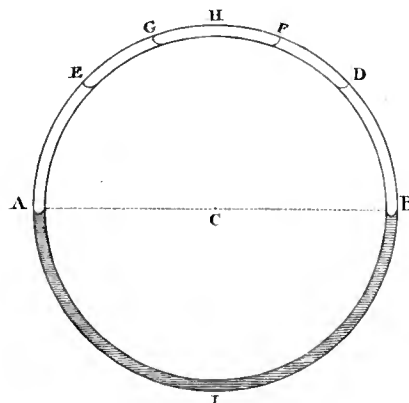


Fig. 5.





A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JANUARY 1800.

ARTICLE I.

Account of the improvements made on the Farm in the great Park of his Majesty, the King, at Windsor. By NATHANIEL KENT, Esq.

(Concluded from our last.)

NEXT to the advantage obtained from oxen, as much benefit as possible has been endeavoured to be derived from sheep by means of the fold. Two ewe flocks are kept, of four hundred each; the soil being light and dry, admits of winter folding (except when the weather is wet) upon the young clover;—a practice much to be recommended, as it produces a great crop of clover, and prepares the land the ensuing autumn for a crop of wheat without any further assistance. Another excellent practice is folding upon light land, in dry weather, immediately upon the sowing of the wheat, or kept back a fortnight or three weeks on that account; and it is not amiss to have the fold rather large, and to give the sheep a turn or two round the fold in a morning before they are let out to tread and settle the land, which does a great deal of good over and above their dung.

A third method of folding has been found to answer almost beyond description. This was first tried in the winter of 1793; but from an idea of the shepherd, that it injured the sheep, has been since disused: but as there is good reason to believe that there was no just ground for such an opinion, it is meant to be revived next winter.

A dry sheltered spot is selected, and sods of maiden earth, a foot deep, are laid over the space of a very large fold. It is then bedded thinly with rushes, leaves of trees, fern, moss, short straw, or stubble; and in hard or wet weather, the flock, instead of being penned upon the clover in the open fields, is put into the warmer fold, where the usual quantity of hay is given to them in racks; and every night they are so penned, the fold is fresh littered. When this has been continued, at intervals, during the winter, a layer of lime, chalk, rubble, or ashes, six inches thick, is spread over the whole surface—and when it has heated together, about the month of April, the whole is turned up, and mixed together, and makes the very best manure that can be used for turneps.

I have been particular in describing these methods of folding, as they are not common in any place, and in others entirely unknown, and to Gentlemen who have parks and large plantations which afford abundance of leaves, this hint may be the more deserving attention.

Upon the Norfolk farm, the land not having been yet marled or clayed, the clover is apt sometimes to fail, which is also the case elsewhere, upon the same sort of land. When this happens, his Majesty does what every other person in a similar situation should do; instead of letting the ground remain unproductive, the next year it is sowed with vetches, which are nearly as valuable as the clover, and wheat always grows remarkably kind after them.

As to implements, the Norfolk plough is chiefly what is used; and upon a light soil, it is certainly preferable to any other. It ploughs a cleaner furrow, by completely moving the whole body of earth, and inverts it much better than any other plough; and to establish its superiority over the common ploughs of the neighbourhood, I need only add, that from its construction it is nearly the draught of an ox easier. There is likewise a Norfolk harrow, very useful for harrowing what are called brush-turneps, or any other turneps, preparatory to their being hoed. I must be allowed, likewise, to mention the drill-roller which consists of cast-iron rings, made at the Norwich foundery, and slipt on upon a round piece of wood, as an axle-tree. This is one of the best things that has been introduced, for the preparation of the land for any sort of corn, where the soil will admit of its being used. By the corn being so well deposited, it takes better root, and at least one fourth of the quantity usually sown may be saved.

The Flemish farm, which I have before mentioned, was so named from an intention, at first, of carrying on a system of husbandry similar to that practised in Flanders, which consists of an alternate crop for man and beast; but the soil being strong and cohesive, upon trial, it has been found to answer best under a four-course shift, more like some parts of Gloucestershire; as thus—first year, wheat; second, cabbage or clover; third, oats; fourth, beans. The quantity of arable land of this farm is one and sixty acres, or forty acres in a shift. There are two things observed upon this farm, which

may be worth notice:—The first is the practice which has for these two years past been adopted, by taking off the tops of the beans just as the blossom is set; this not only improves the quality, but increases the quantity, and causes them to ripen sooner, which is a considerable advantage, by giving time to get the succeeding crop of wheat in, perhaps, a fortnight earlier. The other is, that of sowing clover early in the spring, among twenty acres or one half of the wheat, and bush-harrowing and rolling it in. This has produced a very fair crop of clover the next year; and the other half, after the wheat, is winter and spring fallowed, and planted with cabbage. There is a double advantage resulting from this; that one half of this shift, so managed, becomes a summer crop, and the other half a winter crop; and by observing the next year to change the parts, by sowing the clover where the cabbage was before, the clover and cabbage do not come round upon the same ground but once in eight years.

Cabbage has been tried several years, but his Majesty's husbandmen never got into the right management of it till this year; but now the crop is remarkably fine.

It will not be improper to mention, that the drum-headed cabbage is the best sort; that the seed should be sown in August, the plants first set out in November, and transplanted for good in July. The next thing to be noted is their application:—They are certainly inferior to turneps for fattening, but superior in the increase of milk, either of cows or ewes, and therefore they are particularly good where there is a dairy or breeding flock of sheep: and I trust his Majesty will, the next yearning season, try an experiment, of which I have high expectation, which is to slice or quarter the cabbage, and feed the ewes with them upon such of the meadows as want manuring, which I flatter myself will be of inestimable service to the ewes and lambs, and be the means of increasing the next year's crop of hay considerably.

The true light of viewing these improvements is to consider them as a sort of new creation to the public; for, as it is a fact not to be controverted, that the reduced number of acres in the park, from their improved state, support as many deer and other cattle as the whole did before, the produce obtained from the farms is all clear gain; and as crops of wheat and rye from the 140 acres sown, upon the most moderate calculation, may be set at 3,360 bushels, and allowing six bushels to a human mouth, this gives a yearly provision in bread for 560 people; to say nothing of the fattening-off of forty oxen, the bread of 800 sheep, and the growth of at least 5000 bushels of oats and beans; all of which, it must be observed, goes in aid of the public market, as the work is done by oxen entirely.

As more experiments are in future made, I may perhaps trouble the Society with an account of them, as I am persuaded they cannot be registered any where else, to give them the credit, and to excite the imitation I flatter myself they may deserve: but for the present, I shall close my observations upon his Majesty's farms, with a description of

his mill, which I consider as the most benevolent thing that can be done for the poor, and which I most earnestly recommend to all Gentlemen of landed property, who have like means of doing it. A small over-shot mill is erected, and worked by the waste water from the lake below the Lodge, where a sufficiency of corn, two thirds wheat, and one third rye, is ground, dressed, and given to all the labourers, at sixteen-pence per stone of fourteen pounds, in quantities suitable to the size of their families, which is the first of all comforts to them, and a saving of at least twenty per cent. from what it would cost them to buy it from the mealmen or shopkeepers.

I am, SIR,

Your obedient humble servant,

NATHANIEL KENT.

Craig's Court,

30th October, 1798.

II.

Extract of a Letter from WILLIAM PETRIE, Esq. on Board the Good-Hope, Indian-man, at Sea, lat. 35. 40. S. long. 44. E. giving an Account of a singular Accident by Lightning. Communicated by ROB. PETRIE, M. D.

SATURDAY, the 13th July, about midnight. a flash of lightning of a globular form, came in contact with the fore part of the ship, and instantly exploding, occasioned a report, compared by those who heard it, to that of a cannon hard charged with double-headed shot; differing much from the sound of ordinary thunder, which had been frequent in the course of the evening. This might have been owing to the nearness of the explosion, the sound neither vibrating much nor meeting with any body to revibrate it, before reaching the ear, as happens when such explosions take place high up in the atmosphere.

At the moment of explosion several people on the main-deck felt sensibly shocked in different parts of the body, and William Stanhope, a soldier in the 29th L. Dragoons, was instantly killed. Thomas Steelman, a sailor, sitting wrapped up in a sail and leaning to the foremast, was found in a state of insensibility, and seemingly lifeless. He was immediately carried below, and as the body passed me, I was sensibly affected with the smell of burnt horn or singed wool, which, however, was compared by others to the smell of burning sulphur. From the time of his being struck until symptoms of returning life made their appearance, at least six or eight minutes must have elapsed. Being at that moment engaged in fruitless attempts to recover the soldier, I had not an opportunity of ascertaining whether the motion of the heart and arteries was entirely interrupted,

nor

nor was this circumstance attended to by the surgeon, but from the general appearances it is extremely probable that both the circulation and respiration were completely suspended. In recovering from this state he gave a loud scream, accompanied with considerable agitation of the body, but no convulsive-like motion. His eyes had a very wild appearance, and the whole countenance was remarkably expressive of terror. He foamed at the mouth and made frequent attempts to articulate, but was unable. He seemed quite insensible to what was passing, and continued screeching at short intervals for nearly two hours. Upon examining the body, the whole inside of both thighs was found stripped of the cuticle, as if the part had been blistered and the false-skin removed, and a few fingers of each hand were in the same manner affected. In the course of the night his intellects returned, and suffering little pain from the superficial burning, he passed the next and following day with little inconvenience, and in seeming good health, but could not call to mind any of the circumstances that had taken place. On the 16th, he complained to the assistant surgeon of a slight sense of soreness in the skin of the right side of the head, with a want of feeling in one particular spot. Upon examining the part no sore could be discovered, but a portion of the skin, about the size of a dollar, over the right parietal bone felt depressed, as if beaten in, and quite insensible to the touch. Some spiritous application was made use of, and next day the part appearing of a darker colour than natural, the hair was removed. The same application was continued, but on the 18th, the spot had become quite black, felt soft, and appeared in a state of gangrene. On the 19th, the gangrene seeming to extend, the surgeon made a few scarifications round the edges, which bringing on inflammation put a stop to its further progress. In two days the mortified parts sloughed off, and left a portion of the scull fully the size of a dollar, including a part of the right parietal and frontal bones with the coronal suture running through its middle, entirely bare and denuded of the pericranium. The wound has continued to discharge freely of purulent matter, and the edges granulate favourably. The bone, however, has been gradually becoming of a darker colour, and is now (the 7th of Aug.) entirely black in one part. The discolouration commenced at the suture, and will probably extend as far as the mortification in the skin; so that before a cure can be accomplished an extensive exfoliation must take place. It will be fortunate if the exfoliation does not penetrate deeper than the outer table of the scull, as his life will be in imminent danger should so large a surface of brain be exposed from such a cause.

What appears singular in the case of this man, is the spot on the head running to mortification without any evident exciting cause, and without any previous inflammation. It could not have arisen from either a blow or fall, as he was found exactly in the position which he had, previous to the lightning. It was no doubt an effect of the lightning, but how it should have confined its influence to so small an extent, and act

in a manner so differently from what it had done in other parts of the body, are circumstances for which it is difficult to account. The extensive excoriation on the thighs and hands was evidently burning, produced by the electric matter passing over those parts, but no such appearance could be traced on the head. The canvas cap he had on was found entire, neither was the hair so much as singed; so that the part could not be said to be burnt. Indeed the whole appearances are against such a conclusion, for had this been the case, some degree of inflammation would have been observable previous to gangrene. The only attempt towards an explanation that I have to offer on the subject, is to refer it to a known law of the effects produced by very powerful stimulants on the living animal body: which when applied in too great quantity exhaust it instantly of its vital or irritable principle. It is in this manner that electricity or lightning acts when death is produced; and in a like manner it is probable that the most powerful poisons, such as the Ticunas and Upaz gum produce their effects. So that what happened in the present case may be considered as *death* in one small spot, occasioned by the immediate and total extinction of the irritable principle, from an over *dose* of electric matter, (the most powerful and most diffusible of all stimuli,) and necessarily running to mortification as a *piece* of dead animal matter. Its partial action is the difficulty for which I pretend not to account.

The soldier had exactly the appearance of people described to have been killed by lightning. The body retained its heat a very considerable time after death, the muscles never became rigid, and appearances of putrescency very soon presented themselves.

The local situation of those two men at the time the lightning came in contact with the ship, necessarily exposed them more than others. The soldier was leaning against the water engine, which has a good deal of iron in its construction, and being sentry had a bayonet attached to him. The sailor, as already mentioned, was leaning against the foremast, which being wet by the previous rain was just in a state ready to conduct electric matter, and consequently rendered him more obnoxious to its influence than had he been detached at some distance.

Five or six pigs, in a sty, near the head of the ship, were killed, while others in the same situation, but separated by canvas, entirely escaped. One pig on the gun-deck was killed, and five or six had palsy produced in their hind quarters.

Aug. 14, 1798. Examined the wound on the head of the sailor, this morning, and found a portion of the bone, nearly the size of the mortified spot, quite loose and likely to separate entirely in a few days. This exfoliation extends to the outer table only.

It should have been observed that no mark of external violence was observable on any part of the body of the soldier.

III.

*A Botanical Description of Urccola Elastica, or Caout-chouc Vine of Sumatra and Pullo Pinang; with an account of the properties of its inspissated Juice, compared with those of the American Caout-chouc. By WILLIAM ROXBURGH, M.D.**

FOR the discovery of this useful vine, we are, I believe, indebted to Mr. Howison, late Surgeon at Pullo-pinang; but it would appear he had no opportunity of determining its botanical character. To Doctor Charles Campbell of Fort Marlborough, we owe the gratification arising from a knowledge thereof.

About twelve months ago I received from that gentleman, by means of Mr. Fleming, very complete specimens, in full foliage, flower, and fruit. From these I was enabled to reduce it to its class and order in the Linnæan System. It forms a new genus in the class Pentandria, and order Monogynia, and comes in immediately after Tabernæmontana, consequently belongs to the thirtieth natural order, or class called Contortæ by Linnaeus in his natural method of classification or arrangement. One of the qualities of the plants of this order is, their yielding, on being cut, a juice which is generally milky, and for the most part deemed of a poisonous nature.

The generic name, Urccola, which I have given to this plant, is from the structure of the corol, and the specific name from the quality of its thickened juice. So far as I can find, it does not appear that ever this vine has been taken notice of by any European till now. I have carefully looked over the Hortus Malabaricus, Rumphius's Herbarium Amboinense, &c. &c. Figures of Indian plants, without being able to find any one that can with any degree of certainty be referred to. A substance of the same nature, and probably the very same, was discovered in the island of Mauritius, by M. Poivre, and from thence sent to France; but, so far as I know, we are still ignorant of the plant that yields it.

The impropriety of giving to Caout-chouc the term gum, resin, or gum-resin, every one seems sensible of, as it possesses qualities totally different from all such substances as are usually arranged under those generic names: yet it still continues, by most authors I have met with, to be denominated elastic resin, or elastic gum. Some term it simply Caout-chouc, which I wish may be considered as the generic name of all such concrete vegetable juices (mentioned in this memoir) as possess elasticity, inflammability, and are soluble in the essential oils, without the assistance of heat.

In a mere definition, it would be improper to state what qualities the object does not

* Asiatic Researches, vol. v. 167.

possess; consequently it must be understood that this substance is not soluble in the menstruums which usually dissolve resins and gums.

East India Caoût-chouc would be a very proper specific name for that of *Urceola elastica*, were there not other trees which yield juices so similar, as to come under the same generic character; but as this is really the case, I will apply the name of the tree which yields it for a specific one. E. G. Caoût-chouc of *Urceola elastica*, Caoût-chouc of *Ficus Indica*, Caoût-chouc of *Artocarpus integrifolia*, &c. &c.

Description of the Plant Urceola. Pentandria Monogynia.

Gen. Char. calyx beneath five-toothed; corol one petaled, pitcher shaped, with its contracted mouth five-toothed: nectary entire, surrounding the germs; follicles two, round, drupaceous; seeds numerous, immersed in pulp.

Urceola Elastica.

Shrubby, twining, leaves opposite, oblong, panicles terminal, is a native of Sumatra, Pullo-pinang, &c. Malay countries.

Stem, woody, climbing over trees, &c. to a very great extent, young shoots twining, and a little hairy, bark of the old woody parts thick, dark coloured, considerably uneven, a little scabrous, on which I found several species of moss, particularly large patches of lichen; the wood is white, light, and porous.

Leaves, opposite, short-petioled, horizontal, ovate, oblong, pointed, entire, a little scabrous, with a few scattered white hairs on the under side.

Stipules, none.

Panicles, terminal, brachiate, very ramus.

Flowers, numerous, minute, of a dull, greenish colour, and hairy on the outside.

Braets, lanceolate, one at each division and subdivision of the panicle.

Calyx, perianth, one-leaved, five-toothed, permanent.

Corol, one petaled, pitcher shaped, hairy, mouth much contracted, five-toothed, divisions erect, acute, nectary entire, cylindrick, embracing the lower two-thirds of the germs.

Stamens, filaments five, very short, from the base of the corol. Anthers arrow shaped, converging, bearing their pollen in two grooves on the inside, near the apex; between these grooves and the insertions of the filaments they are covered with white soft hairs.

Pistil, germs two; above the nectary they are very hairy round the margins of their truncated tops. Style single, shorter than the stamens. Stigma ovate, with a circular band, dividing it into two portions of different colours.

Per. Follicles two, round, laterally compressed into the shape of a turnip, wrinkled, leathery, about three inches in their greatest diameters—one celled, two valved.

Seeds,

Seeds, very numerous, reniform, immersed in firm fleshy pulp.

Explanation of the Figures.

1. A branchlet in flower, natural size.
2. A flower magnified.
3. The same laid open, which exposes to view the situation of the stamens inserted into the bottom of the corol, the nectarium surrounding the lower half of the two germs, their upper half with hairy margins, the style and ovate party-coloured; stigma appearing above the nectary.
4. Outside of one of the stamens } much magnified.
5. Inside of the same }
6. The nectarium laid open, exposing to view the whole of the pistil.
7. The two seed vessels (called by *Linnaeus* follicles), natural size; half of one of them is removed, to shew the seed immersed in pulp. A portion thereof is also cut away, which more clearly shews the situation and shape of the seed.

From wounds made in the bark of this plant there oozes a milky fluid, which, on exposure to the open air, separates into an elastic coagulum, and watery liquid, apparently of no use, after the separation takes place. This coagulum is not only like the American caout-chouc or Indian rubber, but possesses the same properties, as will be seen from the following experiments and observations made on some which had been extracted from the vine about five months ago. A ball of it now before me, is to my sense, totally void of smell, even when cut into, is very firm, nearly spherical, measures nine and a half inches in circumference, and weighs seven ounces and a quarter, its colour on the outside is that of American caout-chouc, where fresh cut into of a light brown colour till the action of the air darken it; throughout there are numerous small cells, filled with a portion of light brown watery liquid above mentioned. This ball, in simply falling from a height of fifteen feet, rebounds about ten or twelve times, the first is from five to seven feet high, the succeeding ones of course lessening by gradation.

This substance is not now soluble in the above mentioned liquid contained in its cells, although so intimately blended therewith when first drawn from the plant, as to render it so thin, as to be readily applied to the various purposes to which it is so well adapted when in a fluid state.

From what has been said, it will be evident that this caout-chouc, possesses a considerable share of solidity, and elasticity in an eminent degree. I compared the last quality, with that of American caout-chouc by taking small slips of each, and extending them till they broke; that of *Urceola* was found capable of bearing a much greater degree of extension, (and contraction) than the American: however, this may be owing to the time the respective substances have been drawn from their plants.

The *Urceola caout-chouc*, rubs out the marks of a black lead pencil, as readily as the American, and is evidently the substance of which the Chinese make their elastic rings.

It contains much combustible matter, burning entirely away, with a clear flame, emitting a considerable deal of dark-coloured smoke which readily condenses into a large proportion of exceeding fine soot, or lamp-black; at the same time it gives but little smell, and that not disagreeable; the combustion is often so rapid, as to cause drops of a black liquid, very like tar, to fall from the burning mass; this is equally inflammable with the rest, and continues when cold in its semi-fluid state, but totally void of elasticity; in America the caout-chouc is used for torches, ours appear to be equally fit for that purpose. Exposed in a silver spoon to a heat, about equal to that which melts lead or tin, it is reduced into a thick, black, inflammable liquid, such as drops from it during combustion, and is equally deprived of its elastic powers, consequently rendered unfit for those purposes, for which its original elasticity rendered it so proper.

It is insoluble in spirits of wine, nor has water any more effect on it, except when assisted by heat, and then it is only softened by it.

Sulphuric acid reduced it into a black, brittle, charcoal like substance, beginning at the surface of the caout-chouc, and if the pieces are not very thin, or small, it requires some days to penetrate to their centre; during the process, the acid is rendered very dark coloured, almost black. If the sulphuric acid is previously diluted, with only an equal quantity of water, it does not then appear to have any effect on this substance, nor is the colour of the liquid changed thereby.

Nitric acid reduced it in twelve hours to a soft, yellow, unelastic mass, while the acid is rendered yellow; at the end of two days, the caout-chouc had acquired some degree of friability and hardness. The same experiment made on American caout-chouc was attended with similar effects. Muriatic acid had no effect on it.

Sulphuric æther only softened it, and rendered the different minute portions it was cut into easily united, and without any seeming diminution of elasticity.

Nitric æther I did not find a better menstruum than the vitriolic, consequently, if the æther I employed was pure, of which I have some doubt, this substance must differ essentially from that of America, which *Berniard* reports to be soluble in nitric æther.

Where this substance can be had in a solid state, there is no necessity for dissolving or softening it, to render it applicable to the various uses for which it may be required; but where the dry caout-chouc is only procurable, sulphuric æther promises to be an useful medium, by which it may be rendered so soft as to be readily formed into a variety of shapes.

Like American caout-chouc, it is soluble in the essential oil of turpentine, and I find

it equally so in Cajeput oil, an essential oil, said to be obtained from the leaves of *Melaleuca Leucadendron*. Both solutions appear perfect, thick, and very glutinous. Spirits of wine, added to the solution in Cajeput oil, soon united with the oil, and left the caout-chouc floating on the mixture in a soft semi-fluid state, which, on being washed in the same liquor, and exposed to the air, became as firm as before it was dissolved, and retained its elastic powers perfectly, while in the intermediate states between semi-fluid and firm, it could be drawn out into long, transparent threads, resembling, in the polish of their surface, the fibres of the tendons of animals; when they broke, the elasticity was so great, that each end instantaneously returned to its respective mass. Through all these stages the least pressure with the finger and thumb united different portions, as perfectly as if they never had been separated, and without any clamminess, or sticking to the fingers, which renders most of the solutions of caout-chouc, so very unfit for the purposes for which they are required. A piece of catgut covered with the half inspissated solution, and rolled between two smooth surfaces, soon acquired a polish, and consistence very proper for bougies. Cajeput oil, I also found a good menstruum for American caout-chouc, which was as readily separated by the addition of a little spirit of wine, or rum, as the other, and appears equally fit for use, as I covered a piece of catgut with the washed solution, as perfectly as with that of *Urceola*. The only difference I could observe, was a little more adhesiveness from its not drying so quickly; the oil of turpentine had greater attraction for the caout-chouc than for the spirits of wine, consequently remained obstinately united to the former, which prevented its being brought into that state of firmness fit for handling, which it acquired when Cajeput oil was the menstruum.

The Cajeput solution employed as a varnish did not dry, but remained moist and clammy, whereas the turpentine solution dried pretty fast.

Expressed oil of olives and linseed proved imperfect menstrua while cold, as the caout-chouc, in several days, was only rendered soft, and the oils viscid, but with a degree of heat equal to that which melts tin, continued for about twenty-five minutes it was perfectly dissolved, but the solution remained thin and void of elasticity. I also found it soluble in wax, and in butter in the same degree of heat, but still these solutions were without elasticity, or any appearance of being useful.

I shall now conclude what I have to offer on the caout-chouc, or *Urceola elastica*, with observing that some philosophers of eminence have entertained doubts of the American caout-chouc being a simple vegetable substance, and suspect it to be an artificial production, an idea which I hope the above detailed experiments will help to eradicate, and consequently to restore the histories of that substance by *M. De la Condamine* and others, to that degree of credit to which they seem justly entitled; in support of which it may be further observed, that besides *Urceola elastica* there are many other trees,

natives of the Torrid Zone, that yield a milky juice, possessing qualities nearly of the same nature; as *artocarpus integrifolia* (common jack tree) *figus religiosus* et *Indica*, *Hippomane biglandulosa*, *Cecropia peltata*, &c.

The caout-chouc or *figus religiosa*, the Hindus consider the most tenacious vegetable juice they are acquainted with; from it their best bird lime is prepared. I have examined its qualities as well as those of *figus Indica* and *artocarpus integrifolia*, by experiments, similar to those above related, and found them triflingly elastic when compared with the American and *Urceola* caout-choucs, but infinitely more viscid than either; they are also inflammable, though in a less degree, and shew nearly the same phenomena, when immersed in the mineral acids, solution of caustic alkali, alcohol, fat, and essential oils; but the solution in Cajeput oil could not be separated by spirits of wine and collected again like the solutions of the *Urceola* and American caout-chouc.

IV.

On the different Sorts of Lime used in Agriculture. By SMITHSON TENNANT, Esq. F. R. S.*

I WAS informed last summer that in the neighbourhood of Doncaster two kinds of lime were employed in agriculture, which were supposed to differ materially in their effects. One of these, which was procured near the town, it was necessary to use sparingly, and to spread very evenly over the land; for it was said that a large proportion of it, instead of increasing, diminished the fertility of the soil; and that wherever a heap of it was left in one spot all vegetation was prevented for many years. The other sort of lime which was obtained from a village near Ferry Bridge, though considerably dearer from the distant carriage, was more frequently employed on account of its superior utility. A large quantity was never found to be injurious; and the spots which were entirely covered with it, instead of being rendered barren, became remarkably fertile. The different properties ascribed to these two kinds of lime were so very distinct that it seemed probable they could not be imaginary; and it therefore appeared to be worth the trouble of ascertaining them more fully, and of attempting to discover the nature of the ingredients from whence the difference arose. For this purpose I procured some pieces of each sort of lime-stone, and first tried what would be their effect upon vegetables in their natural state, by reducing them to coarse powder, and sowing in them the seeds of different plants. In both kinds the seeds grew equally well, and nearly in the same manner as they would in sand or any other substance which

* *Philos. Transactions*, 1799. Part II. p. 305.

affords no nourishment to vegetables. Pieces of each sort of stone were then burnt to lime; and after they had been exposed some weeks to the air, that their causticity might be diminished, some seeds were sown in them. In the kind of lime which was found most beneficial to land, almost all the seeds came up and continued to grow as long as they were supplied with water, and the roots of the plants had many fibres which had penetrated to the bottom of the cup in which they grew. Upon examining the composition of this sort of lime, it proved to consist entirely of calcareous earth. By its exposure to the air for about three months it was found to have absorbed four fifths of the fixed air required to saturate it. In the other kind a few only of the seeds grew, and the plants produced from them had hardly any stalks or roots, being formed almost entirely of the two seed leaves which lay quite loose upon the surface. This sort of lime being spread upon a garden soil, to the thickness of about a tenth of an inch, prevented nearly all the seeds which had been sown from growing up, whilst no injury was occasioned by common lime used in the same manner. Upon examining the composition of this substance, which was so destructive to the plants, it was discovered to contain three parts of pure calcareous earth, and two of magnesia. The quantity of fixed air which it had absorbed by being exposed for about the same time as the pure lime just mentioned, was only 42 hundredths of that combined with it before it was burnt.

As it seemed probable that the magnesia contained in this lime was the cause of its peculiar properties, the following experiments were made to determine the effects of that substance upon the growth of vegetables. Some seeds, chiefly of colewort, which were preferred from their growing quickly, were sown in uncalcined magnesia; but though they sprouted, the leaves never rose above the surface, and the plants were entirely without roots, nor did they appear to grow better in magnesia which had been washed in water containing fixed air. Calcined magnesia was, however, much more destructive, as the seeds would not come up in it. To compare its effects on vegetables with those of lime, each of these earths were mixed in different proportions with sand in small cups, in which seeds were then sown. The lime was obtained from marble; and before it was put into the sand, was made to fall to powder by being moistened with water. In a mixture of four ounces of sand with three or four grains of calcined magnesia, it was a long time before the seeds came up, and the plants had hardly any roots or stalks; and ten grains or more of magnesia, there was no appearance of vegetation. Thirty or forty grains of lime did not retard the growth of the seeds more than three or four of magnesia, and the injurious effects were not so lasting. The lime, by absorbing fixed air, soon lost its destructive properties; so that after keeping these mixtures four or five weeks seeds were found to grow in that with forty grains of lime nearly as well as in the pure sand; but in that with four grains of magnesia they produced only the seed leaves, as was described before. It was necessary, occasionally, to break in pieces the sand which had so
much

much lime, as it would otherwise have been too hard to admit the seeds to penetrate through it. Plants will bear a much larger proportion of magnesia in vegetable soil than in sand; with twenty grains, however, of calcined magnesia in as much soil as was equal in bulk to four ounces of sand, the seeds produced only the seed leaves without any roots; and with about forty grains they were entirely prevented from coming up.

The countries where the magnesia lime is employed, it was said that the barrenness of any spot on which a heap of it had been laid would continue for many years. To learn how far it could by time be deprived of its injurious qualities, I procured some pieces of mortar made of this species of lime from two houses, one of which had been built three and the other eight years: they were taken from the outside of the building where they had been exposed to the air. After they were reduced to powder, seeds were sown in them; only a few came up, and even those produced merely the seed leaves without any roots. As plants would grow in the lime-stone from which this species of lime was formed, although not in the mortar made from it, I wished to know what proportion of the fixed air originally contained in the lime-stone had been absorbed by the mortar. For this purpose a piece of it was finely powdered to render it of an uniform quality; it was then tried how much of this powder and of the lime-stone would saturate the same quantity of acid; by this means I ascertained the proportions of lime-stone and mortar containing equal quantities of the magnesian lime. The fixed air being obtained from them in those proportions and measured in an inverted vessel with quicksilver, it was found that the mortar which had been exposed three years had absorbed 43, and that of eight years only 47 hundredths of the quantity originally contained in the lime-stone. I was not able to obtain any mortar which had been made earlier, though it deserves to be known how much fixed air it was ultimately capable of absorbing. Common mortar which had been exposed to the air for a year and three quarters, had regained 63 hundredths of its full quantity of fixed air.

As the preceding experiments were tried during the winter, in a room warmed by fire, perhaps under circumstances more favourable to vegetation, the same quantity of magnesia would not be equally pernicious.

Magnesian lime-stone may be easily distinguished from that which is purely calcareous by the slowness of its solution in acids, which is so considerable, that even the softest kind of the former is much longer in dissolving than marble. From this property of the magnesian lime-stone there appeared to be reason for suspecting that the kind of marble which had been called Dolomite, from M. *Dolomieu*, who first remarked its peculiarity in dissolving slowly, might also be similar in its composition. An analysis of this substance was lately given in the *Journal de Physique*, but this is probably erroneous; for, upon examining three specimens, they were found to consist of magnesia and calcareous earth, like the magnesian lime-stone; so that it ought, no doubt, to be

con-

considered as the same species of stone, but in a state of greater purity. The pieces of dolomite were from different places, one of them being found among the ruins of Rome, where it is thought to have come from Greece, as many statues of Grecian workmanship are made of it, and no quarries of a similar kind are known in Italy; the second was said to have been thrown up by mount Vesuvius; and the third was from Tona, one of the western islands of Scotland. In many kinds of common marble small particles of veins may be observed, which are a long time in dissolving. These, upon examination, I discovered to contain a considerable portion of magnesia; but as they were probably not quite free from the surrounding marble, I did not ascertain the quantity precisely.

The chrysalized structure which may generally be observed in the magnesian limestone seems to shew that it has not been formed by accidental union of the two earths, but must have resulted from their chemical combination. The difficulty of dissolving it may also arise from the attraction of the different component parts to each other. The mortar found from this kind of lime is as soluble in acids as common marble, and the substances of which it consists are easily separated. The magnesia may be taken from it by boiling it in muriated lime, and lime is precipitated from it by lime water; but neither of these effects can be moderated by the stone before it is calcined.

Magnesian lime-stone is probably very abundant in various parts of England. It appears to extend for thirty or forty miles from a little south-west of Workop, in Nottinghamshire, to near Ferry Bridge, in Yorkshire. About five or six miles further north there is a quarry of it near Sherburn; but whether this is a continuation of the stratum near Ferry Bridge I have not learnt. From some specimens which were sent me I find that the cathedral and mells of York are made of it. I have not been able to learn whether there were any shells in the lime-stone of the tract of the country before mentioned. In Mr. *Marshall's* account of the agriculture of the Midland counties, he speaks of the lime made at Breedon, near Derby, as destructive to vegetables when used in large quantities. I therefore procured some pieces of it, and they were discovered to contain nearly the same proportion of magnesia as that before described. In this quarry the stone is frequently crystallized in a rhomboidal form, and petrified shells, not calcareous, but similar in composition to the stone itself, are sometimes but very rarely found in it. This substance seems to be common in Northumberland. In the third vol. of the *Annals of Agriculture* Dr. *Fenwick*, of Newcastle, observes that the farmers of that country divide limes into hot and mild. The former of these is no doubt magnesian, as it has similar effects on the soil, and he remarks that it is not so easily dissolved in acids as the latter. At Matlock, in Derbyshire, the two kinds are contiguous to each other; the walks on the side of the river where the houses are built being magnesian, and on the other calcareous. The magnesian rock appears also to be
incumbent

incumbent upon a calcareous stratum; for in descending a cave formed in this rock, a distinct vein of common lime-stone may be observed which contains no magnesia. The latter stratum is very full of shells; but though there is some also in the magnesian rock, yet they are very rare. In the following tables, containing the analysis of various specimens, some other places are mentioned where this substance is found, but of which I received no further information.

Although it was known that the magnesian marble and lime-stone consisted of two earths, their proportion was attempted to be discovered by trying how much gypsum and epsom salt could be obtained by means of vitriolic acid from a certain weight of each specimen. When the superfluous vitriolic acid had been evaporated by heat, the Epsom salt was separated from the Gypsum by water. The result of these trials is expressed in the following table.

	<i>Dry Gypsum.</i>	<i>Dry Epsom Salt.</i>
5 grains of lime-stone from Breedon gave	3.9	3.15
Matlock	3.95	2.9
Workshop	3.8	3.0
York	3.8	3.1
3 grs. of calcareous spar and 1 gr. of calcined magnesia gave	3.9	2.7

As the preceding method of estimating the quantities of magnesia and calcareous earth is liable to considerable error, I afterwards examined them in the following manner, which seems capable of great exactness. Twenty-five grains of each substance were dissolved by marine acid in a cup of platina, and after the solution was evaporated to dryness, it was made red hot for a few minutes. The mass remaining in the cup which consisted of muriated lime, and of the magnesia freed from the acid, was washed out with water and poured into a phial. There was then added to it a known quantity of diluted marine acid, somewhat more than was sufficient to re-dissolve the magnesia, and after the solution a certain weight of calcareous spar, part of which would be dissolved by the superfluous acid. By the quantity of that remaining undissolved it was learnt how much acid was required to dissolve the magnesia. The iron and argillaceous earth, contained in some specimens, were precipitated by the spar, and therefore could not occasion any error. The calcareous spar, however, dissolved more slowly where there was argillaceous earth, as it became coated with it; but this incrustation was occasionally removed, and in all the experiments the spar was left in the solution till it suffered no further diminution. For this purpose it was necessary to keep them slightly warm for some days, during which time the phials were generally closed to prevent any escape of the acid. The first experiment in the following table was made upon known quantities of magnesia and calcareous earth to try the accuracy of the process. For
this

this purpose also the second was repeated upon a piece of lime-stone, previously powdered to render every part of it the same quality. The first column shews the quantity of calcareous spar which might have been dissolved by the acid required to take up the magnesia. The second shews the corresponding quantites of magnesia in 35 grs. of each substance. The third expresses the quantity of lime. This was inferred by subtracting the weight of the magnesia and of the iron and clay from 13,2 grs. the weight of the whole quantity of earth in 25 grs. of lime-stone. This is probably not very incorrect, as in two specimens which differed most in the proportion of magnesia and lime; the weight of the two earths was nearly the same.

A piece of dolomite from Rome, was wrapped in a thin piece of platina, that no part of it might be lost, and being then exposed to a strong heat left of earth—52,9 per Cent.

Dolomite from Mount Vesuvius . . .	52,8
Breedon lime-stone . . .	52,4
Calcareous spar left of lime . . .	55,8

In three of the experiments also the calcareous earth was precipitated by mineral alkali; and the quantity of it being tried by that of the marine acid required to dissolve it, it corresponded very nearly with that put down.

A quantity of marine acid which would dissolve 15 grs. of calcareous spar, would also dissolve 5,5 of calcined magnesia, and 2,5 grs. of spar; so that 12,5 grs. of spar required the same quantity of acid as 5,5 grs. of magnesia.

The magnesia used was pure, and made red hot immediately before it was weighed.

<i>Substances examined.</i>	<i>Quantity of Spar which the Acid required to take up the Magnesia would have dissolved.</i>	<i>Quantity of Magnesia.</i>	<i>Quantity of Lime.</i>	<i>Iron and Clay.</i>
Mixture of 5,5. grs. of magnesia, and 14 grs. of calcareous spar	12,5	5,5	7,8	0
35 grs. of Breedon lime-stone previously powdered . .	11,53	5,071	7,929	.2
25 grs. from part of the same powder	11,56	5,082	7,913	.2
25 grs. of dolomite from Rome	12,2	5,37	7,73	.1
— dolomite from Iona	10,1	4,4	7,8	1.0 { insoluble substance

<i>Substances examined.</i>	<i>Quantity of Spar which the Acid required to take up the Magnesia would have dissolved.</i>	<i>Quantity of Magnesia.</i>	<i>Quantity of Lime.</i>	<i>Iron and Clay.</i>
—— Vesuvian dolomite	10,38	5,565	8,575	.06
Of second exp. from part of the same Vesuvian dolomite	10,03	4,411	8,849	.06
25 grs. of magnesian limestone from Wanfworth, near Doncaster	12,75	5,61	7,34	.25
—— Thorpe arch	10,95	4,84	7,8	.6
—— Matlock	12,5	5,5	7,388	.31
—— York Minster	11,	4,84	8,26	.1
—— Worktop	11,6	5,104	7,496	.6
—— Sherburn	11,5	5,08	7,56	.56
—— Westminster-Hall . .	10,1	4,44	8,37	.4

V.

*Account of the strange Effects produced by Respiration of the Gaseous Oxide of Azote.**

THE reader may recollect an extract of a letter from Mr. Davy, at p. 93, of our present volume, mentioning the discovery which constitutes the object of the present extract. In the pamphlet of Dr. Beddoes, mentioned below, we have a detail of some of the remarkable phenomena there alluded to; but are still uninstructed with regard to the method of repeating them with safety and effect. On this head, however, the Doctor assures us that, the communication of the full experience of the operators will be made without reserve or unnecessary delay in a periodical work, to be entitled *Researches concerning Nature and Man*; the first number of which will appear in less than three months, and will contain a paper on the Philosophy of Medicine, by Dr. B.; part of a vast chemical investigation connected with the gas so often mentioned,

* From Dr. Beddoes's *Notice of some Observations made at the Medical Pneumatic Institution*. Bristol printed 1799.

and including its history by Mr. Davy; an account of the cases in which it shall have been used; of an account of our experience (viz, of the Medical Pneumatic Institution) in pthical cases, by Dr. Kinglake, together with some communications.

The narrative of effects, in the words of the author, is as follows, p. 6.

“ Certain circumstances belonging to the gas, denominated by its great discoverer, Dr. Priestley, dephlogisticated nitrous gas, engaged Mr. Davy's attention. After making some experiments, which proved that its composition, properties, and mode of action had been mistaken by the latest experimenters, he was induced to respire it in small quantities mingled with common air. In these first trials he thought, without fixing his opinion, that it acted as a depressing power; and I had communicated his suspicion to Mr. Watt. Dissatisfied with the result, however, he at last ventured to breathe it pure. The first inspirations of the gas produced giddiness, fulness of the head, and in short, feelings resembling those of incipient intoxication, but unaccompanied by pleasurable sensation. At the next experiment I was present. The quantity was larger, and the gas more pure. The scene exhibited was the most extraordinary I had ever witnessed, except in the case of that epileptic patient, whom I have described (*Considerations on airs*, part iv. p. 13) as agitated, in consequence of the respiration of oxygen gas, with a long succession of the most violent movements. The two spectacles differed, indeed, essentially in one respect. In the former every thing was alarming; in the latter, after the first moments of surprize, it was impossible not to recognize the expressions of the most extatic pleasure. I find it entirely out of my power to paint the appearances, such as they exhibited themselves to me. I saw and heard shouting, leaping, running, and other gestures, which may be supposed to be exhibited by a person who gives full loose to feelings, excited by a piece of joyful and unlooked for news. As in the case of the epileptic patient, *no weariness or depression followed*: so in this case, *no exhaustion or languor or uneasy feeling took place*. The experiment Mr. Davy has very frequently repeated, and generally with the highest pleasurable sensations, and except under particular circumstances, with considerable muscular exertion, which have not in any instance been succeeded by fatigue or sadness.

“ Since that time, a number of persons have inhaled the same gas. The following is an abstract of the reports furnished by themselves. The inaccuracies (should any occur) will soon be checked by the full account. Imperfections both accounts will have, for it is impossible for the combined endeavours of the spectator and the subject of experiment adequately to represent what was sometimes seen and felt.

“ The individuals mentioned below might be classed in various ways. Many had previous apprehension. Some had never heard of the expected effect. Others disbelieved it. A distinction ought also to be made between those who respired before we had learned to prepare the air with certainty, and those who have respired it since.

From many hundreds of experiments, we have also now acquired an idea of the dose suitable to different temperaments. But there was a time, when for want of such knowledge, the results were less agreeable than might have been wished. All these circumstances require attention. At present, however, without attempting either strict arrangement or analysis, I shall merely endeavour to state the leading circumstances as briefly as perspicuity will allow.

“ Mr. *J. W. Tobin* (after the first imperfect trials), when the air was pure, experienced sometimes sublime emotions with tranquil gestures, sometimes violent muscular action, with sensations indescribably exquisite; no subsequent debility—no exhaustion.—His trials have been very numerous. Of late he has felt only sedate pleasure. In Mr. *Davy* the effect is not diminished.

“ *Patrick Dwyer* has always exhibited a ludicrous struggle between a propensity to laugh, undoubtedly produced by the air, and an eager desire to continue the inhalation.

“ *Rev. Richmond Barbauld* felt exhilarated, and was compelled to laugh, not by any ludicrous idea, but by an impulse unconnected with thought, and similar to that which is felt by children full of health and spirits—lassitude and languor through the day afterwards, which Mr. B. is disposed to attribute to hot oppressive weather, and a preceding sleepless night.

“ *Mrs. Barbauld—the Children's Friend.* At first pleasurable sensations, occasioning involuntary laughter; some momentary faintness afterwards.—We now understand the regulation of the dose, so as perhaps to be able to remove Mr. *Barbauld's* languor, and to give Mrs. *Barbauld* the pleasure without the transitory faintness.

“ *Mr. George Burnet* had never heard of the effect of the air, after inhalation broke out into the most rapturous exclamations I ever witnessed, breathed at two o'clock P. M. and had all day a most delightful flow of spirits.

“ *Mrs. Beddoes*—pretty uniform pleasurable sensations—propensity to muscular exertion, could walk much better up Clifton Hill—has frequently seemed to be ascending like a balloon, a feeling which Mr. *Burnet* strongly expressed.

“ *Mr. James Thompson.* Involuntary laughter, thrilling in his toes and fingers, exquisite sensations of pleasure. A pain in the back and knees, occasioned by fatigue the day before, recurred a few minutes afterwards. A similar observation, we think, we have made on others; and we impute it to the undoubted power of the gas to increase the sensibility, or nervous power, beyond any other agent, and probably in a peculiar manner.

“ *Mr. Thomas Pople*—at first unpleasant feelings of tension; afterwards agreeable luxurious languor, with suspension of muscular power—lastly increased powers of body and mind—vivid and highly pleasurable sensations. In a second experiment, all the faculties absorbed in fine pleasing feelings of existence.

“ Mr.

“ Mr. *Stephen Hammick*, Surgeon of the Royal Hospital, Plymouth. In a small dose, yawning and languor.---It should be observed, that the first sensation has often been disagreeable, as giddiness; and a few persons, previously apprehensive, have left off inhaling as soon as they felt this. Two larger doses produced a glow, unrestrainable tendency to muscular action, high spirits and more vivid ideas.

“ A bag of common air was first given to Mr. *Hammick*, and he observed that it produced no effect. The same precaution against the delusions of imagination was of course frequently taken.

“ Mr. *William Clayfield* has most resisted the effects of the gas. Pretty strong doses produced a transitory intoxication. In two instances, very large doses have excited the violent muscular orgasm, accompanied with exquisite pleasure, and followed by no debility.

“ Mr. *Robert Southey* could not distinguish between the first effects, and an apprehension, of which he was unable to divest himself. His first definite sensations were, a fullness, and dizziness in the head, such as to induce fear of falling. This was succeeded by a laugh, which was involuntary, but highly pleasurable, accompanied with a peculiar thrilling in the extremities—a sensation perfectly new and delightful. For many hours after this experiment, he imagined that his taste and smell were more acute, and is certain that he felt unusually strong and cheerful. In a second experiment, he felt pleasure still superior, and has since poetically remarked, that he supposes the atmosphere of the highest of all possible heavens to be composed of this gas.

“ ——— *Wilmot, M. D.* Involuntary laughing, with unusual muscular motions, but no particularly pleasant or unpleasant feeling, heat in the chest, heat and perspiration in the feet. On a second inhalation the sensations were pleasurable.

“ *Robert Kinglake, M. D.* Additional freedom and power of respiration, succeeded by an almost delirious, but highly pleasurable sensation in the head, which became universal, with increased tone of the muscles. At last, an intoxicating placidity absorbed for five minutes all voluntary power, and left a cheerfulness and alacrity for several hours.

“ A second stronger dose produced a perfect *trance* for about a minute; then a glow pervaded the system. The permanent effects were, an invigorated feeling of vital power, and improved spirits.

“ By both trials, particularly by the former, old rheumatic feelings seemed to be revived for the moment.

“ Mr. *Notcutt*, formerly lecturer on chemistry at Hackney, was twice thrown into an extatic pleasurable trance; the first time his spirits were better for the day; after the second, he was languid, but is inclined to impute this to exercise in oppressively hot weather. Perhaps, however, the second dose was too strong for his constitution.

“ Mr.

" Mr. *Wedgwood* breathed atmospheric air first without knowing it was so. He declared it to have no effect, which confirmed him in his disbelief of the power of the gas. After breathing this some time, however, he threw the bag from him, kept breathing on laboriously with an open mouth, holding his nose with his left hand* without power to take it away, though aware of the ludicrousness of his situation, all his muscles seemed to be thrown into vibratory motion, he had a violent inclination to make antic gestures—seemed lighter than the atmosphere, and as if about to mount. Before the experiment, he was a good deal fatigued after a very long ride, of which he permanently lost all sense. In a second experiment nearly the same effects, but with less pleasure. In a third, much greater pleasure.

" Mr. *Josiah Wedgwood*, and Mr. *Thomas Wedgwood* experienced rather unpleasant feelings, of some duration in the latter. But we doubt of the quality of the air breathed by both, as they took it (except Mr. *T. Wedgwood* once) at an early period of the investigation; and in this last experiment it had an effect rather agreeable.

Mr. *Lovell Edgeworth*, at first felt tremor, vertigo, dizziness of sight, which by degrees subsided, next a strong propensity to bite the mouth-piece; after finishing the air, eagerly wished for more, then was inclined to laugh, and did burst into a most violent fit of laughter; and lastly, capered about the room without having power to restrain himself.

" Miss *Morgan* found her feelings slightly pleasant from a small dose, but succeeded by giddiness. A larger dose raised the pleasurable sensations very high, and muscular power seemed increased; but upon the whole, the feelings were nearer soothing calmness than vivid exhilaration. In another trial with a large dose, more lively and lasting pleasure followed, the muscular power seemed unbounded, though on attempting to walk there was the intoxication. After each of these trials, a slight difficulty of breathing was felt within doors, but in the open air, the spirits were very greatly exhilarated. In another trial, sensation became at each inspiration gradually more indistinct, and recollection was lost for a few moments, the feelings not being unpleasant. Spirits afterwards rather depressed than raised. In all the latter trials the head seemed full, and vision indistinct, during inhalation.

" The Author of this notice, notwithstanding the freedom with which he had formerly inhaled oxygen gas, for some time waved the trial of the other. His apoplectic make, joined to the frequent occurrence of a degree of giddiness in others, rendered him timid. The perfect safety, however, with which he saw the two paralytic patients below mentioned, and also a third, who is just beginning a course of air, perform the experiment, overcame his scruples. For if the first unpleasant impression, which has

* This was practised in all the experiments.

been occasionally observed, had the slightest connection with the symptoms of apoplexy, or palsy, he supposed that either these patients, or some other person, would have been injured by the inhalation. He has now taken it daily for some time, in a manner that will be hereafter described. The first sensations had nothing unpleasant; the succeeding have been agreeable beyond his conception or belief, even after the rapturous descriptions he had heard, and the eagerness to repeat the inhalation which he had so often witnessed. He seems to himself, at the time, (for why should one fear to use ludicrous terms when they are expressive?) to be bathed all over with a bucket full of good humour; and a placid feeling pervades his whole frame. The heat of the chest is much greater from a small dose than he ever felt from the largest quantity of oxygen. A constant fine glow, which affects the stomach, led him one day to take an inconvenient portion of food, and to try the air afterwards. It very soon removed the sense of distention, and, he supposes, expedited digestion. He has never tried to bring on the high orgasm; but has generally felt more alacrity at the moment—not one languid, low, *crapulary* feeling afterwards. It occurred to him that under a certain administration of this gas, sleep might possibly be dispensed with; he is sure that from less sleep he derives more refreshment than for many years past. And his morning alertness equals that of a healthy boy.

“ During the printing of this paper, the author took a large dose before an excellent judge of the phenomena of intoxication, who on observing him attentively for some time, exclaimed, “ *why your eyes twinkle as if you were drunk. You are certainly drunk.* The observation was accurate. Intoxication, as indicated by unsteadiness and stammering at the time, and a random feeling for some hours after, was produced. It subsided into simple high spirits, and no languor followed. Till he took this air, going to a play always brought on a head-ach next morning. Now he rises just as fresh as on other occasions.

“ Upon the whole, he believes that the Pneumatic Institution might advance a fair claim to the premium, anciently proposed for the discovery of a new pleasure; and he ventures to say that the first slight unpleasant sensations may be obviated by due management, and the gas exhibited with safety even to hysterical females.”

After these accounts the Author proceeds to relate the case of a young lady subject to hysterical fits, and a little to cramps, who, on the suggestion of a friend, tried the gas without any previous mention of her nervous affections. She was immediately thrown into a succession of hysterical fits of considerable violence and of above an hours duration, which returned at the same time the following evening, but gave way to the subsequent exhibition of opium and the bark in large doses. After about a week she declined medicine and the fits returned with increased violence. Laughing, crying, starting, painful acuteness, and dulness of hearing, inordinate motion of almost every muscle,

muscle, with loss of power in each, expectation of immediate death successively occurred, and in a degree so alarming as to render it difficult to remove the apprehensions of the young lady's friends.

These fits appeared to have been certainly produced by the specific power of the gas; and other trials cautiously made by ladies who were acquainted with all the previous facts, and subject to hysteric affections, produced effects which tended to confirm the above inference.

I cannot attempt, with any hope of precision, to give an analysis of the other parts of the Doctor's pamphlet, and still less can I think of making any remarks on a subject so new, so striking, and at the same time so obscure. He seems to be of opinion that the blood may impart to the solids various compounds of oxygen, azote, &c. which are quite unknown to our chemistry;—that the nerves may require one kind of supply and the muscles another;—that the gas in question communicates the principle which gives energy to the nerves;—that of this principle the system of nervous patients possesses a morbid excess, but that the system of paralytic subjects is defective in that respect. From these and other reasonings it was expected to be beneficial in the latter disease, and the few cases yet tried are very strong in confirmation of this induction.

VI.

*A Chemical Examination of the Bath Waters, by GEORGE SMITH GIBBES, M. D.
F. R. S.*

(Concluded from page 404.)

A Great deal of attention seems necessary in ascertaining to any certainty the degree of heat of the Bath waters. So many causes occur to lower their temperature a few degrees, that their precise heat at any one part of the Bath will not be found to answer exactly with any other part. After pumping a considerable time at the King's Bath, the temperature is generally from 114 of *Fahrenheit* to 117, or even 118. The hot bath is nearly of the same temperature with the King's bath. The cross bath is of a somewhat lower temperature, from 110 to 112. As the pumps lead immediately from the springs, the temperature of these waters can be better ascertained from them than from the baths.

Twelve grains of nitrate of silver were added to six ounces of the King's bath waters, and seven grains of precipitate were left to the filtre. No precipitation was observed on the addition of more nitrate of silver to the filtered liquor. To four ounces of this liquor were added two drachms of a saturated solution of acetate of barytes, and twenty grains of precipitate remained on the filtre. There was no farther precipitation

tation of the addition of more acetate of barytes. Six grains of oxalic acid produced, when added to this filtered liquor, fifteen grains of precipitate. The solution of carbonate of ammonia produced a very slight precipitation in the remaining liquor.

The King's Bath waters, after they are cooled, appeared to be precisely of the same weight as the water which is used in this city for ordinary purposes. Although the Bath waters, when cold, appear to differ but little from common hard waters, yet at their source they are accompanied with some very powerful agents. Their high temperature gives to the iron they contain an extraordinary degree of activity. I have observed in a great variety of cases, that these waters produce very tonic effects, when large doses of the different preparations of steel have failed.

There is a prodigious quantity of æriform fluid continually evolved from these springs, which rises in large bubbles, and is dissipated in the surrounding atmosphere. Having collected a large quantity of this gas, I subjected it to the following experiments. A measure of this gas was added to a measure of nitrous gas, and a very trifling absorption took place, I mixed a larger quantity of it, with nitrous gas, and a very trifling discoloration took place. The same quantity of atmospheric air, and the same nitrous gas, were united together, and very red fumes immediately appeared. Into a vessel full of this air I poured some fresh lime water; on agitation the lime water became very turbid, and of a milk-white colour. A small portion of this air was absorbed by water. On mixing it with atmospheric air, and exposing it to the flame of a taper, there was not the slightest detonation. A burning body was instantly extinguished when placed in this air.

From the foregoing experiments I am led to believe that the Bath waters contain some very active principles. Besides their heat, which most assuredly increases the action of their other component parts, we find that they lower the purity of the air, by the quantity of azotic gas which is continually poured forth into the atmosphere over the baths. Large quantities of this air must be inspired by those who use the open bath, and as we know that an alteration in the proportions of the component parts of atmospheric air will produce evident effects on the human constitution, this circumstance may, I think, with propriety be pointed out as a source of medical inquiry. I have been informed by a very learned and scientific person, that siliceous earth has been found to produce, when dissolved in water, some very considerable effects on the animal economy. As my experiments lead me to believe, that this earth forms a large proportion of the solid contents of these waters, as it appears to be very minutely divided, and as the high temperature may give it activity, I think this circumstance also may be regarded as worthy the attention of medical practitioners. I could mention a great variety of instances where the Bath waters have produced great effects in disease; but as this is not intended as a medical communication, I must content myself with observing,

that those who have hitherto considered these waters as inefficacious, will agree with me, I imagine, in allowing that these principles must have considerable medical properties.

It seems difficult to account for the azotic gas which rises with these waters. The country round Bath contains prodigious quantities of lime-stone, in the greatest part of which I have been able to detect the exuviae of sea animals. As there must have been a great deposition of animal substances, and as these waters apply a continued and powerful heat to the lower strata of lime-stone, the azotic gas may thereby be detached, and brought to the surface with them. It may also happen that the atmospheric air may find a passage into the bowels of the earth, so that assisted by the great heat of these waters the oxygene may be combined with substances for which it has a great attraction, allowing the azotic gas to pass and appear by itself at the surface. Hence may arise the carbonic acid, which may be detached from the lime-stone by the acids formed by the combinations with the oxygene, producing thereby the sulphate of lime, &c. so easily discovered in the Bath waters.

VII.

Analysis of an Ore of Iron, the Composition of which has been hitherto misunderstood, by Mr. WILLIAM HENRY; including a Letter on Ores of Iron, addressed to Mr. THOMAS HENRY, F.R.S. By CHARLES HATCHETT, ESQ. F.R.S.

Manchester, Dec. 20, 1799.

ABOUT two years ago, a drysalter of this town requested me to examine chemically, a substance, of which he had purchased a large quantity, presuming it to be plumbago. Mere inspection, however, of its external characters, induced me to suppose that he had been deceived; and, after a careful analysis, my suspicion proved to be well-founded. I send you the following account of the mineral, and its composition; because it appears to have been, hitherto, not sufficiently understood, and even to have been mistated, by that excellent mineralogist, Mr. Kirwan.

The resemblance of this mineral to powdered plumbago is striking enough to mislead a hasty observer. It is in the state of a fine scaly powder, of a steel-grey colour, with somewhat of a reddish hue, which appears more evidently when it is finely spread on a white ground. Its lustre is metallic, and it possesses no transparency. When rubbed between the fingers, it has an unctuous feel; and adheres, but does not give a permanent stain to

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the skin. It does not, like plumbago, leave a shining trace on paper, but only a few glittering particles, which are removed by a sudden blow of the finger. If sprinkled lightly on the surface of a vessel of water, it subsides slowly; and a small part even remains suspended at last. Its specific gravity is but moderate; but difficult to determine precisely on account of its state of disintegration.

By calcination, in a low red heat, during an hour, it lost about one per cent, which was probably nothing but a little accidental moisture. It was not altered by being heated with free access of air. The dilute mineral acids, by long digestion, took up about ten per cent, which proved to be oxyd of iron. After this treatment, I thought its reddish hue evidently diminished, but its lustre and other external characters remained unaltered. When projected on red hot and melted nitre, not the smallest deflagration ensued; and, after washing off the salt, the mineral was recovered without diminution of weight or change of qualities.

Two hundred grains of the ore were next exposed to a strong red heat, during two hours, with 30 grains of charcoal. The residue weighed 152 grains; i. e. there had been a loss of 48 grains. Of this residue all but 17 grains were dissolved by dilute muriatic acid, with a copious discharge of hydrogenous gas. In order to ascertain how much of the insoluble part was unconsumed charcoal, it was projected on melted nitre with which it deflagrated. The part that resisted this treatment being collected after the salt was washed off, weighed 11 grains. If, therefore, to 48 we add 6, (the amount of the unconsumed coal) we obtain the true loss of 200 grains of the ore, viz. 54, or 27 per cent. But of these 200 grains, 11 will be shewn not to be oxyd of iron. The loss of 54 grains was therefore sustained in reality by only 189 grains of oxyd of iron; and the truly oxydated part of the ore lost, as nearly as possible, at the rate of $28\frac{1}{2}$ per cent.

The iron dissolved by the muriatic acid, was next precipitated by carbonate of soda, and repeatedly heated to dryness with nitric acid. After this, it was digested with dilute nitric acid, which took up no portion of the oxyd. This shews that no other metal was present.

This insoluble residue of 11 grains, which had the form of a white and impalpable powder, was repeatedly boiled to dryness with strong sulphuric acid, and then washed with hot distilled water. By this treatment it lost $2\frac{1}{2}$ grains, which proved to be a luminous earth. The remaining $8\frac{1}{2}$ grains were found, by the result of their fusion with mineral alkali, to be siliceous earth.

A mixture of 200 grains of the ore with the proper fluxes and charcoal, exposed to a violent fire in a blast furnace, afforded me a button of metal, weighing 144 grains. The iron thus obtained has the specific gravity of 7,300; was somewhat, but not perfectly

malleable; and was judged by an eminent iron master, to whom I shewed it, to be intermediate between crude and good wrought iron.

From the foregoing experiments, the component parts of this ore, and their proportions, may be assigned as follows:

One hundred parts contain
 Of oxygen, $28\frac{1}{2}$
 Iron, - 66
 Alumine, - $1\frac{1}{4}$
 Silica, - $4\frac{1}{4}$

From this analysis, we may infer, also, the proportion of foreign ingredients, in the metallic button obtained from the ore. For, since 200 grains of the ore contain only 132 of real metal, while the weight of the iron obtained was 144, it appears that the 144 parts of metal must contain 12 of foreign ingredients, or $8\frac{1}{4}$ per cent.

The general results of the foregoing analysis having been communicated by my father to Mr. *Hatchett*, along with a specimen of the ore, he was favored in reply with the following interesting remarks.

“The substance which Mr. *William Henry* has had the goodness to send me by your hands is, as he supposes, one of the species of the eisen-glimmer. The first, (called by the Germans eisenmann) is of a steel-grey colour and lustre. It does not feel unctuous, nor stain the fingers. The second (called eisenrahm) of which your specimen is a variety, is to be found described in the following authors.

Brann und Rothier Eisenrahm	} Widenmann's Handbuch der Mineralogie, p. 807
Ferrum Ochraceum Rubrum Inquinans	
Mine de Fer Micacé Rougeatre	

Hæmatites Micaceus, Wallerii, spec. 333; also in Mus, Lesh. of Kärnten, tom. 2. p. 453; Werner's Verzeichnis, 1 Band. p. 153; Catalogue de Raab, tom. 2. p. 265-269; and Kirwan, 1st edit. p. 275. The last mentioned author says, that it is combined with plumbago; but this appears to be a mere conjecture, without foundation. In his 2d. edit. vol. 2, p. 166 and 172, he considers the eisenmann and eisenrahm among those ores of iron, which consist of iron, combined with from 24 to 36 per cent. of oxygen. Although I am not acquainted with any regular analysis of this substance, yet, from what is known at present, there is every reason to believe that your son is in the right.

“The various writers, on the systematical arrangement of minerals, have separated the ores of iron, like those of other metals, according to their different ideas; and appear, in general, too much to have overlooked the chain, by which nature connects her products. This chain is peculiarly observable in the mineral kingdom, and is most evident in the natural metallic compounds called ores. The grey iron ores, for instance, may be regarded as differing from each other, only in the proportion of oxygen, by which they

they are more or less removed from the metallic state. The compass of this letter will not allow me to enumerate them; and I shall only therefore observe, that the eisenmann appears to me to differ from the eisenrahm, merely by a less degree of oxydation; and that the varieties of each are so many gradations, by which the first passes into the second. For example, the eisenmann, in the complete state, has the lustre and colour of steel; is not unctuous to the touch, and when scraped or pulverized, affords a grey powder. But other varieties yield a greyish red, or even a red powder; and, at the same time certain degrees of unctuousity are to be observed. These I consider as gradations of eisenmann into eisenrahm. Again, some varieties of the latter possess the steel-grey colour and lustre, and, in the like proportion, become more and more unctuous, and of a fuller red; till, at length, they resemble an impalpable powder of the red hæmatites. This sort may be observed like an efflorescence on the surface of the hæmatites, found at Ulverstone, and other places on the borders of Lancashire. And, as gradations, similar to those between the eisenmann and eisenrahm, are to be observed in the varieties of hæmatites, I am inclined to embrace the opinion which Wallerius seems to have formed, when he applies the name of hæmatites micaceus to the eisenmann and eisenrahm: For I strongly believe, that a great part of the varieties of hæmatites have been derived from the matter of these by a stalactitical operation with the adventitious accession of some argill and silex.

“The oxydation of the eisenrahm is even carried so far as to form transparent lamellar crystals, about $\frac{1}{16}$ of an inch broad, exactly like mica, of a beautiful deep garnet colour. This variety, I believe, has only been found in Dauphiné; and the only specimen in England I purchased at M. de Calonne’s sale. In respect to your son’s specimen, it seems to be a variety of eisenrahm, in which some of the characters of eisenmann are blended. A similar kind I have had from Luxillian in Cornwall; and when I was at Exeter, I saw great quantities of the eisenmann for sale; but the place where found was kept a secret. Soon afterwards, however, I had occasion to visit some tin mines on Dart Moor; and in one called Vytifor, I discovered the eisenmann forming thin veins between the main lode, or vein of tin ore, and the walls; and this was the mine from which Exeter had been supplied.”

VIII.

An Account of some Experiments on the Fecundation of Vegetables. By THOMAS ANDREW KNIGHT, Esq.*

THE result of some experiments which I have amused myself in making on Plants, appearing to me to be interesting to the naturalists by proving the existence of superfœtation in the vegetable world, and being likely to conduce to some improvements in Agriculture I have taken the liberty to communicate them to you.

The breeders of animals have very long entertained an opinion that considerable advantages are obtained by breeding from males and females not related to each other. Though this opinion has lately been controverted, the numbers of its opposers are gradually diminished; and I can speak from my own observation and experience that animals degenerate in size at least on the same pasture and in other respects under the same management when this process of crossing the breed is neglected.

The close analogy between the animal and vegetable world and the sexual system equally pervading both, induced me to suppose that similar means might be productive of similar effects in each; and the event, has, I think, fully justified this opinion. The principal object I had in view was, to obtain new and improved varieties of the apple, to supply the place of those which have become diseased and unproductive by having been cultivated beyond the period which nature appears to have assigned to their existence. But as I foresaw that several years must elapse before the success or failure of the process could possibly be ascertained, I wished in the interval to see what would be its effects on annual plants. Among these none appeared so well calculated to answer my purpose as the common pea; not only because I could obtain many varieties of this plant of different forms, sizes, and colours; but also because the structure of its blossom by preventing the ingress of insects and adventitious farina has rendered its varieties remarkably permanent. I had a kind growing in my garden which having been long cultivated in the same soil had ceased to be productive, and did not appear to recover the whole of its former vigor when removed to a soil of a somewhat different quality. On this, my first experiment in 1787, was made. Having opened a dozen of its immature blossoms, I destroyed the male parts, taking great care not to injure the female ones; and a few days afterwards, when the blossoms appeared mature, I introduced the farina of a very large and luxuriant grey pea into one half of the blossoms, leaving the other half as they were. The pods of each grew equally well; but I soon perceived

* *Philos. Transactions*, 1799. Part II. p. 195.

that those into whose blossoms the farina had not been introduced, the seed remained nearly as they were before the blossom expanded, and in that state they withered. Those in the other pods attained maturity, but were not in any sensible degree different from those afforded by other plants of the same variety; owing, I imagine, to the external covering of the seed (as I have found in other plants) being furnished entirely by the female. In the succeeding spring the difference, however, became extremely obvious; for the plants from them arose with excessive luxuriance and the colour of their leaves and stems clearly indicated that they had all exchanged their whiteness for the colour of the male parent; the seeds produced in autumn were dark grey. By introducing the farina of another white variety (or in some instances by simple culture) I found this colour was easily discharged and a numerous variety of new kinds produced, many of which were in size and every other respect much superior to the original white kind, and grew with excessive luxuriance, some of them attaining the height of more than twelve feet. I had frequent occasion to observe in this plant a stronger tendency to produce purple blossoms; and coloured seeds than white ones; for when I introduced the farina of a purple blossom into a white one, the whole of the seeds in the succeeding year became coloured; but when I endeavoured to discharge this colour by reversing the process, a part only of them afforded plants with white blossoms; this part sometimes occupying one end of the pod, and being at other times irregularly intermixed with those, which, when sown, retained their colour. It may, perhaps, be supposed that something might depend on the quantity of farina employed; but I never could discover in this or any other experiment in which superfecundation did not take place, that the largest or smallest quantity of farina afforded any difference in the effect produced.

The dissimilarity I observed in the offspring afforded by different kinds of farina in these experiments, pointed out to me an easy method of ascertaining whether superfecundation (the existence of which has been admitted among animals) could also take place in the vegetable world. For as the offspring of a white pea is always white, unless the farina of a coloured kind be introduced into the blossom, and as the colour of the grey one is always transferred to its offspring though the female be white, it readily occurred to me, that if the farina of both were mingled or applied at the same moment the offspring of each could be easily distinguished.

My first experiment was not altogether successful; for the offspring of five pods (the whole which escaped the birds) received their colour from the coloured male. There was, however, a strong resemblance to the other male in the growth and character of more than one of the plants; and the seeds of several in the autumn very closely resembled it in every thing but colour. In this experiment I used the farina of a white pea, which possessed the remarkable property of shrivelling excessively when ripe; and in the second year I obtained white seeds from the grey ones above mentioned, perfectly
similar

similar to it. I am strongly disposed to believe that the seeds were here of common parentage; but I do not conceive myself to be in possession of facts sufficient to enable me to speak with decision on this question.

If, however, the female afford the first organized atom, and the farina act only as a stimulus, it appears to me by no means impossible that the explosion of two vesicles of farina at the same moment (taken from different plants) may afford seeds (as I have supposed) of common parentage; and as I am unable to discover any source of inaccuracy in this experiment, I must believe this to have happened.

Another species of superfœtation (if I have justly applied that term to a process in which one seed appears to have been the offspring of two males) has occurred to me so often as to remove all possibility of doubt as to its existence. In 1797, the year after I had seen the result of the last mentioned experiment, having prepared a great many white blossoms, I introduced the farina of a white and that of a grey nearly at the same moment into each; and as in the last year the character of the coloured male had prevailed, I used its farina more sparingly than that of the white one; and now almost every pod afforded plants of different colours. The majority, however, were white; but the characters of the two kinds were not sufficiently distinct to allow me to judge with precision whether any of the seeds were produced of common parentage or not. In the last year I was more fortunate; having prepared blossoms of the little early frame pea, I introduced its own farina, and immediately afterwards that of a very large and late grey kind, and I sowed the seeds thus obtained, in the end of last summer. Many of them retained the colour and character of the small early pea, not in the slightest degree altered, and blossomed before they were eighteen inches high; whilst others (taken from the same pods) whose colour was changed, grew to the height of more than four feet and were killed by the frost before any blossoms appeared.

It is evident that in these instances superfœtation took place; and it is equally evident that the seeds were not all of common parentage. Should subsequent experience evince that a single plant may be the offspring of two males, the analogy between animal and vegetable nature may induce some curious conjecture relative to the process of generation in the animal world.

(To be continued.)

IX.

Description of an Hygrometer and Photometer. By Mr. JOHN LESLIE. Communicated by the INVENTOR.

THESE two instruments, in their form analogous, however dissimilar in their application, were suggested by the same train of reflections. Experience has amply confirmed the justness of the principles on which they are founded. But the developement of those principles, besides the professed objects of the instruments, exhibits several classes of nice and important physical inquiries for which they are most happily calculated. Regarding them as valuable acquisitions to our philosophical apparatus, I was induced to bestow much attention on their improvement, and have through perseverance succeeded to my utmost wishes. By successive steps, they have at length attained that simplicity of construction in which the mind rests satisfied. I consider it my duty, therefore, without farther delay to communicate them to the public. Of the researches, however, which, by their means, I have made, embracing a considerable extent of investigation, I must defer the account, as it will form part of a work shortly to be committed to the press, and which it is presumed will be found to contain a variety of new views in the physical sciences. For the present, I shall content myself with briefly stating the progress of my ideas, with accurately describing the mode of constructing those instruments, and with pointing out in general terms the subjects of inquiry for which, by their delicacy and facility of application, they are peculiarly fitted.

My attention was first directed to the subject of hygrometry by the perusal of the late Dr. Hutton's very ingenious *Theory of Rain*. The affection of air to humidity, as differently modified by heat, appeared to perform a most important part in the oeconomy of nature. But to ascertain the actual disposition of the atmosphere was still a desideratum. Sensible of the imperfection, if not the absolute inutility, of the ordinary contrivances for that purpose, I was soon convinced that they depended not only on arbitrary assumptions, but on hypothesis altogether erroneous. Abandoning, therefore, the various expedients to serve as hygrometers, I sought to discover other principles, and to introduce that geometrical precision which so eminently distinguishes the more cultivated branches of science. An examination of the circumstances attending the action of air on a humid surface seemed to offer the best prospect of success.

It is well known that evaporation produces cold, but the nature of the process and the true conditions which determine the effect, require still to be investigated. Water, exposed to free air, suffers a continued waste by exhalation; it must at the same time sustain a corresponding expence of heat. In this view, therefore, the temperature of the humid mass should undergo a progressive and unlimited diminution. Yet such is not actually

the case, for the cold induced never passes certain limits. It is manifest, therefore, that the evaporable matter must at last *receive* heat from some other source exactly in proportion as it *loses* its own. Nor is it difficult to perceive how that will be derived, for each succeeding portion of air which, in dissolving its due quantity of moisture, touches the humid surface, must be cooled down to the same standard, and consequently must deposit the excess of its heat. Hence, while the repeated *abstractions* of heat are uniform, the corresponding *additions* of heat will continually increase, till they at last counterbalance the former, and then the depressed temperature of the humid surface will remain constant. But each portion of air, parting with its surplus heat, must dissolve as much water as will saturate it, and consequently will carry off a quantity of heat proportional to that moisture, and necessary to its gaseous constitution and union with atmospheric air. As these two causes ultimately equalize together, the one will serve as a measure of the other; that is, *the cold produced by evaporation will accurately denote the degree of dryness of the air, or its distance from the point of saturation.* Thus, the effect of that process depends entirely on the disposition of the air, and is not modified at all by agitation, or the frequent renewal of surfaces. Such means can only accelerate the term of equilibrium, in the same manner as wind brings a thermometer more quickly to the standard than still air; though this standard in both cases continues the same.

Let it be observed that this reasoning is independent altogether of any hypothesis. By whatever mode the process of evaporation is carried on, our general conclusion will hold true, if it be only granted that the conveying of heat and the dissolving of moisture are effects which take place at the same time. Suppose it even possible for the air to hang stagnant around the humid mass, and the moisture to be transferred along the contiguous strata; still the heat being conducted by the same agents the result would be unchanged. In reality, however, air, acquiring elasticity by its action on moisture, is quickly succeeded by fresh portions, which thus maintain a perpetual circulation.

To discover the dryness or humidity of the air, therefore, we have only to find the change of temperature induced on a body of water insulated or exposed on all sides to evaporation. This principle I first established in the year 1790. My situation at that time afforded me all the facilities I could desire of reducing it into practice. Living with the late Mr. Wedgwood, I enjoyed the advantages of his celebrated manufactory, while my efforts were stimulated by the example of his eminent talents, and by his liberal zeal for the improvement of the sciences.

I procured a cup of unglazed biscuit-ware which is quite bibulous, about the size and shape of a pigeon's egg. This was filled with water, and suspended freely by a silk thread: beside it was placed a very nice thermometer, having the ordinary degrees subdivided into tenths, and of a peculiar construction, to indicate the difference between its prior and subsequent state. Being plunged into the cup, the mercury quickly descended and marked on the scale the depression of temperature which the water had suffered, that is, the measure of the dryness of the ambient air. The performance of this double instrument was very complete,

complete, and for the space of two years I frequently employed it in meteorological observations and other researches. I was still dissatisfied, however, with its complex nature, and the attention required in using it. Another resource presented itself in the augmented elasticity which air acquires in dissolving moisture. To measure that effect, I contrived a simple machine which yet answered perfectly the end proposed. And though, in its original object, it has been since superseded, I found it of the highest utility in detecting those minute alterations of volume which take place in the union of various bodies, thus extending the bounds of corpuscular dynamics, and marking the alliance with philosophical chemistry.

In the severe winter of 1795, I was naturally tempted to make experiments on the evaporation of ice, and the cold thereby produced. Instead of fixing the thermometer in a block of ice, I had the bulb covered with a congealed crust, by repeatedly sprinkling it with water, and suffering this to freeze. Placing in the same situation another corresponding thermometer with a naked bulb, I was surprised to remark how quickly and steadily their interval reached its *maximum*, extent, the evaporation of a minute film of ice, proving sufficient to cool down to its proper standard the whole mass of included quicksilver. Of the legitimacy of this inference I was convinced, when I reflected on the vast consumption of heat which must take place during the transition into the gaseous form, of a comparatively small portion of evaporable matter*. Two thermometers, therefore, filled with any expandible fluid, with quicksilver, alcohol, or air, the bulb of the one being wetted and the other dry, will by their difference denote the state of the air in respect to humidity. Nothing was wanted but to combine those instruments in such manner that they should indicate merely their difference of temperature. To accomplish this object, it fortunately occurred to me to employ two hollow glass balls, communicating with each other by a narrow tube, which contained some coloured liquor. In ordinary cases, the intermediate liquor would continue stationary; for the air in both balls having the same temperature, and consequently the same elasticity, the opposite pressures would precisely counteract each other. But if, from the action of the external air on the moistened surface, the one ball became colder, it is manifest the liquor would be pushed towards it by the superior elasticity of the air included in the other, so as to mark by the space of its approach the depression of temperature induced by evaporation†. This contrivance succeeded admirably, and after repeated trials I fixed on the most suitable form and dimensions. In the course of those trials, I was led to the discovery of the photometer, which shall be hereafter described. A farther alteration was then made in the construction of the instrument, and I was anxious

* A cold of one degree centigrade is produced in a mass of water by the evaporation of less than the 500th part of its weight.

† This supposes that air expands uniformly with equal additions of heat, a position not indeed strictly accurate, but which differs much less from the reality than is sometimes alledged. Experimenters are not even agreed whether those expansions form a rising or a descending series. At any rate, in the narrow range of the instrument, that deviation from regularity becomes altogether insensible.

to render it conveniently portable. At last I attained my wishes so completely, that the hygrometer when once adjusted can scarcely be deranged by the roughest treatment. It only remained to hit on some coloured liquor which should permanently retain its colour, and at the same time should not, by any change of temperature, be liable to modify the elasticity of the air included with it, either by absorbing or parting with humidity. Alkaline-lye, tinged with carmine, seemed to answer the best; but still I found that, by the influence of strong light, the liquor gradually changed its colour, while it shifted somewhat its place and advanced nearer the ball where it exposed the moist surface. I no longer doubted that the oxygene of the included air united with the colouring matter, and formed a precipitate. Instead of common air, therefore, I filled the balls with hydrogen gas, which appears fully to realize my expectation, and from the experience of nearly two months of the brightest season of the year, I believe I may confidently promise on the durability of the colour*. I shall now proceed to describe the hygrometer, and to give such directions as may enable an intelligent person, acquainted with such manipulations, to construct it.

To the one end of a slender tube, from 4 to 8 inches long, and with an uniform bore from $\frac{1}{32}$ to $\frac{1}{16}$ of an inch wide, is blown a ball about 4 or 6 10ths of an inch in diameter, of coloured glass, such as black, blue, or green, and is bent inwards, so that its posterior surface shall be in a line with the nearest edge of the tube. A similar tube, though somewhat shorter, with a bore of equal or greater width, has this enlarged at the one end into a cylinder capable of holding as much liquor as would fill the stem of the other tube, and terminated with a clear ball of the same size as the former. The tubes are a little widened at their other ends, to facilitate the junction. The balls are next to be filled with hydrogen gas, which may be done in various ways. One of the simplest is to insert each tube, and fasten it with bees-wax in a narrow necked flask containing the gas; then heat the ball with the flame of a candle, and suffer it to cool, and repeat this operation two or three times. The shorter piece is dipped in deliquated potash tinged with carmine, and a few bubbles of hydrogen gas are forced out by the heat of the hands till, on being allowed to cool, a proper quantity of the coloured liquor rises. The open ends of the two pieces are then dried, gently heated, and united by the flame of a blow-pipe in one straight tube. By forcing air with the heat of the hand from one ball into the other, it is easy to make the liquor hang together with its summit near the middle of the longer stem. The whole is then suspended in a close room from the coloured ball, and the other ball plunged in a vessel of water while a temporary scale is attached to the upper stem. On adding cold water, the liquor descends to near the juncture, and on adding warm water it rises near the upper ball; the difference between those two temperatures measured by a thermometer, and

* It would still be most desirable to employ common air only. Some acid or alkaline liquor, disposed neither to abstract nor communicate moisture, may be found capable of retaining its colour under the joint influence of light and oxygene. Probably certain metallic solutions have that property. I have very lately tried indigo dissolved in sulphuric acid. It seems to answer well; but at the brumal solstice, and in the murky atmosphere of London, I cannot easily judge what effect strong light would produce.

referred to the intermediate space, gives the magnitude of a degree*. I have adopted, as the most natural and convenient division of the thermometer, that of celsius or the centigrade; so that each degree of the hygrometer corresponds to the thousandth part of the interval between the freezing and boiling points. All the liquor is now pushed back into one of the balls, and by applying the flame of a candle the shorter branch is gently bent round till its ball touches the inside of the opposite tube, and lies $\frac{3}{4}$ of an inch below the inflected ball. The scale, which should contain from 50 to 150 degrees, is next divided,† and is fastened between the two branches with cement made of rosin and bees-wax. (See a drawing of the instrument of the full size, Fig. 1. Plate XIX.)

The instrument is adjusted by throwing air from the one to the other ball till the liquor rests at the top of the scale. The lower ball and its annexed cylinder, are covered with thin silk of the same colour as the upper ball, and a few threads are likewise lapped about that part of the tube which it touches. The instrument is lastly cemented into a piece of wood, either end of which admits a cylindrical case that serves equally to protect or to hold it. On other occasions, the hygrometer is inserted into the socket of a round bottom piece where it stands vertical.

But this instrument does not merely point out the dryness of the air; it enables us to determine the *absolute quantity* of moisture which it is capable of imbibing: for the conversion of water into steam is found to consume 524 $\frac{1}{2}$ degrees of the centigrade division, and evaporation, analagous in its effect may be presumed to occasion the same waste of heat. If, therefore, air had the same capacity as water, for each degree of the hygrometer it would deposit as much heat as it would abstract by dissolving the $\frac{1}{3240}$ part of its weight of humidity. But the capacity of air is to that of water as 11 to 6, and consequently it would require in that proportion a greater evaporation to produce the same effect. We may hence conclude, that, for each hygrometric degree, the air would require $\frac{1}{6} \times \frac{1}{3240}$ or $\frac{1}{2838}$ part by weight of water to effect saturation.

Strictly speaking the degrees marked by this hygrometer do not measure the dryness of the air at its actual temperature, but only its state of dryness when cooled down to the

* When a standard instrument is once constructed, others can be more easily, though not quite so exactly, graduated by comparison. In a dry room, they may be used for the occasion as hygrometers, and the magnitude of a degree in each thus determined. If the weather is clear and settled, it would be preferable to convert them into photometers, and expose them exclusively to the direct rays of the sun, when moderately elevated above the horizon. Other expedients will suggest themselves.

† If the temperature of the room happens to vary during the observation, it must be allowed for. When the season permits, the upper ball may be covered with snow, which will render the graduation more certain. There is always a small correction to be made for the inverted position of the cylindrical reservoir in the finished state of the instrument: let n denote the width of that cylinder compared with the bore of the tube, and a = the length of 100 degrees of the hygrometer in English inches; this space must be diminished, before division, by the $\frac{a}{8n}$ th part. The formula is easily derived.

‡ This has been stated, but I expect soon to ascertain the quantity with more precision.

standard

standard of the wet ball. The law, however, being known of the dissolving power of air as affected by heat, it is easy from the disposition of the air with respect to humidity at one temperature to derive that at any other. It will suffice to mention the result of a number of careful experiments:—supposing air at the freezing point to be capable of holding 50 parts of moisture; at 10 degrees centigrade, it will hold 100; at 20°, 200; at 30°, 400;—thus doubling at each increase of 10 degrees. Hence a table may be constructed by which those conversions will be easily made.

To omit nothing that tends to elucidate the theory of the instrument, I must observe, that the air in its contact with the humid surface is not absolutely cooled to the same temperature: the air and water really meet each other at an intermediate point determined by their compounded density and capacity. Consequently the indications of the hygrometer ought to be augmented by the $\frac{1}{400}$ part, or $\frac{1}{6} \times \frac{1}{850}$. But this quantity is too small in any case to be regarded.

Instead of water, the coated ball may be wetted with æther, alcohol, or other volatile liquids, and hence will be determined the attractions of air for those substances under the several modifications of heat, pressure, and previous moisture. It is easy likewise, to reverse the experiments by covering the upper ball with sulphuric acid, potash, or any deliquescent salt. A vast field of inquiry thus opens to view, presenting facts not only important in themselves, but calculated to correct and expand our ideas on the subject of chemical affinities.

By help of this hygrometer I have collected a number of meteorological observations, and investigated the nature and production of dews and their curious affections to the metals, glass, and vegetable substances. But what is more important, I have ascertained the attractions of air for humidity under different pressures, and with different temperatures. I have examined likewise the properties of other gases in those respects, and have succeeded in accommodating the result to a few geometric laws of beautiful simplicity. I forbear to say more at present, but cannot help considering the subject as almost exhausted, and the modifications of the atmosphere as at length reduced to a science.

* * * * *

Having so minutely described the hygrometer, I shall dispatch the photometer, though the more curious instrument, in a few words. In fact it is constructed in the same manner, only the upper ball is blown of black glass, or is blackened, and the lower one is quite diaphanous and free of specks. The former detains the incident light, while the latter transmits it freely. But light, in proportion to its absorption, causes heat, whether uniting with bodies it really constitutes the matter of heat, or only excites heat in the act of combination. But though the black ball acquires constant additions of heat, its temperature will not uniformly and perpetually increase; for the accumulated heat will at last be conducted off by the surrounding air exactly as it is received. The depression of the liquor, therefore, will measure the momentary afflux of light. To prevent the irregular effects of winds, which might accelerate that dispersion, the instrument is inclosed within
a glass

a glass case. But this case serves also an important purpose, for, by confining the circulation of the ambient air which alone transfers the continual augmentation of heat, it doubles the performance of the instrument. The cylindrical case should be made of clear glass neatly rounded over, and hermetically sealed at the end. Its width is not of much importance, only it should leave a free space not less than $\frac{1}{10}$ of an inch round the balls, and at least half an inch at the top. Indeed both the size and form may be regulated by convenience, for I found a receiver of 2200 inches to afford quantities scarcely one-tenth less than those given by a case of the ordinary dimensions.

Since I first constructed this instrument, in the autumn of 1797, I have been delighted with the nicety of its performance. It not only measures the direct rays of the sun, but the reflected light of the sky, for which it is principally designed. It is sensible to every fluctuation of the atmosphere, marks the progress and decline of the light of day, and the periodic increase and diminution of the brightness of the year. It enables us likewise to estimate other lights, such as the flame of a candle. By comparing two photometers, it is easy to determine the relative properties of different coloured substances—in reflecting, absorbing, and transmitting light. In the same manner, they will determine the question, whether the particles of light are spread over the prismatic spectrum with equal intensity. By help of this instrument, too, we can measure the quantity of light transmitted through various diaphanous bodies, and that reflected or absorbed at different angles of incidence from polished or rough surfaces;—in short, perform with the utmost facility all those ingenious experiments which have exercised the sagacity of Bouguer and Lambert. Another set of inquiries for which the photometer is nicely calculated, is to discover the conducting powers of different fluids for heat. If the glass case, for instance, be filled with a gas of higher conducting power than common air, the instrument will be proportionally less affected by the same afflux of light, since those are the two balancing conditions. With air, too, of different densities, the effects are materially different. In that way I have examined various liquids and gases, nay jellies and ice. My experiments on these and other points are nearly completed, and afford results which are satisfactory and important.*

JOHN LESLIE.

X.

Description of the Hydrostatic Lamp of Mr. PETER KEIR. (W.N.)

IT has at all times been considered as a most desirable object, to afford a constant and equable supply of oil to the wick in lamps, and it is an object or condition no less essential to the œconomy of this useful instrument, that the light from the flame should be inter-

* This paper was drawn up at Hamburg in the beginning of last July, for the purpose of insertion in the foreign scientific journals. Previous to its appearance in English, I have revised it, and made some slight alterations and additions.

cepted as little as possible. It happens unfortunately that these two conditions have usually been found to militate against each other. If the receptacle for oil be made tall and thin, the light will indeed be dispersed like that of a candle through a very large portion of the surrounding space or sphere; but as the consumption of a small portion of oil will occasion a considerable fall in so narrow a vessel, the fluid will soon be depressed too low for the capillary attraction of the wick to raise it with effect, and consequently the light will first decay and then go out. If on the other hand, the oil vessel be made broad and comparatively shallow, the flame will certainly be maintained for a long time, because a large quantity of oil must be consumed before the fall will be of any practical consequence; but at the same time the shadow of this broad vessel will obscure nearly an entire hemisphere, if the wick be in the middle of its upper surface, and it will obstruct much of the light even in the most favorable position of the burner, namely, on one side or edge of the vessel.

To remove these inconveniences as much as possible, there have been a variety of contrivances, among which the inverted vessel called the Fountain, is the most remarkable and the most esteemed. This vessel, or receptacle of oil, may be considered as a bottle filled and inverted with its neck plunged in a basin of the same fluid. The experiment may be familiarly made with a basin of water, to the satisfaction of those who are unacquainted with the effects of the pressure of the atmosphere, which prevents the water from quitting the bottle. In this situation it is seen, that when the water in the basin is by any means depressed, a bubble or portion of air forces itself into the bottle and rises to the top, while an equal bulk of water descends into the basin. Or the same effect may be seen in a glass apparatus very common in bird cages. In this manner it is that the oil flows down and supplies the consumption in lamps, which are provided with a reservoir of this kind.

The shadow cast by the vase or reservoir of a fountain lamp, is an inconvenience which may be much diminished by fixing the burner at the end of a tube or branch made as long as may be convenient. Another inconvenience is however less susceptible of an adequate remedy; namely, the expansion of the air included in the upper part of the reservoir, when the temperature of the place is by any means raised. This expansion will cause the oil to descend in greater quantity than the combustion demands, and has been found very prejudicial in assembly rooms, and other places of general resort, by the overflow of that fluid upon the floors, as well as the clothes of the company. It seems as if the famous Robert Hooke, who could not have been unacquainted with the fountain lamp, was led to the invention of his very ingenious lamp with a float, from reflections on this imperfection. This lamp, which is described in Birch's History of the Royal Society, consisted of an hemispherical vessel to hold oil, with a lip at one edge for the wick. An hemispherical solid was constructed so as very nearly to fit the cavity of the vessel, and was suspended by an horizontal axis, upon which it could freely move or librate. It would therefore hang in the vessel and almost totally fill it, when not occupied by any other substance; but as its specific gravity was designedly made equal to half the specific gravity of oil, it would

would be readily understood that it would float when oil was introduced at the lip. And as the floatage would be regulated or confined by the horizontal axis, there were two very curious properties obtained; that the bulk of the solid above the vessel would always be equal to that of the oil itself, and the oil would remain at the same height till it was all burned. It may perhaps afford some amusement to the reader to deduce these properties from the structure of the machine, which may be done with very little consideration.

Dr. Hooke's contrivance, nevertheless, though worthy of great admiration, appears to be practically nothing more than an improvement on the shallow dish lamp. For at the commencement the light does not illuminate much more than one hemisphere of the surrounding space; and when the float has descended, towards the end of the consumption, the illuminated part is upwards of three-fourths of that space.

A much completer solution of the practical problem of illuminating the greatest possible space, while the fall of the oil is rendered the least possible, was given to the public by Mr. Keir, of Kentish Town, in the year 1787, for which he obtained a patent. It seems probable that the advantages of this contrivance being directed to an object so much less striking than that of the lamp of Argand, which was then new in the mind of the public, might, together with the confined exhibition of an article reserved to a few hands, have prevented it from becoming generally known; and as it is probably unknown to many of my philosophical readers, I have thought an account of its construction would be acceptable.

In plate XX are exhibited a view and section of one of these lamps. The section only requires to be explained. From the slender figure of the vase it is evident that the flame is permitted to throw its light in all directions downwards and upwards, nearly in the same manner as a candle. The interior parts is divided into several compartments by the diaphragms at F and C. The space A A, above F, is open to the atmosphere; but the space B B, beneath C, is close. A tube F G proceeds from the space A A to the space B B, so as to reach nearly to the bottom at G: and another tube C D, proceeds from B B upwards, through A A, without communicating with this last space, and is enlarged at the upper part, so as to receive a wick with the apparatus of Argand, or any other. A solution of sea salt, or the mother water of salt, being first poured in by measure at E, flows down the tube into B B, and fills that space. A like measure of oil is next poured, which also descends into B B, and forces the dense saline liquor upwards, through G F, into the space A A. The specific gravity of this last is adjusted by dilution, so that when the space A A is properly filled the oil shall stand in equilibrio at the requisite height near E; that is to say, the surfaces in A, and at E, are elevated above the lower orifice at G in the inverse proportion of the specific gravities. This proportion is usually about 3 to 4. So that if any part of the oil be taken away from E by combustion, or otherwise, there will be a subsidence of the heavy fluid in A A to preserve the equilibrium; and during the whole subsidence in A A there will be a correspondent depression of the upper surface of oil, near E, which will be measured by four third parts of the first elevation of the dense fluid above the

partition, F D. Now the fall in A A, may be rendered very small by enlarging the diameter of the vessel at that part, and at B B; and the elevation of E above A, and consequently the insulation of the radiant flame may be governed at pleasure by prolonging the interval D C.

It is possible in the manipulation of this lamp that some oil or pieces of snuff may fall into the space A A, and float upon the liquid. This effect is to a certain extent beneficial, because the covering of oil prevents evaporation: but if this should require to be remedied, it is easily done by pouring the whole contents of the lamp into a basin, and after a few moments repose or straining, returning the liquids again into the lamp at E by a syphon or funnel, in which they will take their proper places by means of their relative weights.

We may recapitulate the good qualities of this lamp in a few words. 1. It is capable of any desired form or apparatus for the burners. 2. It presents no obstacle to intercept the emitted light; and 3. As it raises the oil by the mere gravitation of a non-elastic fluid it cannot in any case, like the fountain lamp, raise more than is wanted*.

XI.

Reflections on the Decomposition of the Muriate of Soda by the Oxide of Lead.

By CIT. VAUQUELIN.

CHEMISTS agree that the oxide of lead decomposes the muriate of soda; and this, as is well known, is one of the methods which have been proposed to obtain soda from this salt; but the manner in which this decomposition is effected still remains without a satisfactory explanation. In reality all the explanations which have been given imply a

* On the relative consumption and expence of tallow and oil promised (at p. 365 of our present volume) to be resumed, I may here observe that the manufacturers reckon that the lamp of Argand, of the common size, having the mean diameter of the channel containing the wick, = 0, 8, inch, will require half a pint of good whale oil to give its maximum of light for seven hours; and that I found by the method of shadows, some years ago, that such a lamp gives as much light as eight tallow candles of the size, called the middling six, fresh snuffed. The average light of a candle, allowing for the neglect of snuffing, is much lower; but on the other hand the combustion is complete in this lamp, but not so much so in this candle as in those of smaller size. It would be an useful research to determine the economy of the production of light by equal weights of candles of all the different sizes. To return, however, to our comparison, I am disposed on the whole to estimate the lamp upon our present knowledge as equal in duration and effect to one pound of candles; and to conclude, that the quantity of light will follow that of the oil, in all lamps, with thin wicks. And hence, as oil sells at seven shillings and six-pence per gallon, and tallow candles at ten-pence half-penny the pound, the expence of light by oil will be to that by tallow, as 8 to 21. Or if the candle would illuminate, during four hours for a penny, a lamp would at the same expence in oil give equal light for ten hours and a half.---N.

* Read to the French National Institute, on the first Priarial, and in the year 7, (May 20, 1799.) Inserted in the *Annals de Chimie*, xxxi. p. 2.

manifest contradiction. The superior affinity of the oxide of lead for the muriatic acid, which is urged for the solution of the difficulty, is destroyed by the decomposition of the muriate of lead, by means of caustic soda; and that of the carbonic acid contained in the litharge, to which account has been had, is equally contradicted by the absolute inaction of the carbonate of lead upon marine salt, and by minium which contains little of this acid, but nevertheless decomposes the muriate of soda.

To avoid this difficulty, some authors have affirmed that the oxide of lead decomposes sea salt only in part; but this was an error naturally produced by a faulty explanation of a fact which is true in itself.

I have, on the contrary, clearly ascertained that the decomposition of this salt is complete, when the quantity of oxide of lead is sufficient; for how can this partial action take place if pure soda be obtained? and why should the process stop at any determined limit, without having been weakened by some known cause?

With the intention of clearing up this subject I made the following experiments:

1. I mixed seven parts of finely ground litharge, with one part of muriate of soda; I sprinkled the mixture with a sufficient quantity of water, to give it the consistence of thin soup; after which I agitated it for several hours, in order to renew the surfaces and facilitate the action of the ingredients.

The oxide of lead lost its natural colour, and gradually became white; its volume was singularly increased; and in proportion as the water was absorbed the mixture acquired a considerable consistence, so that I was obliged at several reiterations, to add a quantity of water. Lastly, at the end of four days the litharge appearing to have entirely changed its nature, and no further progression being observable in the effects, I diluted the mass with seven or eight parts of water, and filtered. The filtered liquor had a very evident alkaline taste, and contained a small quantity of the muriate of lead in solution, without one atom of the muriate of soda. When it was reduced to about $\frac{1}{10}$ th of its volume, it afforded crystals of carbonate of soda, rendered opaque by some traces of muriate of lead.

2. The oxide of lead washed and dried, had a dirty white colour, and its weight was increased about one eighth part. When gently heated, it acquired a very fine lemon yellow colour, with the loss of 0,025 of its weight. A part of this oxide, heated with a solution of caustic soda, presented the following phenomena:

1. Its orange colour was converted into a dull yellow. 2. Its pulverent form was converted into that of needles, and its bulk was greatly diminished; the solution of soda had not perceptibly changed its taste, but it afforded a very abundant black precipitate, by the hydro-sulphuret of soda; a white deposition, with the muriatic of the nitric acids; but that formed by the latter was re-dissolved in an excess of acid. These depositions were perfectly similar to the portion of matter which was not dissolved by the soda.

3. One hundred parts of the same matter were heated with diluted nitric acid, which dissolved the greatest part; the remainder being of a white colour and crystalline form. This substance, separated from the fluid, was fused upon ignited coals, assumed a black

colour, and flew off in smoke, without leaving any metallic lead; effects which prove that this substance is the common muriate of lead. The portion dissolved in the nitric acid being evaporated by a gentle heat, afforded crystals of the nitrate of lead, among which there were some needles of the muriate of lead, which had been dissolved by the nitric acid.

4. 100 other parts of the same substance, heated with boiling water, were not perceptibly dissolved, and the fluid afforded scarcely any signs of lead, by the hydro-sulphuret of potash.

These experiments appear to me to prove: 1. That litharge, after it has decomposed the muriate of soda, is a muriate of lead, with excess of oxide. 2. That the caustic alkalis dissolve this salt without decomposing it. 3. That it is by virtue of the affinity which the muriate of lead has for this oxide that litharge decomposes sea salt. 4. That it is this quantity of oxide, exceeding the proportion of common muriate of lead, which communicates to this salt the property of acquiring a lemon yellow colour by heat; an event which does not happen with the true or common muriate of lead. 5. That it is this excess which renders this muriate of lead nearly insoluble in water. 6. And that lastly it is this which is dissolved in the nitric acid, and forms the nitrate of lead, leaving behind it the common neutral muriate of lead.

It is so true that the oxide of lead does not decompose the muriate of soda, but by forming a muriate of lead with excess of oxide; that when the common muriate of lead is decomposed by caustic soda, it is impossible even to deprive it of the whole of its muriatic acid. There constantly remains a sufficient quantity to put the lead into the state in which it is found after the decomposition of the muriate of soda. This is proved by the yellow colour it acquires in the fire, by its decomposition with the nitric acid, the separation of the common muriate of lead, and the formation of the nitrate of lead, which takes place in this operation.

It is therefore truly, by virtue of a double affinity, that the oxide of lead decomposes the muriate of soda: namely, by the united forces of the oxide of lead for the muriatic acid, and the muriate of lead for an excess of oxide.

This consideration shews why so large a quantity of the oxide of lead is required for completely decomposing the muriate of soda; because five-sixths at least of this oxide are employed, not in decomposing the marine salt, but in forming the muriate of lead with excess of oxide; and the fourth part at least of this oxide unites with the muriatic acid in the state of true muriate of lead. It will therefore be proper to assert that litharge completely decomposes the muriate of soda, when the dose is sufficient, and that soda never totally decomposes the muriate of lead, however considerable the dose.

If the carbonate of lead do not decompose the muriate of soda, it ought to follow that the carbonate of soda should completely decompose the muriate of lead; and this in fact is confirmed by experience.

It may moreover be observed, that the muriate of lead is not the only salt of this kind which

which has the property of absorbing an excess of oxide ; for the sulphate and nitrate, and perhaps others also, possess it. The proof of this may be had by decomposing the nitrate and the sulphate of lead by the caustic alkalies ; and particularly by ammoniac. There will constantly remain a small quantity of these acids in the oxide of lead, which will become visible the first by the nitrous vapours which are disengaged by heating the washed precipitate ; and the second by the residue left by the nitric acid, with the precipitate obtained from sulphate of lead, which residue is sulphate of lead. The decomposition of the muriate of soda is probably effected by the same means. I propose to examine this question speedily, and shall give an account of my operations to the class.

PHILOSOPHICAL NEWS, ACCOUNTS OF BOOKS, &c.

Philosophical Transactions of the Royal Society of London, for the Year 1799.

Part II. London. ELMSTY.

10. **A**N Account of the Dissection of an Hermaphrodite Dog. To which are prefixed some Observations on Hermaphrodites in general. By Everard Home, Esq. F. R. S.—11. An Enquiry concerning the Weight ascribed to Heat. By Benjamin, Count of Rumford, F. R. S. M. R. I. A. &c. &c.—12. An Account of some Experiments on the Fecundation of Vegetables. By Thomas Andrew Knight, Esq.—13. Observations on the different Species of Asiatic Elephants, and their Mode of Dentition. By J. Corse, Esq.—14. Some Observations on the Structure of the Teeth of Graminivorous Quadrupedes ; particularly those of the Elephant and *Sus Æthiopicus*. By Everard Home, Esq. F. R. S.—15. Experiments to determine the Quantity of tanning principle and gallic Acid contained in the Bark of various Trees. By George Biggin, Esq.—16. Essay on the Resolution of Algebraic Equations ; attempting to distinguish particularly the real principle of every method, and the true causes of the limitations to which it is subject. By Giffon Wilson, Esq.—17. On different Sorts of Lime used in Agriculture. By Smithson Tennant, Esq. F. R. S.—18. Experiments and Observations on Shell and Bone. By Charles Hatchett, Esq. F. R. S.—19. A Catalogue of Oriental Manuscripts, presented to the Royal Society. By Sir William and Lady Jones. Presents received by the Royal Society, from November 1798 to June 1799. Index.

Prospectus or Index to Lord Dundonald's intended Publication. Single Sheet, Octavo ; presented to his Friends and Men of Science, by his Lordship, but not (as I suppose) sold.

In this pamphlet the Earl of Dundonald announces that he intends soon to publish a detailed account of all his discoveries, processes, and patents, and of the circumstances which have hitherto prevented their becoming beneficial either to himself or the public. He complains of the most cruel and oppressive usage from individuals, and neglects on the part of the government, and states that he has not yet determined whether he will surrender or

lay open his patents and processes, because he is disposed to wait a few months to ascertain whether he may form any pecuniary connections which may render his discoveries beneficial to himself, as well as the public. In the case of such surrender he shall think himself at liberty to offer his services to foreign nations.

I cannot attempt to give my readers any abridgment of an index, or table of contents; and still less am I enabled to enter upon any of the subjects which are hinted at, and bear relation to the undertakings of the author, and the depressions his exertions may have suffered. The chief objects of the earl's researches and inventions are well known to the public. The preparation and purification of alkalis, the manufacture of soap, and its uses with different waters; new methods of making ceruse, alum, and other salts; improvements in the manufacture of iron; analysis of fossil coal, with the qualities and uses of its products, coak, coal tar, coal oil, ammoniac; analysis of wheat, production of sugar, and of vegetable alkali; examination and treatment of starch makers' liquor, &c. &c. are among the subjects enumerated in this prospectus.

Sugar from the white Beet.

Mr. Accum has presented me with samples of this sugar received from Berlin, where I understand it is now very commonly manufactured. The written account of the culture, produce, and cheapness received at the same time, appearing to want some corrections, I shall only state at present that the samples were 1. a brown or pale straw coloured sugar, in lumps or agglutinated grains, forming a coarse dry powder. It is not very sweet, and has a peculiar, though not strong, smell, which I think resembles that of some articles of confectionary consisting of sugar and flour heated or fried together. Of this sugar the beet is stated to afford five per cent. of its weight, leaving a pulp which is an excellent food for cattle. 2. A refined sugar, in very small crystalline grains, forming a powder of which the particles are slightly disposed to adhere, and which, when laid upon writing paper, has very nearly the same whiteness. I could not ascertain the figure of any of the grains under a deep magnifier, as most of them seem to be partly rounded. It has no foreign smell or taste. Equal weights of this and of good loaf sugar were separately dissolved in equal weights of water; and six out of seven gentlemen, who were present, and tasted the solutions, without knowing which was the beet sugar, determined that the solution of this last was the sweetest. I was among those who thought so: but it appeared to me that its flavour resembled a coarser sugar than that against which it was tried. From this notion I afterwards took two wine glasses of water, and sweetened the one with beet sugar, and the other with loaf sugar, with the addition of a small proportion of fine moist sugar. When the tastes resembled each other as nearly as I could bring them, I submitted them to the judgment of the company present who, from the irregularity of their conjectures, did not seem to find any notable difference. And when I myself again took up the glasses without noticing the distinctive marks, and endeavoured by the taste to determine which was the beet sugar, it

happened

happened that I was mistaken in my decision. This refined sugar seems therefore to be of considerable purity and strength. It is obtained from the other sugar, in the quantity of 55 per cent., together with 25 per cent. of residual syrup or molasses. 3. The other article was a bottle of this molasses. It is sweet with a singular vegetable flavour, rather fragrant, and would, I doubt not, afford either a pleasant vinous liquor by fermentation, or a considerable quantity of ardent spirit.

Count Rumford's Experimental Essays, Political, Economical, and Philosophical, Essay X. Part I. on the Construction of Kitchen Fire-Places and Kitchen Utensils, together with Remarks and Observations relating to the various Processes of Cookery, and Proposals for improving that most useful Art. Octavo. 94 Pages. 7 Plates. Price 2s. 6d. Cadell and Davies.

As I have usually given an abridged account of the valuable publications of this author, and mean to do the same with regard to this essay, I shall at present give the contents only, which are; Introduction. Chap. I. Of the imperfection of the kitchen fire-places now in common use—Objects particularly to be had in view in attempts to improve them—Of the distribution of the various parts of the machinery of a kitchen—Of the method to be observed in forming the plan of a kitchen that is to be fitted up, and in laying out the work. Chap. II. Detailed accounts, illustrated by correct plans of various kitchens, public and private, that have already been constructed on the author's principles, and under his immediate direction. Chap. III. Of the alterations and improvements that may be made in the kitchen fire-places now in common use in Great-Britain—All improvements in kitchen fire-places impossible, as long as they continue to be incumbered with smoke jacks. They occasion an enormous waste of fuel. Common jacks that go with a weight are much better. Ovens and boilers that are connected with a kitchen range should be detached from it, and heated each by its own separate fire. The closed fire-places for iron ovens and roasters can hardly be made too small.—Of the various means that may be used for improving the large open fire-places of kitchens.—Of the cottage fire-places now in common use, and of the means of improving them.—Of the very great use that small ovens constructed of thin sheet iron would be of to cottages.—Of the great importance of improving the implements and utensils used by the poor in cooking their food.—No improvement in their method of preparing their food possible without it.—Description of an oven suitable for a poor family, with an estimate of the cost of it.—Of nests of three or four smaller ovens heated by one fire.—Of the utility of these nests of ovens in the kitchens of private families.—They may be fitted up at a very small expence.—Occasional remarks respecting the materials proper to be used in constructing the sides and backs of open chimney fire-places.—Appendix.—An account of our experiment that was made to ascertain the expence of putting up a small oven, suitable for the family of a cottager.

A Translation of the Table of Chemical Nomenclature proposed by Guyton, formerly de Morveau, Lavoisier, Berthollet, and de Fourcroy; with Explanations, Additions, and Alterations: To which are subjoined Tables of single Elective Attraction, Tables of Chemical Symbols, Tables of the precise Forces of Chemical Attractions, and Schemes, and Explanations of Cases of single and double Elective Attractions. Second Edition, enlarged and corrected. By George Pearson, M. D. Quarto, 156 Pages, with ten large Tables. Johnson, 1799.

I shall give some account of this excellent summary of chemical science and nomenclature in the next Number of the Journal.

A Description, with Plates, of the Time Keeper invented by the late Mr. Thomas Mudge: to which is prefixed a Narrative by Thomas Mudge, his Son, of Measures taken to give Effect to the Invention, since the Reward bestowed upon it by the House of Commons in the Year 1793; a Republication of a Tract by the late Mr. Mudge on the Improvement of Time Keepers, and a Series of Letters written by him to his Excellency Count-Brühl, between the Years 1773 and 1787. Quarto, 176 Pages, with 9 Plates, and a good Portrait of the late Mr. Mudge. London, sold for the Author by Payne, Cadell, &c.

This work will be considered as an excellent store of facts, relative to a great artist and worthy man. I am sorry that there has been so much controversy respecting them, that I cannot with any fairness give an abridged statement of the narrative of Mr. Mudge, without entering into an ample field of discussion and enquiry, as to what may be said by his opponents. The impartiality of a journalist would require this; and I think that however interesting the subject may be to a certain part of the public, it would scarcely be enough to justify the detail in our work. The title will shew the value of this performance in those respects which can form no grounds of dispute.

An Essay on Electricity, explaining the Principles of that useful Science, and describing the Instruments contrived to illustrate the Theory, or render the Practice entertaining. Illustrated with six Plates; to which is added a Letter to the Author from Mr. John Birch, Surgeon, on the Subject of Medical Electricity. By the late George Adams, Mathematical Instrument Maker. Fifth Edition, with Corrections and Additions. By William Jones, Mathematical Instrument Maker. Octavo, 584 Pages, with an Index. London, sold by the Editor, price 8s.

This new edition of Mr. Adams's very useful and entertaining collection of electrical facts and experiments, is considerably improved by extending some of the descriptions, supplying omissions, and inserting such discoveries, particularly those concerning the Doubler, the Plate Machine, and Galearies, as have been made since the time of the Author, and were indispensibly necessary to bring the treatise to the level of our present knowledge of the subject.

Urceola Elastica.

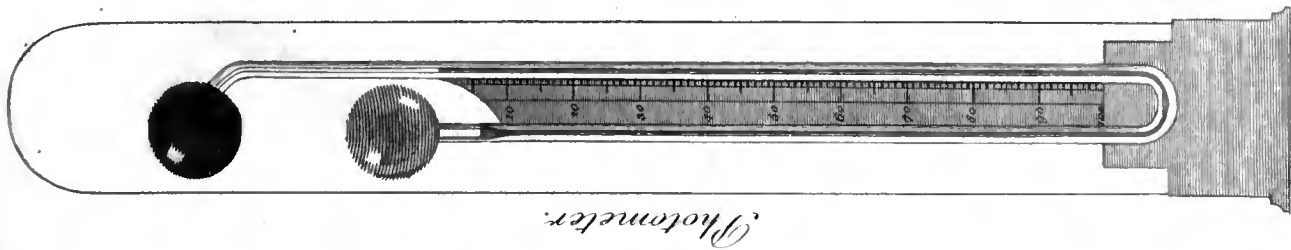
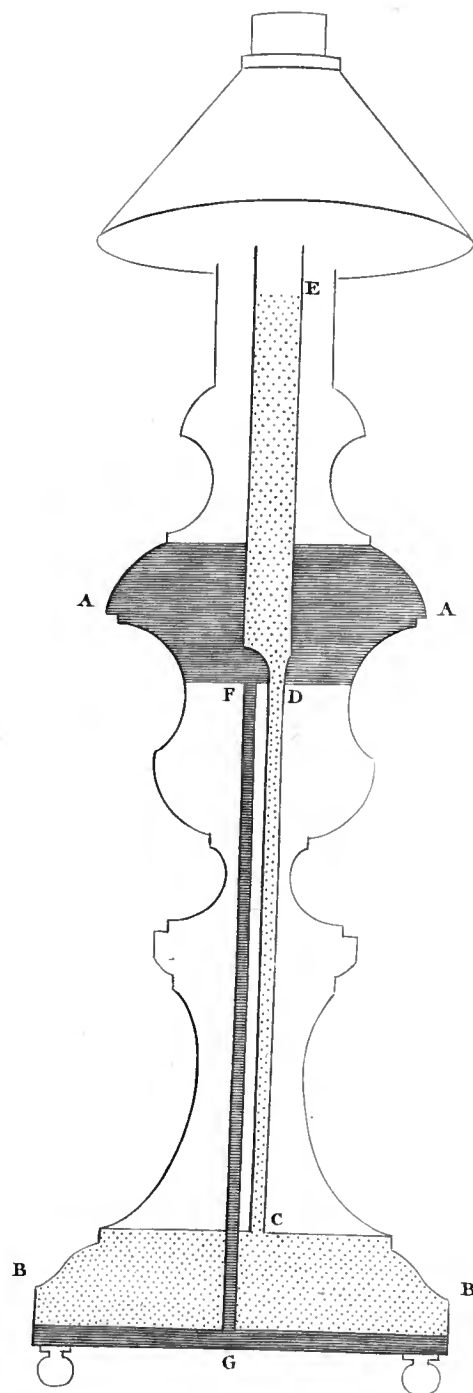


Fig. 1.



Fig. 2.





Keir's hydrostatic Lamp?



A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

FEBRUARY 1800.

ARTICLE I.

*Experiments on Indigo.—By a Correspondent.**

To. Mr. NICHOLSON.

Edinburgh, Jan. 4, 1800.

SIR,

I WISH, through the medium of your Journal, to make known to chemists some experiments, which, I hope, will not only be reckoned curious in themselves, (as they seem to throw a new light on a colouring matter procured from organic bodies) but of much moment, as they relate to a substance, of the utmost value to our commerce and manufactures.

The commodity I allude to is the dye indigo, in which I think I have detected a new principle, which has been overlooked by other chemists; this principle is of so volatile a nature, that it could only be discovered by the means I used, to which I was accidentally led by my first experiment, as you will observe in the sequel; and may mark the reason why the accurate Scheele, Bergman, and others, have taken no notice of it, as they invariably calcined the carbonaceous residue in the open air.

* This communication was accompanied by a letter containing the name and address of the author, and also of the friend to whom he makes his acknowledgments,---N.

It was in consequence of a conversation with an eminent chemist here, that I was led to make the following experiments on indigo; and I beg leave to take this opportunity of making my acknowledgements to him, for the trouble he has taken in repeating most of them, and adding others, which will be particularly noticed here; and which very much illustrate the general opinion I have formed of the changes that indigo undergoes in assuming the blue colour.

That gentleman, considering carbon as a compound, from some analagous reasoning, had been led to think indigo the base of that substance, and which, by its union with oxygen, formed charcoal, and some accounts given of the mode of procuring indigo, strongly favored this supposition; I, on the contrary, had conceived an opinion, that black was owing to an absence of oxygen; and that purple and blue held the next degree in oxygenation, of which we have many examples in the mineral kingdom.

I therefore fought, by means of inflammable bodies, to divest indigo of that portion of oxygen I supposed it to retain, and to discover, if possible, its base. My first experiment was more astonishing than satisfactory; I had mixed a quantity of indigo with sulphur of pot-ash, phosphorus and water, and was boiling them in a glass tube by the flame of a candle, when I was surprised with the appearance of a leaf of a metallic aspect, (in some places of a golden, and in others of a silvery hue) precipitated on the sides of the tube; (I must here observe that there seemed no appearances of solution; that the indigo never came to the green, and that the golden leaf took first a purple appearance, and assumed the metallic one immediately, when the phosphorus came off in white vapour) this leaf I long considered as a compound of phosphorus, or at least the phosphoric acid, or of some change wrought on them; I afterwards procured the same substance in needles, by heating the same matters, a little moistened with water in a crucible, and by phosphorus and indigo only, which confirmed me in the above opinion, till I found that the needles could be obtained by means of a lens concentrating the rays of the sun on indigo, under a jar of atmospherical air; it however should be noted that the lens was of a moderate size, and not powerful enough to inflame the indigo completely. I have since I think got sufficient data to prove, that these needles are a sublimation of a substance sui generis, and that they are probably dispersed in a state of combination through the whole animal and vegetable world, and in such fossil bodies as have once been organized.

I will here divide this account into three parts; I will first take notice of the properties of the needles, and leaf, then detail a singular one which I suppose to belong to the resinous matter, and finally observe the action of certain bodies on indigo in substance, chiefly with a view to show, that indigo acquires a green colour by absorbing oxygen, not by giving it out, according to Berthollet, Bancroft, and others; and that in Hausselman's famous experiments, an acid was most probably present to absorb the alkali, which was the true reason of the indigo's being precipitated.

Needles apparently metallic are to be obtained by sublimating indigo, either with phosphorus and sulphure of pot-ash, with phosphorus alone, in some cases from indigo per se,

(if

(if the temperature is well regulated) exposed to heat in a crucible, and by the rays of the sun concentrated by means of a middle sized lens on indigo; and the metallic leaf may be procured by sulphure of pot-ash, phosphorus and water, or volatile alkali and phosphorus boiled with indigo; and in the dry way by distilling indigo per se in a reverberatory furnace*.

As my most accurate experiments were tried on the residuum obtained in the latter mode, I will begin by giving the account of that experiment, and the means I used to insure against deception; though I had already drawn conclusions from former ones, of which this might be considered only as a correct repetition. I took half an ounce of indigo, and exposed it in a wedgewood retort, to the heat of a reverberatory furnace for several hours, and joined it to a pneumatic apparatus for receiving gases; I got none, though the heat was very intense, but in the receiver there was an alkaline liquor (mixed with a brown oil or tarry matter) which was evidently carbonate of ammoniac, as I ascertained, by the muriatic acid. I once considered this as taking its origin from the prussic acid, but was afterwards undeceived, as will be observed in the sequel. The residue in the retort apparently consisted of a charred matter, which other chemists would have calcined in the open air; but as I knew previously that it contained the volatile matter (which, except in rapid combustion, generally falls down by its own gravity) and the properties of which I was already pretty well acquainted with, I submitted the whole to digestion in muriatic acid, after examining it minutely; when I observed various apparently metallic leaves interspersed through it, some of which I have preserved, and, along with the needles, have put into the object sliders of a microscope.

The muriatic acid did not act on the leaf otherwise than by brightening its appearance, which was before blackish, and now took the golden hue, that was observed on that procured by phosphorus, &c. It was not at all acted on by the magnet; (the muriatic acid must have taken up both the iron and lime that is usually found in indigo) neither does alcohol act on the needles, or leaf; consequently, they can have nothing in common with resinous or bituminous substances, neither does caustic alkali act on them, though boiling, which precludes the idea of their being a concrete acid; no combination of iron with phosphorus, or muriate of ammoniac can resemble them; as siderite has neither their volatile properties, nor has phosphorus ever been found in indigo; besides, that its combination with iron gives a purple with prussiates; a very different effect from that which the solution of the needles present with those salts.

Neither have they any resemblance to martial flowers, which indeed would be decom-

* As I do not wish to take the credit of any thing that may be construed to belong to another, I will here mention, that I have read in O'Brien's calico printing since I made these experiments, that "the curious may sublime indigo, and thereby procure flowers, as with zinc, sulphur, &c." Whether this gentleman ever procured the needles I have mentioned I know not, or what meaning he annexes to flowers of indigo, though I know indigo may be sublimed in the blue state by a gentle heat.

posed by the muriatic acid, nor to the Duc D'Ayen's sublimate; which, besides requiring a great heat, was attracted by the magnet.

Neither does sulphuric acid act on this singular product of indigo, though concentrated and boiling. Nitrous acid only dissolves it, which it does rapidly; and on that solution my experiments, further to ascertain its quality, were performed.

The muriatic acid being washed off, the carbonaceous residuum, (the last of which gave a faint blue with prussiates) the leaves were dissolved in nitrous acid, which gave no symptom of containing iron; and I found alkalis invariably precipitate this solution white; unless, as will afterwards be shown, that the metallic matter be converted into an acid, in which case ammoniac dissolves with it, and seems to have the strongest attraction for it of any of them. When the muriatic acid has not been sufficiently washed from it, and it still retains a portion of iron, very pure prussiate of lime forms with it a deep green colour, which no acid whatever has the effect of changing to blue; but the air and light make it yield a blue precipitate. I once considered this to be the colouring matter of indigo regenerated, without the fixing resinous substance, and nearly in the same state with what Bergman calls precipitated indigo, as a caustic alkali turns it yellow; but I have since discovered that when sufficiently digested in muriatic acid, the nitrous solution yields no colour whatever with prussiate of lime. Galls, either in a watery or spiritous infusion, give a white precipitate, and the absorbant earths the same. By distilling the nitrous acid from indigo, I procured Hausselman's acid; which I found produced the same effects, and is undoubtedly this volatile matter converted into an acid by oxygenation, in a manner analogous to many other inflammable bodies; this acid forms an insoluble precipitate with potash, but has a stronger attraction for caustic ammoniac, which combination cannot be decomposed but by muriate of potash. I found also that this acid formed a green with prussiates, when not sufficiently purified from iron, which, when spread on paper, the rays of the sun (to which it was exposed for above a day) changed to yellow, and not to blue. I may here remark, that indigo in substance, heated with wax or tallow, produces a pink colour, which stained the stick with which I stirred the mixture, and that the nitrous acid changes it to a yellow, which I have observed return to the green and the blue by exposure to the light. These facts have caused me to form an opinion, that this volatile matter is a more general colouring principle of organic bodies than might be supposed; and though at present I will not enter further on the subject, I think I have traced it in other substances as well as indigo. Though I tried various other acids, both animal and vegetable, they had no peculiar effects on the above nitrous solution.

I now come to the singular action, that nitrous acid, repeatedly distilled from indigo, produces (as I suppose) on the resinous fixing matter, and which will be a test to discover that matter in any other body.

To twenty grains of indigo I added about two drachms and two scruples of nitrous acid with two waters; after repeated distillations, the resinous matter was nearly destroyed, and the crystals of the acid of Hausselman were visible on the sides of the retort; the liquor in

in the receiver was of a clear yellow, containing little or no nitrous acid, but yielding a strong smell of bitter almonds; though it was most likely that the nitrous acid should have decomposed the prussic, (which I then thought I had reason to consider as existing in indigo) yet after adding pot-ash to it, I tried it with sulphate of iron, and observed no appearances of blue by that test.

I could now conjecture it to be nothing else than laurel-water, of which it had every indication; I therefore tried it on a kitten, to which about two tea spoonfuls were administered, at three different times; after each dose, it was seized with convulsions, which were perfectly obvious both to the gentleman above alluded to, (who did me the favour to assist at the experiment) and to myself; it, however, survived the trial, owing, no doubt, to the dilution of the liquor, which was not above a third part of that obtained from twenty grains of indigo; and if the above should be proved true, by repeated experiments, it shows, that the products of organized bodies may be imitated by art, a principle that has been denied by many celebrated philosophers.

I will now conclude with some general remarks on the effects of certain substances on indigo itself.

My chemical friend, to whom I have been so often obliged in the course of these experiments, exposed indigo to the action of oxygenated muriatic acid gas, which has the effect of changing it to the green, in contradiction to the theory that supposes indigo to derive its blue from oxygen, and its green from the loss of it. The nitrous acid turns it yellow, as has been often remarked, and which yellow I have observed, when the acid has been weakened, returns to the green and blue by exposure to light. He also burnt a quantity of indigo in oxygenous gas, setting fire to it by means of a lens and the solar rays; the residue was a brownish white substance, which afforded the same solutions and appearances with the other residues of the indigo.

I have all along found that bodies capable of affording oxygen, have made indigo pass first from the blue to the green, and afterwards to the yellow, and in this last instance to the white, when the bituminous matter was destroyed by rapid combustion.

I have it in contemplation to repeat most of Hausselman and Bancroft's experiments, and expect to be able to prove, that when indigo is revived from its solution in an alkali, an acid is always present, or formed, which renders the alkali incapable of dissolving the indigo, by combining with it.

I will only beg leave to mention here one conclusion, drawn from Hausselman's experiments, which is clearly erroneous.

He exposed a solution of indigo by alkali and orpiment in carbonic acid gas; and got the same effect as in oxygenous, viz. the absorption of the gas and the revivification of the indigo: if I have not mistaken Dr. Bancroft, he brings this as an argument in favour of indigo receiving the blue colour by absorption of oxygen, which can only be so, on the supposition that that mixture can decompose carbonic acid, though it does not appear that any other matter has that power than phosphorus, and that only when a strong heat is applied.

Whatever

Whatever may be the fate of the foregoing experiments, whether they may be refuted or not, partially, it is clear that the metallic appearance has never before been so decidedly separated from indigo, and probably it is an universal principle in nature that may occasion these golden and silvery hues visible on many insects, particularly fire flies, which are supposed to give out phosphorated hydrogenous gas, and also in several kinds of fishes.

On the whole, my opinion is, that indigo consists of a peculiar volatile matter *sui generis*, capable of becoming blue by a small portion of oxygen, (though that is only rendered probable by the above experiments) combined with a resinous or bituminous substance, convertible by the nitrous acid into laurel-oil and gaseous products.

I am, Sir, with equal respect for your character,

as a man and as a philosopher,

your most obedient servant,

A FRIEND TO THE EXPERIMENTAL RESEARCH OF TRUTH.

P.S. I forgot to mention that I have tried to procure both the needles and leaf from Prussian blue by the same means that I used with indigo, but could produce no such effect.

Since I finished this account, I have repeated one of Dr. Bancroft's experiments, or at least on the same principle with his, and one of those that could be most decisive with regard to his theory. I took a caustic alkali of the shops, and treated it again with lime, till it gave almost no effervescence, with the same sulphuric acid, diluted with about eight waters, that I used in the latter part of the experiment. I took a drachm of indigo, of a kind that gave no effervescence with that acid, and two drachms of brown sugar, which did not effervesce either. I boiled these with two ounce measures of the above alkali; in a little time the liquor passed from the blue to the green and yellow; I then stopped the process, and after trying if it effervesced with the acid, which I found it scarcely did, I threw a quantity of it up into a jar of atmospherical air above Mercury, whilst it was almost boiling; when cold, I marked the height of the liquor; after standing two days, it had risen about two-thirds of an inch in a jar about five and a half high, and, except at the bottom, the indigo was regenerated in the blue state; I then threw up a quantity of the above diluted acid, and had a copious effervescence, and the surface of the liquor, below the air bubbles, descended to the mark: this was evidently carbonic acid; but to make the matter certain, I injected lime water, so that it touched the sides of the jar; when at first its appearance was pellucid, and afterwards a white precipitate of carbonate was evident on the sides of the jar. In attempting to transfer the jar to the water apparatus, I had allowed a good deal of atmospherical air to mix with the residual gas, so had no farther opportunity of examining it. I am sensible that this experiment ought to be repeated with accuracy, especially so as to determine the real quantity of carbonic acid, which was undoubtedly formed from the oxygen of the atmospherical air, and carbonaceous matter either in the sugar or indigo.

II.

New Researches into the Affinities of which the Earths exert upon each other in the humid and dry Way. By Citizen GUYTON.

(Concluded from page 422.)

Experiment 16.

BARYTES and strontian, in the same circumstances, shewed nodisposition to unite.

Experiment 17. Strontian was presented to alumine by means of the same solvent. The mixture assumed a milky colour, and afforded a precipitate, which the acid did not redissolve.

In support of these experiments, I may refer to those of Citizen Vauquelin, in which he has shown the action of barytes and strontian upon alumine*. This property is here asserted only in the due humid way as well as with regard to lime. They do not determine the vitrification of alumine. My experiments on this subject perfectly agree with those of Macquer, Ehrman, Achard, and Kirwan†.

We are indebted to the illustrious Scheele, in his Dissertation upon Quartz, Alumine, and Lime‡, for the first observation respecting the combination of alumine and lime. I have repeated the experiment which proved to him the possibility of this union, and I observed, as he did, that the product was a peculiar kind of earth, composed of alumine and lime, when alum was decomposed by a more considerable quantity of lime water than was necessary to precipitate the whole of the alumine.

The consequences to be drawn from these experiments naturally present themselves. Two earths held in solution by the same fluid, water for example, if they be soluble in that fluid or alkali if this solvent be necessary, or otherwise in one and the same acid. These exercise a mutual action, which tends to unite them, and separate them from the solvent, which, according to the laws of elective attraction, is effectual with regard to some earths, and ineffectual with others.

Thus it is, for example, that in the 1, 5, 10, 11th experiments, barytes and strontian act very differently with lime. A considerable resemblance in the results of the same earths, in different solvents, may also be remarked. Lastly, in the 10th experiment upon the earthy muriates, there are only four in which the mixtures did not give manifest tokens of the divellent affinities; and it may perhaps be proper to exclude from the list of exceptions the muriates of magnesia and alumine, which constantly assumed a milky tinge.

It must not however be concluded, that no affinity exists where there is no decomposition, or that it cannot even prove divellent in such cases; that is to say, by changing the

* *Annales de Chimie*, XXIX. 270, or *Philos. Journal*, III. 122.

† See the *Journal Polytechnique*. Cah. 3. p. 307, and Kirwan's *Mineralogy*, I. 56.

‡ See the French edition of his *Essays*, p. 96, &c.

common solvent of the two earths. I think it the more proper to insist on this observation, because magnesia and alumine themselves afford a striking example. If they be precipitated together from their solution in the muriatic acid by pot-ash, they contract an union sufficiently intimate to present the character of a peculiar earth, inasmuch that pot-ash no longer acts upon the alumine. This fact, which is of great value with regard to the present question, was observed by citizens Clouet and Hachette in a series of experiments, in which they engaged for the purpose of assisting me to fill up certain blanks in a table, in which I proposed to give all the direct combinations of simple or undecomposed substances.

§ 2. *Experiments in the dry Way.*

With regard to the affinities of earths to each other in the igneous fluid, or in the dry way, some of the facts which support it have been known long before the true cause was suspected. The results which relate to the property called fusibility were alone perceived, and no farther researches were made to explain what daily happens in the workshops of manufacturers, I mean the vitrification of lime and flint by each other.

Three years ago I announced Nos. 35 and 39 of my experiments on the fusibility of the earths by their mutual attraction, that equal parts of barytes and flint afforded in the crucible a very distinct vitriform product, and that equal parts of barytes and lime afforded a transparent glass*. After having mentioned these two results, I shall confine myself to add in this place those of some new experiments, directed particularly to the strontian and jargon earths.

Experiment 18. Strontian and flint mixed in equal portions, and placed in a saucer of platina in a fire of 140 pyrometric degrees, afforded a fine white frit, nearly equal in appearance to enamel, which slightly scratched glass. A few small transparent vitreous globules were seen, and some adhered to the vessel of platina.

Experiment 19. The mixture of strontian and lime also placed upon platina, after having undergone a heat of 153 degrees, left a hard white frit, porous beneath with bubbles, without adherence to the vessel; likewise presenting small vitreous globules, and some knobs of enamel.

Experiment 20. The jargon earth treated in the same manner with flint, produced by heat of 140 degrees a yellowish grey frit, pulverulent at its surface, with some white vitreous points of a beautiful transparency strongly adhering to the platina, and so hard as to cut glass.

Experiment 21. The mixture of jargon earth and lime afforded in the same circumstances a grey frit of little solidity, or even granular, and one small globule of enamel adhering to a point of the crucible of platina.

These facts are doubtless sufficient to prove, that these earths have also a sufficient affinity to each other to produce vitrification, when the due proportion shall have been de-

* Journal Polytechnique 3 cahier, pag. 306 & 308.

terminated by repeated trials. But before I draw any more general conclusions, I must make a few reflections on the manner in which we ought to consider this action of the earths upon each other.

§ 3. *Respecting the Manner in which these Phenomena ought to be viewed.*

Every chemist must be struck with the various points of resemblance which many of these facts seem to establish between the alcalis and some of these earths, and may be tempted to think, that they might with equal propriety be placed among alkaline as earthy substances, particularly if lime be joined with barytes and strontian.

In fact, these three earths have an acrid taste like the alkalis; the first in the highest degree, the second rather less, and the third in a degree still less, but so characteristic, that it is from this that our first idea of causticity has been derived.

These earths are soluble in water nearly in proportion to their causticity; but it is the effect, and not the dose which forms the character; otherwise we should have as many classes as bodies, which had not all their properties alike in the same degree of power and proportion of effect.

The solutions of the two first afford with ease and in abundance very distinct crystals; it is difficult to obtain a very regular crystallization of lime; but this solution, like that of the others, deposits part of its earth by cooling in the solid form. It is a true crystallization; for crystallization is not constituted either by transparency, or the visible regularity of form. There is a real crystallization whenever a fluid body passes to the concrete state by the rapid or slow abstraction of the fluid which gave it its form, or which is the same thing, which held it in solution. It is usual and very proper in the description of chemical phenomena, to distinguish crystalline depositions from those which are pulverulent; but this is done to distinguish them, and by no means to point out a different nature. If this were not true, we ought to reckon the metals uncrystallizable, because they are without transparency, or because they exhibit only a rough exterior, when too sudden cooling prevents us from seeing the symmetric arrangement of their particles. We might also affirm, that the salts which afford the most beautiful crystals, do not in fact crystallize when the water of their solution is suddenly abstracted by alcohol. Barytes itself here affords an example, for its aqueous solution is precipitated by alcohol in the pulverulent form. Here the crystallization, instead of being determined by the figure and attractive forces of the first particles, depends no longer on these circumstances, but on the mode of separation of the dissolving fluid, and all the accidents which may render its products, externally, of greater or less regularity with regard to our perceptions. If it were necessary, I might support these reflections by an experiment, which is the reverse of the foregoing, respecting barytes. As there are circumstances in which it passes to the concrete state without any visible crystallization, so on the other hand I have ascertained, that there are circumstances in which lime may be brought to the crystalline state. For this purpose, it is necessary only that lime water should be distilled in a glass retort, and that new lime

water should be added successively for four or five times before the distillation be urged to dryness; after which the vessels are to be left to cool without disturbance. At the bottom of the retort will be found small grains, exhibiting very perceptible faces and angles, and which must not be confounded with the pellicle usually formed by the carbonate of lime.

Barytes, strontian, and lime also possess in common with the alkalis, the property of giving a green colour to the syrup of violets, a red to curcuma, and of restoring blue colours, which have been reddened by acids; and this, in the language of accurate chemistry, implies nothing more, than that these substances have a stronger affinity to the acids, than is possessed by the colouring matter itself. I have elsewhere shewn, that tin and iron likewise restore the blue colour by seizing the acid*.

From the experiments before related, it follows that the three earths have, as well as the alkalis, a decided affinity with silica and alumina, as well in the dry as in the humid way; so that lime, takes silica from pot-ash, and seizes the alumina at the instant it is abandoned by the sulphuric acid.

The action which these earths exercise upon the oils, the soaps, and animal matters, the union they contract with the Prussian colouring principle, sulphur, and the acids, form so many new points of resemblance with the alkaline substances.

These numerous resemblances cannot be disputed, and appear at first sight to demand a common classical denomination, but does this opinion agree with sound principles; are there not also facts which oppose it. It will be proper to examine this point.

All bodies have common properties more or less numerous; so that in order to denote their qualities rigorously, it will be proper to admit two classes only, or to distinguish them under two relations; the first comprehending similar bodies, or those which have all their properties alike, and the latter those which have not this condition, or possess distinct properties:

In the second place, the properties of bodies are almost invariably the different degrees of the same effect. We call those substances fusible, which flow without requiring an intensity of heat greater than that of our common furnaces; we say that a substance is soluble, when it dissolves in water to a certain quantity; that such a body is fixed, because it does not rise in vapour, but with great difficulty, and in the same manner we speak of other properties. The limit of our classes is always the result of such comparisons; and as we can scarcely find two substances which ought to be placed absolutely on the same line, it follows that we must either renounce classification, or give a certain extent to our limits, by means of which similar effects, though very disproportioned, may serve by their connexion to determine the most essential characters.

A long succession of useless efforts ought to convince us, that nature has prepared nothing for our methods; but this is not a reason why we should destroy this instrument of

* *Recherches sur la matiere colorante des vegetaux, &c. Ann. de Chimie, tom. xxx. p. 185.*

science. It may be denominated a false key, but nevertheless, such as may assist us, in our approach certain objects which we should not otherwise behold, but confusedly and at a distance.

A third truth, which has certainly struck all those who have meditated upon the true object of a methodical classification, is, that the properties which exclude, are very differently characteristic from those which bring objects together. This arises from the circumstance, that in the confused accumulation of circumstances of resemblance, frequently ill determined, we find no basis of sufficient solidity to ascertain our divisions, except in the oppositions presented by such expressions as substances decomposed or not decomposed, combustible and incombustible, ductile and not ductile, soluble and insoluble, and the like. These principles being established, we have here to consider the facts of both kinds; those which exhibit an analogy between other substances and alkalies, and such other facts as form oppositions between them.

Among the former it is remarked, that magnesia may unite with sulphur; that magnesia and alumine produce the saponaceous state in oils; that the metallic oxides also form soap; that the oxides of lead and iron* produce the same effect as the alkalis in the vitrification of earths, particularly flint, &c. &c.

With regard to the facts which establish an opposition, and which we have observed to be the most conclusive, it is sufficient to observe, that the alkaline carbonates are soluble in water, but those of the three earths are not; that the same difference exists between the alkaline and the earthy soaps, in which respect the latter have a much greater resemblance to the metallic soaps; that the fixity of the earths is much greater than that of the alkalies; and to conclude, by a still more striking fact, the alkalies are soluble in alcohol, and the earths insoluble. If a sufficient quantity of alcohol be poured into an aqueous solution of barytes, the liquor is soon rendered turbid by small white flocks, which in a few minutes fall to the bottom of the vessel, and leave the fluid no longer capable of altering the colours of culcuma and fernambouc.

From the experiments and observations contained in this memoir, I think the following conclusions may be drawn:

There is a tendency to union existing between all the earths, as well in the humid as in the dry way, which according to the degree of elective attraction, determines their precipitation from a common solvent and their vitreous composition.

The union of two earths operates like the alloy of two metals, by virtue of the same law, which excludes the supposition of a property in one of the two bodies which may belong to another order.

By comparing the results of these attractions to the solutions, by any saline substances whatsoever, alkaline or acid; we should be often embarrassed to point out which of the

* Mr. Kirwan has shewn, that the oxide of iron, in a double or triple proportion, causes flint, and even magnesia to flow; that alumine mixed with two parts of oxide of iron is fusible at 160 degrees of the pyrometric scale of Wedgwood. *Mineralogy* I. 71.

earths act on each other in the manner of these solvents, because we find lime take silex from pot-ash; this last yields alumine to magnesia, and lime vitrifies barytes, as barytes vitrifies silex.

Lastly, the phenomena which may give rise to resemblances of this kind, ought not to be considered but as the effects of a more general cause; that attraction of near bodies, which in the inequality of its relations forms the bond of natural combinations, and the power of the instruments employed by chemistry to disunite them.

III.

Concerning the Influence of the Moon upon the Atmosphere of the Earth. By CIT. LAMARK.*

THE moon has certainly a great influence on the state of the atmosphere of the earth. For if that universal gravitation which produces the attraction between the moon and the earth, can occasion the flux and reflux of the sea, how can it be denied, that the same cause should not produce a kind of flux and reflux of the atmospheric air, continually displaced by the successive changes in the distances and positions of these bodies which gravitate towards each other? This influence of the moon on the atmosphere of the earth has never been doubted; but no person has yet, to my knowledge, examined the nature of that influence, in a manner sufficiently precise, to exhibit its true effects. Philosophers have been too desirous of discovering in certain aspects, namely, the syzgies and quadratures, the indicating points of the changes which this planet incessantly produces in the terrestrial atmosphere.

I have directed my observations for a great number of years to examine the variations in the state of the atmosphere, in order to discover, if possible, the principal causes, especially those which act most regularly in these phenomena; and I have at length succeeded in discovering the following principles:

1. It is in the elevation and depression of the moon, above or below the equator, that we must seek for the causes of those regularly variable effects which it produces on our atmosphere.

2. The determinate circumstances which augment or diminish the influence of the moon in its different declinations are, the apogees and perigees of this planet, its oppositions and conjunctions with the sun, and lastly, the solstices and equinoxes.

It is well known that after every time that the moon crosses the equator, it remains for about fourteen days in the hemisphere it has entered, whether austral or boreal. Every lunar month therefore presents one revolution of the moon in the zodiac, which may be divided into two distinct parts, and which produce or cause two particular atmospheric constitutions. I call one of these the boreal constitution, namely, that during which the

* Soc. Philomath. No. 15, p. 117.

moon passes through the six northern signs of the zodiac, and I call the other the austral constitution, because the moon passes through the six southern signs, during this period.

I am convinced by observation, that during a boreal constitution, the winds which chiefly prevail in this climate blow from the south, south-west, and from the west. Sometimes in summer they pass to the south-east. In general the barometer during this constitution, presents but moderate elevations in the column of Mercury. The weather is usually rainy or damp, and the air is loaded with numerous clouds. And lastly, it is particularly during this constitution that we observe the effects of storms and tempests, when the causes which occasion them become active

On the contrary, during an austral constitution, the winds which chiefly predominate, blow from the north and north-west, and in summer north-east, and even easterly winds. In general during this constitution, the barometer exhibits considerable elevations in the column of Mercury, at least if the wind is not very strong; the weather is then most usually clear, cold and dry, and in the summer it is seldom (I might almost say never) during this constitution that storms are formed.

These two atmospheric constitutions are not however so permanently characterised as to render it easy to distinguish them at all times by the state of the atmosphere. The atmospheric air is so moveable a fluid, and so easily displaced, that it is not surprising that in the temperate zones where the influence of the heavenly bodies acts less strongly, than between the tropics from various causes, that they counteract very often the regular influence of the moon, and tend to mask and even change its effects*.

The perturbations which these variable causes produce on the regular effects of the influence of the moon on the atmosphere, occasion in fact many variations in the two atmospheric constitutions I have been describing; and this is doubtless the reason why they have been hitherto disregarded. But I can positively assert, that these perturbations, though frequent, and sometimes very considerable, do not prevent the character of each of these constitutions from being remarked in the greatest number of cases.

The probability that I have found is, according to my observations, estimated as 5 out of 8; that is to say, out of 48 atmospheric constitutions comprehended in the lunar year, I estimate there will be found at least 30 agreeing with the principles pointed out in this memoir; and I must add, that among the disturbing causes which modify the before-mentioned effects, several may be foreseen, and perhaps even appreciated as to their quantity of effect.

It is not an opinion that I here offer, it is a fact I announce;—an order of things that I point out, and that any one may verify by observation. It appears to me to be altogether unnecessary to give a detail in this place, of all the reasons which might be adduced to prove the great utility of this knowledge.

LAMARK.

* To avoid lengthening this extract, I pass over in silence the enumeration and development of such of the variable causes as I have ascertained.

IV.

Directions for making the best Composition for the Metals of reflecting Telescopes, and the Method of Casting, Grinding, Polishing, and giving the great Speculum the true parabolic Figure. By the Rev. JOHN EDWARDS, B. A.*

THE methods in general used for casting, grinding, and polishing the metals for reflecting telescopes being well known to workmen, and having been treated of in the most full and satisfactory manner in Dr. Smith's optics, and also by Mr. Mudge, in the Philosophical Transactions, Vol. LXVII. Part. I. I shall not dwell upon these points, but shall add such directions and observations of *my own*, as I have found by experience to answer much better than the method taught by those writers. Some telescopes constructed by me have been tried by the Rev. Dr. Maskelyne, Astronomer Royal, and found very greatly to excel in brightness†, and to equal in other respects telescopes of the same size, constructed by the best artists in London.

Of the best Composition for reflecting Specula.

That I may not be tedious upon this point, it may be necessary to acquaint my reader, that I have made experiments upon the following metals and semi-metals, in order to discover a composition for a speculum which should reflect the greatest quantity of light, and consequently be capable of receiving the finest polish. I combined them in several proportions, and ground and polished them. The metals and semi-metals I tried were silver, platina, iron, copper, brass, lead and tin, crude antimony, regulus of antimony, martial regulus of antimony, arsenic, bismuth, zinc, and antimony combined with cawk-stone‡. Having tried many compositions of them, (see the Appendix) I found that 32 ounces of copper, with 15 or 16 ounces of grain tin (according to the purity of the copper) with the addition of a little § brass and arsenic; viz. one ounce of each to the above proportion of copper and tin, will form a metal, capable when polished in a proper manner, of reflecting much more light than any other metal, that has as yet been offered to the public. When I

* From the Nautical Almanac for 1787, inserted at the request and suggestion of a correspondent, who observes, that the almanac for that year has become very scarce in consequence of the demand for this treatise, which is not elsewhere to be found.---N.

† Mr. Edwards' telescopes shew a white object perfectly white, and all objects of their natural colours; very different from common reflecting telescopes, which give a dingy copperish appearance to objects. I found by a careful experiment, that they shew objects as bright as a treble object glass achromatic telescope, both being put under equal circumstances of areas of the aperture of the object metal and object glass, and equal magnifying powers; whereas the diameter of the aperture of a common reflecting telescope must be to that of an achromatic telescope as 8 to 5, to produce an equal effect.

NEVIL MASKELYNE.

‡ See a most curious experiment upon cawk stone and antimony in the Philosophical Transactions, No. CX.

§ If one ounce of silver be added to this composition, the metal will be much better and whiter.

say that the proportion of tin is from 15, or 16 to 32 ounces of copper, I would be understood, that the proportion of tin will not always be accurately the same, as copper will take more or less tin to perfectly saturate it, according to its purity. It might be of use previously to purify the copper as much as possible. A very little experience in these matters, will enable any one to know exactly when the copper is completely saturated; as the composition will, if broken, appear of a most beautiful bright and glossy nature, very much resembling the fine face of quicksilver. My method to ascertain that point accurately, is to melt 32 ounces of copper, and to add to it when sufficiently fused, 15 ounces of tin, and to pour the mixture into an ingot; then to a certain known portion of this composition, I add a very small, but known portion of tin; and thus by a few trials I can easily obtain the point of complete saturation, and the maximum of perfection. Having then ascertained what portion of tin I added to the above known quantity of the composition, I add the proportional quantity of tin to the whole when melted a second time. Thus if I find that I must add a quarter of an ounce of tin to one pound of the composition, so as to obtain the *ne plus ultra* of brilliancy*, then I know that when I shall melt the remainder of the metal a second time, in order to cast the speculum, I must add one of grain tin to four pounds of the composition, made according to the proportion of 32 ounces of copper to 15 ounces of tin. The arsenic must be added in the *second melting*, when the speculum is intended† to be cast, as the heat of the mixture in the *first melting* is so great, as to render the most part of the arsenic volatile, and in a great measure prevent its action upon the metals. It is somewhat singular that arsenic, though particularly recommended by Sir Isaac Newton‡ for this purpose, should be hastily thrown aside by the founders, as well as passed over unnoticed by the writers upon this subject. This imprudent disuse I can only attribute to the disagreeable fumes, or vapours, which arise when it is introduced into the crucible to the melted mixture, which may produce disagreeable effects upon the operator, if proper care is not taken to prevent them from being received into the lungs§. All the precaution necessary is, to bruise the arsenic coarsely, and introduce it into the crucible with a pair of tongs, having tied it up in a piece of paper; give it then a stir with a wooden spatula, retaining your breath, avoid it till you can see no more vapours arise from the crucible, when the metal will be ready to be poured into the flasks to cast the speculum. The great use of arsenic in this composition, is to render

* If too much tin should be added; viz. if 17 ounces of tin is put to 32 ounces of copper, the composition is not *brilliant* when broken, but of a *grey blue* and dull colour. If the quantity of tin be further increased, the metal will be almost black.

† Sir Isaac Newton melted the copper first, then added the arsenic, and lastly the tin; as without doubt, he knew that the tin should remain in a fluid state the shortest time possible. It is true that Sir Isaac Newton added the arsenic to the melted copper; but as he well knew that a great part of it would be rendered volatile, he therefore added a very large quantity of it; viz. arsenic 1 to copper 6.

‡ See Dr. David Gregory's optics, by Dr. Brown and Dr. Desaguliers, p. 219, or Philosophical Transactions, No. LXXXI.

§ I have been assured, by two ingenious experimental philosophers, that the fumes of arsenic, even when the garlic smell is very strong, are not in the least prejudicial to the lungs.

NEVIL MASKELYNE.

the metal much more compact and solid, and indeed much more *beautiful*, as one may experience by comparing the composition *with* arsenic, with the same composition of copper and tin *without* arsenic. In general I find one* ounce of arsenic sufficient for one pound of the metal. A much greater quantity of arsenic may be used, without any disadvantage to the beauty or compactness of the metal; but then it is too apt to tarnish if exposed to the air for some time: three quarters of an ounce, or an ounce of arsenic, to one pound of composition, will not tarnish in the least degree. Indeed, the reason why the metals generally made use of for specula, tarnish when they are much exposed to the air, is because the quantity of copper in their composition is not nearly saturated, and the acid contained in the air, by acting upon it, extracts the copper from the tin, and turns the metal into a dirty or dingy colored speculum, and which (besides the great loss of light) causes the common reflecting telescopes to shew all objects of a dirty red, or yellowish colour. This, however, is not the case in the metals made of the above composition; for as the copper is completely saturated, the air cannot act upon it in the least degree. I must not, however, pass over one caution in the mode or manner of melting the composition; and that is, that the copper must be melted first of all, and rendered as fluid as possible, then the brass and silver must be added, and the whole fluxed with the common black flux, made of two parts of tartar to one of nitre, or by stirring the melted mixture with a wooden spatula of birch, and made as fluid as possible. The tin must now be added, and the whole poured off immediately, after it is once stirred together; for if the mixture is *continued* on the fire some time after the tin is added to it, it will always prove porous afterwards, though it be melted a second time with the smallest heat possible. As I ever found this to be the case, I naturally conjectured, that the metal would be most solid and free from pores, when the tin remained the least time possible in a state of calcination. Experience determined the truth of my conjecture, and I now find, that the best method possible to make this composition to the greatest advantage, is to melt the copper as fluid as possible, and flux it with the black flux; to melt the tin in a separate crucible by itself; to take the two crucibles out of the fire, and pour the melted tin into the fluid copper; and stir it instantly with a wooden spatula, and pour the whole immediately into a large quantity of cold water. The sudden chill from the cold water divides the melted mass into an infinite number of small particles, and by that means cools it instantaneously, and consequently prevents the tin from calcining sensibly; and hence I have always found that in the second melting, the composition was entirely free from pores, even though no *arsenic* had been employed. Yet the addition of *arsenic* ever rendered it much more compact, and indeed† specifically heavier, as well as more brilliant and beautiful. On reversing the process, if the tin is put into the bottom

* One ounce of arsenic will, however, sufficiently act upon and bind three pounds weight of the metal, so that it shall never tarnish by the air.

† The specific gravity of the composition itself is 8,78, with the addition of one ounce of arsenic to one pound of metal, is 8,89.

of the crucible, and the copper at the top of it, which I have frequently done, the copper will melt with a very little heat; whereas when copper is put into the crucible by itself, it requires a pretty strong heat to cause it to melt. When I first made use of this method, I imagined I had discovered a very easy one to melt the copper, and consequently I thought I had greatly improved the common method; and as Mr. Mudge ascribed the pores in the metal to the tin being calcined by the great heat of the fluid copper at its first melting, I naturally expected to find the metal made by the above process totally free from pores, especially in the second melting, as the heat was considerably less than if the copper had been melted first by itself. However, I always found it full of pores, much more porous than I had ever seen it before. For some time I could not discover the true cause, having no idea that the pores were owing to the tin remaining so long in the fire in a fluid state, and therefore in a state of perpetual calcination. I attributed the porosity of the metals which I made of this composition to a multitude of causes, till, thoroughly tired of experiments and conjectures, to ascertain the true reason, I was determined to melt the copper first, and the tin afterwards, as I had always done, before I dropped upon this improved method as I imagined. The result was, the metal was infinitely more compact, and much less porous. By melting the copper first, and then adding the tin to it, I soon discovered that the longer the tin remained in the fire, the more porous the metal turned out; and *vice versa*, the sooner I poured it off, after I had added the tin, the more compact and free from pores it proved. From these observations, I determined to try the effect of adding the tin in a fluid state to the melted copper, and to cool the whole immediately, to prevent, as far as I was able, calcination from taking place. Experience confirmed my conjectures; and I soon found, that by pouring the whole melted mass the instant they were mixed and stirred together into cold water, the metal always proved in the second melting solid, and much more compact, beautiful, and white, than I had ever seen it by any other process. One thing I cannot pass over, as it affords a clear proof of the use of *arsenic* in rendering the metal much more solid and compact, and consequently more free from pores, than if no arsenic had been used in the composition. Whenever I made the composition by melting the copper and tin together, by putting them into the crucible at the same time, and melting them down together, the metal was always porous, as I observed before: however, frequently I melted it afterwards, and though I gave it no more heat than was barely necessary to melt it, yet if I added to this very porous metal, after it was melted, a small quantity of arsenic, viz. one ounce to one pound of the metal, it was really astonishing to see how much better the metal turned out, being considerably harder than before, and incomparably less porous. I mention this circumstance, which any one may easily try, to shew the very great advantage of using a small portion of arsenic to render it more compact; and as Sir Isaac Newton justly observes*, more *white* than before. The use of the small portion of brass in this composition, is to render it more tough, and not so excessive

* See Appendix to Gregory's Optics, p. 219, or the Philosophical Transactions, No. LXXXI.

brittle as this composition without the brass would prove. A small portion of silver will make the metal much whiter, though if too much is added, it is apt to be porous. Having said so much relative to the composition of the metal, which indeed is a capital article, I pass on to

The Manner of casting the Metal.

The sand most proper for casting this, and indeed any other metal, is a fine sand, with no more loam or clay mixed with it naturally than is absolutely necessary to make it tenacious enough to adhere together when properly moistened. If too much clay is mixed by nature with the sand, it will always *blow* the metal in different directions, sometimes, indeed, to the great danger of the operator. On the contrary, if the sand does not contain a sufficient quantity of clay, it will not remain in the flasks, or take a proper impression from the pattern or model. The best sand I could ever meet with for the purpose of casting specula is the common *Highbate* sand, (near London) generally used by the London founders. It should be as little wet as may be, and well beaten, but not too hard. The flasks should be at least two inches wider than the metal intended to be cast. If the sand is not of a sufficient thickness round the metal, it will instantly become dry when the hot fluid metal is poured into it, and, consequently, will contract, and, of course, the fluid metal will run out of the flasks. A proper thickness of sand will, however, prevent this accident. The metal or pattern should be made of brass, or hard pewter, and must be a little larger and thicker than the speculum intended to be cast from it, as the thing cast is always a little less than the pattern, owing to its contracting a small degree in cooling. A wooden pattern will not quit the sand near so well as one made of metal; besides wood will always warp by the moisture of the sand, and, consequently, will give a false figure or form to the intended speculum. As the composition I have given for the speculum is the *hardest*, and, consequently, the most brittle of any metal yet known, so it is the most difficult to cast. The common method of casting other specula will not avail in the least degree here; and it was a very considerable time before I found out a certain and infallible way to cast them free from faults or flaws in the face. In general, they cracked in the cooling, from the moisture of the sand. The only method possible to cast them well (for indeed I have tried many methods) is to cast them with the face *downwards*. The ingate or git should be at the back of the metal, and at the very edge of it; its breadth, where it joins the metal, should be at least half the diameter of the metal, and its thickness must be half the thickness of the metal at the edge; the upper part of the git should contain as much metal at least, or even more, than the speculum itself: I could give my reader sufficient reasons for every part of the process above directed, but I might be thought too tedious: suffice it then that I inform him that he will find these directions will answer in practice; and I believe I can say, that no one whatever can cast specula of this brilliant and brittle composition by any other means than what I have now pointed out. When the pattern, with its ingate or git, is taken out of the sand, ten or a dozen small holes should be made through the sand at the back of the mould with a small wire, or common knitting

knitting needle, to permit the *air* to escape as the metal is poured into the mould. I have found by experience that several small holes are infinitely better for that purpose* than one large hole. When the metal is melted a second time, which must be done with as small a degree of heat as possible, add the proportional quantity of crude arsenic in coarse powder, and stir it well with a wooden spatula; when the fumes are gone off, take the metal off the fire, take away the dross, and add half an ounce or an ounce of powdered rosin, or equal parts of powdered rosin and nitre, in order to give the metal a good face; stir it well with a stick, and pour it immediately into the flasks. When the git is filled up with the fluid metal, strike the flasks gently, so as to shake or jog the metal in them in a small degree; this will prevent any flaws in the face from any air-bubbles being lodged there. When the metal has remained in the flasks for a few minutes, so as to become entirely solid, open the flasks while the metal is red hot (it cannot crack in this state, though it is exposed to the air, as all metals are malleable when they are red hot) and take out the speculum with a pair of tongs, laying hold of it by the git, but take care to keep the face downwards, to prevent it from sinking. Force out the sand from the hole in the middle of the mirror with a piece of wood or iron, and place the speculum in an iron pot, with a large quantity of hot ashes or small coals, so as to bury the speculum in them a sufficient depth. If the sand is not forced out of the hole in the manner above directed, the metal, by sinking as it cools, will embrace the sand in the middle of the speculum so tight, as to cause it to crack before it becomes entirely cold. And if the metal is not taken out of the sand, and put in a pot with hot ashes or coals to anneal it, I can assert, that the moisture from the sand will always break the metal. Let the speculum remain in the ashes till the whole is become quite cold. The git may be easily taken off by marking it round with a common fine half round file, and giving it then a gentle blow. The metal is then to be rough ground and figured.

Of the rough grinding and figuring the Speculum.

In rough grinding, figuring, and polishing the metal, two tools only are necessary besides a common grind stone. One chief reason why workmen do not give a good figure to their specula is, because by pursuing Dr. Smith's or Mr. Mudge's method, they use too many tools, which, in a great measure, destroy each other's effects. As nature always acts in the most simple manner, so if we could always imitate her in this respect, we should arrive at a much greater degree of excellence in most of our mechanical pursuits. Besides the tools generally made use of by workmen are considerably too large in diameter ever to give a direct and uniform figure. All the tools I make use of are a rough grinder, composed of lead and tin mixed together, or else of pewter; this rough grinder serves also for a polisher; this tool, with a bed of stones or hones, are all that are necessary. A bruiser (as directed by Dr. Smith and Mr. Mudge) is totally unnecessary, causes considerably more work, and,

* If several small holes are made for the air to escape, the back of the metal will be cast much neater than if one hole only is used for this purpose. Besides, when one hole only is used to let the air escape, the metal is very apt to crack in that place, owing to the sinking of the metal in cooling.

after all, is really detrimental. The best method I have ever found to rough grind the speculum is to grind the surface of it quite bright upon a * common grindstone, made nearly to the figure or focus of the speculum by a gage. Take it then to a convex tool, made of lead and tin, or else of pewter, and grind the metal upon it with fine emery. This emery, however fine it may be, will break up the metal very much, but we can easily cure that process, as I shall shew hereafter. This tool or rough grinder should be made of an *elliptical form*, and not circular, (for a reason I shall point out hereafter) and of such dimensions that the shortest diameter of the ellipse shall be equal in breadth to the diameter of the mirror, and the longest diameter of the elliptical tool should be to the shortest diameter in the proportion of ten to nine accurately, for a reason to be mentioned hereafter. The manner of working or figuring the metal upon this tool, and indeed upon all the succeeding tools, is taught in Dr. Smith's Optics, or the before mentioned volume of the Philosophical Transactions. I shall refer my reader to those publications, as I only mean to give my own improvements. When the metal is brought to a true figure, it must be taken to a convex tool, formed with some stones from a place called Edgedon, in Shropshire, situated between Ludlow and Bishop's Castle. These stones or hones are of a fine grain, and will easily cut the metal, and bring it to a fine face. Indeed, the blue hones † used in general by the opticians for this purpose will scarce touch the metal, and it will be a laborious undertaking to bring the metal to a fine face, so as to take out all the breaks up from the emery by the common blue hones. By means, however, of the above mentioned stones, they may be easily ground and truly figured. The bed of stones should be of a circular figure, and but very little larger than the metal intended to be figured upon it, viz. about two-tenths of an inch, but not more for a speculum of four or five inches in diameter. If the tool is made considerably larger than the metal, it will grind the metal perpetually into a larger sphere, and by no means of a good figure: if the metal and tool are of the same size exactly, the metal will work truly spherical; but it is apt to shorten its focus less and less, unless the metal and tool are worked alternately upwards. It had, therefore, better be made a little ‡ larger than the mirror, when it will not alter its focus. Too much water should not be used at a time upon the hone pavement, or the figure will be very bad, which may easily be seen by the face of the metal appearing of different degrees of brightness in different parts of it. When the metal is brought to a very fine face and figure by the bed of stones, it is ready to receive a polish: but before I shall give any directions with regard to the manner of polishing it, I must mention a circumstance or two I had inadvertently passed over. The metal

* The grindstone may easily be brought to the form of the gage by holding the sharp end of an iron bar against it while it is turned round, till so much is worn away from its surface as shall cause it to take the true curvature of the gage

† Should any one, however, make use of the common blue hones, he should use as little water as possible when the metal is put upon them, as they will cut much better when barely wet, than if much water is used upon their face.

‡ About one-twentieth part greater in diameter.

must not be cast too thick, or it will never take the parabolic figure intended to be given to it. The best proportion I have found for this purpose is, a metal of four inches and a half diameter, and eighteen inches focus, should be four-tenths of an inch thick at the edge of it: the back of the mirror should be convex to strengthen it, and to cause it to spring and adhere to the polisher uniformly. Its convexity should be equal to its concavity on the face, that the metal may be every where of an equal thickness. The handle should be made of lead of the same convexity and concavity as the metal, its thickness about double that of the metal, and its diameter three-fourths of that of the speculum; it should have a hole in the middle, with a copper or iron screw on it, so as to put it together with the mirror, to which it is fastened with pitch on a collar lathe, in order to smooth and finish the edge of the metal, which may be done by holding a fine file to it when in the lathe at the first, and afterwards one of the above mentioned stones.

(To be concluded in our next.)

V.

Early Developement of the Antiphlogistian Theory of Combustion. By ROBERT HOOKE.

To Mr. NICHOLSON.

London, January 7, 1800.

SIR,

MUCH pains have with justice been bestowed on the publication and developement of the discoveries of John Mayow; but I think the prior claims of another man, of no less ability, seem to be overlooked. The *Traſtatus quinque Medico-Phyſici* of Mayow were published at Oxford in 1674, and the licence of Lord Brouncker, first president of the Royal Society, for printing Hooke's *Micrographia*, bears date Nov. 23, 1664, which itself appeared in 1675. I send the following extract from page 103 of that work, and have no doubt but it will prove acceptable to the readers of your useful Miscellany.

I am, &c.

R. B.

From the experiment of charring of coals (whereby we see that, notwithstanding the great heat and the duration of it, the solid parts of the wood remain, whilst they are preserved from the free access of the air undissipated) we may learn that which has not, that I know of, been published or hinted, nay, not so much as thought of, by any; and that, in short, is this:

First, *that the air* in which we live, move, and breathe, and which encompasses very many and cherishes most bodies it encompasses, that this air is the *menstruum*, or universal dissolvent of all *sulphureous* bodies.

Secondly,

Secondly, *that this action* it performs not till the body be first sufficiently heated, as we find requisite also to the dissolution of many other bodies by several other menstruums.

Thirdly, *that this action* of dissolution produces or generates a very great heat, and that which we call fire; and this is common also to many dissolutions of other bodies made by menstruums, of which I could give multitudes of instances.

Fourthly, *that this action* is performed with so great a violence, and does so minutely act, and rapidly agitate the smallest parts of the *combustible* matter, that it produces in the *diaphanous medium* of the air the action or pulse of light, which what it is I have elsewhere already shewn.

Fifthly, *that the dissolution* of sulphureous bodies is made by a substance inherent, and mixt with the air that is like it, if not the very same, with that which is fixed in saltpetre, which, by multitudes of experiments that may be made with saltpetre, will, I think, most evidently be demonstrated.

Sixthly, *that in this dissolution* of bodies by the air, a certain part is united, and mixt or dissolved, and turned into the air, and made to fly up and down with it in the same manner as a *metalline*, or other body, dissolved into any *menstruum*, does follow the motions and progresses of that *menstruum* till it be precipitated:

Seventhly, that as there is one part that is dissoluble by the air, so are there other parts, with which the parts of the air mixing and uniting, do make a *coagulum* or precipitation, as one may call it, which causes it to be separated from the air; but this *precipitate* is so light, and in so small, or rarified, or porous clusters, that it is very volatile, and is easily carried up by the motion of the air, though afterwards, when the heat and agitation that kept it rarified ceases, it easily condenses, and commixt with other indissoluble parts, it sticks and adheres to the next bodies it meets withal; and this is a certain salt that may be extracted out of foot.

Eighthly, that many indissoluble parts being very apt and prompt to be rarified, and so whilst they continue in that heat and agitation, are lighter than the ambient air, are thereby thrust and carried upwards with great violence, and by that means carry along with them not only that *saline concrete* I mentioned before, but many terrestrial or indissoluble and irrefragable parts, nay, many parts also which are dissoluble, but are not suffered to stay long enough in a sufficient heat to make them prompt and apt for that action. And therefore we find in foot not only a part that, being continued longer in a competent heat, will be dissolved by the air, or take fire and burn, but a part also which is fixt, terrestrial, and irrefragable.

Ninthly, that as there are these several parts that will rarify and fly, or be driven up by the heat, so are there many others that, as they are indissoluble by the *aerial menstruum*, so are they of such sluggish and gross parts, that they are not easily rarefied by heat, and therefore cannot be raised by it; the volatility or fixedness of a body seeming to consist only in this, that the one is of a texture, or has component parts, that will be easily rarified into the form of air, and to the other that it has such as will not, without much ado, be brought to such a constitution; and this is that part which remains behind in a white body called ashes, which contains a substance or salt which chemists call *alkali*: what the particular natures of each

each of these bodies are, I shall not here examine, intending it in another place, but shall rather add, that this hypothesis does so exactly agree with all phenomena of fire, and so genuinely explicate each particular circumstance that I have hitherto observed, that it is more than probable that this cause which I have assigned is the true, adequate, real, and only cause of these phenomena; and therefore I shall proceed a little further to shew the nature and use of the air.

Tenthly, therefore the dissolving parts of the air are but few that is, it seems, of the nature of those *saline menstrooms* or spirits that have very much phlegm mixed with the spirits, and therefore a small parcel of it is quickly glutted, and will dissolve no more, and therefore unless some fresh part of this menstruum be applied to the body to be dissolved, the action ceases, and the body leaves to be dissolved and to shine, which is the indication of it, though placed or kept in the greatest heat; whereas *saltpetre* is a *menstruum*, when melted and red hot, that abounds more with those dissolvent particles, and therefore as a small quantity of it will dissolve a great sulphureous body, so will the dissolution be very quick and violent.

Therefore, in the eleventh place, it is observable that, as in other solutions, if a copious and quick supply of fresh menstruum, though but weak, be poured on, and applied to the dissoluble body, it quickly consumes it; so this *menstruum* of the air if by bellows, or any other such contrivance, it be copiously applied to the shining body, is found to dissolve it as soon and as violently as the more strong menstruum of melted nitre.

Therefore, twelfthly, it seems reasonable to think that there is no such thing as an element of fire that should attract or draw up the flame, or towards which the flame should endeavour to ascend out of a desire or appetite of uniting with that as its *homogeneous* primitive, and generating element; but that the shining transient body which we call flame is nothing else but a mixture of air and volatile sulphureous parts of dissoluble or combustible bodies, which are acting upon each other whilst they ascend, that is, flame seems to be a mixture of air and the combustible volatile parts of any body, which parts the encompassing air does dissolve or work upon, which action, as it does intend the heat of the *aerial* parts of the dissolvent, so does it thereby further rarify those parts that are acting, or that are very near them, whereby they growing much lighter than the heavy parts of that menstruum that are more remote, are thereby protruded and driven upwards; and this may be easily observed also in dissolutions made by any other *menstruum*, especially such as either create heat or bubbles. Now this action of the *menstruum* or air on the dissoluble parts is made with such violence, or is such that it imparts such a motion or pulse to the *diaphanous* parts of the air, as I have elsewhere shewn is requisite to produce light.

This hypothesis I have endeavoured to raise from an infinite of observations and experiments, the process of which would be much too long to be here inserted, and will perhaps another time afford matter copious enough for a much larger discourse; the air being a subject which (though all the world has hitherto lived and breathed in, and been unconversant about) has yet been so little truly examined or explained, that a diligent enquirer will be able to find but very little information from what has been (till of late) written of it; but being once well understood, it will, I doubt not, enable a man to render an

intelligible, nay probable, if not the true reason of all the *phenomena* of fire, which, as it has been found by writers and philosophers of all ages a matter of no small difficulty, as may be sufficiently understood by their strange *hypotheses*, and unintelligible solutions of some few *phenomena* of it; so will it prove a matter of no small concern and use in human affairs, as I shall elsewhere endeavour to manifest, when I come to shew the use of the air in respiration, and for the preservation of the life; nay, for the conservation and restoration of the health, and natural constitution of mankind, as well as all other aerial animals, as also the uses of this principle, or propriety of the air in chemical, mechanical, and other operations. In this place I have only time to hint an *hypothesis*, which, if God permits me life and opportunity, I may elsewhere prosecute, improve, and publish.

VI.

Experiments and Observations on Shell and Bone. By CHARLES HATCHETT, Esq. F. R. S. *

SOME experiments, which I lately made at the request of Mr. Home, and which he has done me the honor to mention in his ingenious paper on the teeth of granivorous quadrupeds, induced me to turn my attention more particularly to the chemical examination of shell and bone, especially as the former appeared to have been hitherto much neglected.

The time since these experiments were began, has not been sufficient to enable me to enter into all the minutiae of the chemical analysis of these substances; but as some remarkable facts were ascertained, I have now ventured to bring them forward, with the addition of some observations, although as yet the whole is little more than a very imperfect outline.

The first of these experiments were made on the shells of marine animals; and to avoid repetition and prolixity, I shall, in a great measure, once for all, describe the menstrua, the precipitants, and the mode of operation.

When shells were examined they were immersed in acetous acid, or nitric acid diluted, according to circumstances, with 4, 5, 6, or more parts of distilled water; and the solution was always made without heat.

The carbonate of lime was precipitated by the carbonate of ammoniac, or of pot-ash; and phosphate of lime (if present) was previously precipitated by pure or caustic ammoniac.

If any other phosphate like that of soda was suspected, it was discovered by solution of acetite of lead.

Bones and teeth were also subjected to the action of the acetous, or diluted nitric and muriatic acids.

* Philos. Transf. 1799.

The dissolved portion was examined by the above-mentioned precipitants; and in experiments where the quantity of the substance would permit, the phosphoric acid was also separated by nitric or sulphuric acid. The phosphoric acid thus obtained, was proved after concentration by experiments, which being usually employed for such purposes, are too well known to require description.

It is necessary moreover to observe, that as the substances examined were very numerous, and my principal object was to discover the most prominent characters in them, I did not for the present attempt in general, to ascertain minutely the proportions, so much as the number and quality of their respective ingredients.

The greater part, if not all, of marine shells, appear to be of two descriptions in respect to the substance of which they are composed. Those which will be first noticed have a porcellaneous aspect, with an enamelled surface, and when broken, are often in a slight degree, of a fibrous texture.

The shells of the other division have generally, if not always, a strong epidermis, under which is the shell, principally or intirely composed of the substance called *nacre*, or mother of pearl.

Of the porcellaneous shells, various species of *voluta*, *cypræa*, and others of a similar nature were examined.

Of the shells composed of *nacre*, or mother of pearl, I selected the oyster, the river muscle, the *haliotis iris*, and the *turbo olearius*.

Experiments on Porcellaneous Shells.

Shells of this description, when exposed to a red heat in a crucible during about a quarter of an hour, crackled and lost the colours of their enamelled surface; they did not emit any apparent smoke, nor any smell like that of burnt horn, or cartilage. Their figure remained unchanged, excepting a few flaws; and they became of an opaque white, tinged partially with pale grey, but retained part of their original gloss.

The shells which had not been exposed to fire, (whether intire or in powder) dissolved with great effervescence in the various acids; and the solution afterwards remained colourless and transparent.

But the shells which had been burned, upon being dissolved, deposited a very small quantity of animal coal; and thereby the presence of some gluten was denoted, although the proportion was too small to be discovered in the solution of the shells which had not been burned.

The various solutions were filtrated, and were examined by pure ammoniac and acetite of lead; but I never obtained any trace of phosphate of lime, nor of any other combination of phosphoric acid.

The carbonate of lime was afterwards precipitated by carbonate of ammoniac; and from

many experiments it appeared, that porcellaneous shells consist of carbonate of lime, cemented by a very small portion of animal gluten.

Previous to the experiments on shells composed of nacre, or mother of pearl, I examined some patellæ from Madeira.

When these were exposed to a red heat in a crucible, there was a perceptible smell, like that of horn, hair, or feathers.

The proportion of carbonic matter deposited by the subsequent solution, was more considerable than that of the shells above mentioned; and the proportion of carbonate of lime relative to their weight was less.

When the recent shells were immersed in very dilute nitric acid, the epidermis was separated, the whole of the carbonate of lime was dissolved, and a gelatinous substance, nearly liquid, remained, but without retaining the figure of the shell, and without any fibrous appearance.

These shells evidently, therefore, contain a larger portion of a more viscid gelatinous substance, than those before mentioned; but the solution separated from the gelatinous substance afforded nothing but carbonate of lime.

Experiments on Shells composed of Nacre, or Mother of Pearl.

When the shell of the common oyster was exposed to a red heat, the effects were the same as those observed in the patellæ, and the solution of the unburned shell was similar, only the gelatinous part was of a greater consistency.

A species of the river muscle was next subjected to experiment. This when burned in a crucible emitted much smoke, with a strong smell of burned cartilage, or horn; the shell throughout became of a dark grey, and exfoliated. By solution in the acids a large quantity of carbonic matter was separated; and much less of carbonate of lime was obtained from a given weight of the shell, than from those already mentioned.

Upon immersing an unburned shell in dilute nitric acid, a rapid solution and effervescence at first took place, but gradually became less, so that the disengagement of the carbonic acid gas was to be perceived only at intervals.

At the end of two days I found nearly the whole of the carbonate of lime dissolved, but a series of membranes retaining the figure of the shell remained, of which the epidermis constituted the first.

In the beginning the carbonate of lime was readily dissolved, because the acid menstruum had an easy access; but after this it had more difficulty to insinuate itself between the different membranes, and of course the solution of the carbonate of lime was slower.

During the solution the carbonic acid gas was entangled, and retained in many places between the membranes, so as to give to the whole a cellular appearance.

The *halotis iris* and the *turbo olearius* resembled this muscle, excepting that their membranaceous parts were more compact and dense.

These shells, when deprived by an acid menstruum of their hardening substance, or carbonate of lime, appear to be formed of various membranes, applied stratum super stratum.

Each membrane has a corresponding coat or crust of carbonate of lime; which is so situated, that it is always between every two membranes, beginning with the epidermis, and ending with the last formed internal membrane.

The animals which inhabit these stratified shells, increase their habitation by the addition of a stratum of carbonate of lime, secured by a new membrane; and as every additional stratum exceeds in extent that which was previously formed, the shell becomes stronger in proportion as it is enlarged, and the growth and age of the animal becomes denoted by the number of the strata which concur to form the shell.

Although the *halotis iris*, and the *turbo olearius*, are composed of the true mother of pearl, I was induced to repeat the foregoing experiments on some detached pieces of mother of pearl, such as are brought from China.

These experiments I need not describe, as the results were precisely the same.

I must, however, observe, that the membranaceous or cartilaginous parts of these shells, as of the pieces of mother of pearl, retained the exact figure of the shell, or piece which had been immersed in the acid menstruum; and these membranaceous parts distinctly appeared to be composed of fibres placed in a parallel direction, corresponding to the configuration of the shell.

The same experiments were made on pearls; which proved to be similar in composition to the mother of pearl; and so far as their size would enable me to discern, they appeared to be formed by concentric coats of membrane and carbonate of lime; by this structure they much resemble the globular calcareous concretions found at Carlsbad, and other places, called pisolithes.

The wavy appearance, and irrefrascency of mother of pearl, and of pearl, are evidently the effect of their lamellated structure and semi-transparency; in which, in some degree, they are resembled by the lamellated stone, called adularia.

When the experiments on the porcellaneous shells, and on those formed of mother of pearl, are compared, it appears that the porcellaneous shells are composed of carbonate of lime, cemented by a very small portion of gluten; and that mother of pearl, and pearl, do not differ from these, except by a smaller proportion of carbonate of lime; which instead of being simply cemented by animal gluten, is intermixed with, and serves to harden a membranaceous or cartilaginous substance; and this substance, even when deprived of the carbonate of lime, still retains the figure of the shell.

But between these extremes, there will probably be found many gradations; and these we have the greater reason to expect, from the example afforded by the patellæ, which have been lately mentioned.

Some few experiments were made on certain land shells; and in the common garden snail I thought that I discovered some traces of phosphate of lime; but as I did not find

any in the *helix memoralis*, it may be doubted whether the presence of phosphate of lime should be considered as a chemical character of land shells *.

Experiments on the covering Substance of Crustaceous Marine Animals †.

As I was not acquainted with any experiments, by which the chemical nature of the substance which covers crustaceous marine animals had been determined, I was desirous to ascertain in what respect it was different from shell; and I began these experiments on three species of the echinus, with which I had been favoured by the Right Hon. President.

I was the more inclined to begin with the echini, because naturalists do not appear to be perfectly agreed, whether to call them testaceous or crustaceous animals.

Klein, who has written a work upon echini, after having noticed the various opinions of Rondelet, Rumphius, and others, determines that they are to be regarded as testaceous animals. His words are, "Sic plurimas testas marinas, in statu naturali consideratas, cum echinodermatis potius quam cum crustis astacorum vel cancerorum conferre licebit. Itaque echinoderma cum Aristotele qui echinos inter testacea quibus facultas ingrediendi est reponit, nec non cum Belonio, Aldrovando, et excellentissimo Sloanio religiose testam appellamus, quam satis duram in nonnullis offendimus ‡."

But Linnæus was of the contrary opinion, as appears from his definition of the echinus. "Corpus subrotundum *crusta ossis tectum* spinis mobilibus sæpius aspera §."

Now as the experiments above related had proved, that the shells of marine animals were composed of carbonate of lime, without any phosphate, I thought it very possible that the covering of the crustaceous animals might, in some respect, be different; and if so, I should thus by chemical characters, be enabled to ascertain the class to which the echinus was to be referred.

Of the three echini which were examined, one had small spines; the second had large obtuse spires; and the third was of a very flat form.

Portions of these echini were separately immersed in acetous, muriatic, and diluted nitric acid, by each of which they were completely dissolved with much effervescence; depositing at the same time a thin outer skin, or epidermis. The transparency of the solutions was also disturbed by a portion of gluten which remained suspended, and communicated a brownish colour to the liquors.

The solutions in acetous and diluted nitric acid were filtrated; after which, from the acetous solution of each echinus, I obtained a precipitate of phosphate of lead, by the ad-

* Some experiments which I have lately made upon the cuttle bone of the shops have proved, that the term bone is here misapplied; if the presence of phosphate of lime is to be regarded as the characteristic of bone; for this substance in composition is exactly similar to shell, and consists of various membranes hardened by carbonate of lime, without the smallest mixture of phosphate.

† Under this head I have included my experiments upon echini star-fish, crabs, lobsters, &c.

‡ Klein *Naturalis dispositio Echinodermatum*, &c. p. 10.

§ *Systema Naturæ*. Edit. Gmelin, p. 3168.

dition of acetite of lead; and having thus proved the presence of phosphoric acid, I saturated the nitric solutions with pure ammoniac, by which a quantity of phosphate of lime was obtained, much inferior however in quantity to the carbonate of lime, which was afterwards precipitated by carbonate of ammoniac.

The composition of the crust of the echinus is therefore different from that of marine shells; and by the relative proportions and nature of the ingredients, it approaches most nearly to the shells of the eggs of birds; which in like manner consist of carbonate, with a small proportion of phosphate of lime cemented by gluten.

It remained now to examine the composition of those substances which are decidedly called crustaceous; but previous to this some experiments were made on the asterias, or star fish, of which I took the species commonly found on our coasts, and known by the popular name of five fingers (*asterias rubens*.)

The asterias is thus described by Linnæus. “*Corpus depressum, subtus sulcatum; crusta coriacea, tentaculis muricata* *.”

When the asterias was immersed in the acids, a considerable effervescence was produced, and a thin external stratum was dissolved; after which it remained in a perfectly coriaceous state, and complete in respect to the original figure.

The dissolved portion being examined by the usual precipitants, proved to be carbonate of lime, without any mixture of phosphate; but in another species of the asterias, which had twelve rays (*asterias papposa*,) I discovered a small quantity of phosphate of lime. I am therefore induced to suspect that in the different species of the asterias, nature makes an imperfect attempt to form shell on some, and a crustaceous coating on others; and that a series of gradations is thus formed between the testaceous, the crustaceous, and the coriaceous marine animals.

It was now requisite to ascertain if phosphate of lime is a component part of the substance which covers the crustaceous marine, or aquatic animals, such as the crab, lobster, prawn, and crayfish.

Pieces of this substance, taken from various parts of those animals, was at different times immersed in acetous and in diluted nitric acid; those which had been placed in the diluted nitric acid produced a moderate effervescence, and in a short time were found to be soft and elastic, of a yellowish white colour, and like a cartilage, which retained the original figure.

The same effects were produced by acetous acid, but in a less degree; in the latter case also a colouring matter remained, and was soluble in alcohol.

All the solutions, both acetous and nitric, afforded carbonate and phosphate of lime, although the former was in the largest proportion.

There is reason to conclude, therefore, that phosphate of lime, mingled with the carbonate, is a chemical characteristic which distinguishes the crustaceous from the testaceous substances; and that the principal difference in the qualities of each, when complete, is

* *Systema Naturæ*. Ed. Gmelin. p. 3160.

caused by the proportion of the hardening substances relative to the gluten by which they are cemented; or by the abundance and consistency of the gelatinous, membranaceous or cartilagenous substance, in and on which the carbonate of lime, or the mixture of carbonate and phosphate of lime, has been secreted and deposited. Moreover as the presence of phosphate of lime, mingled with carbonate, appears to be a chemical character of crustaceous marine animals, there is every reason to conclude that Linnæus did right not to place the echini among the testaceous ones.

The presence of phosphate of lime in the substance which covers the crustaceous marine animals, appears to denote an approximation to the nature of bone, which, not only by the experiments of Mr. Gahn, but by the united testimony of all chemists, has been proved principally to consist (as far as the ossifying substance is concerned) of phosphate of lime.

This consideration, therefore, induced me to repeat the above experiments on the bones of various animals.

It is scarcely necessary for me to mention the usual effects of acids on bones steeped in them, as they are known to every physiologist and anatomist.

In every operation of this nature, the ossifying substance, which is principally phosphate of lime, is dissolved, and a cartilage or membrane of the figure of the original bone remains; so that the first origin of bones appears to be by the formation of a membrane or cartilage of the requisite figure, which, when the subsequent secretion of the ossifying substance takes place, is penetrated by, and thus becomes more or less converted into the state of bone.

It is also known that the nature of the bone is more influenced by the greater or less predominance of the membranaceous or cartilagenous part than by any other cause. It is not, therefore, for me to add any thing to this part; and in respect to the substance which is the cause of ossification little also requires to be mentioned, for this (as has been already observed) is known principally to consist of phosphate of lime. I shall only therefore briefly mention the result of certain experiments.

(To be continued.)

VII.

Experiments upon the Resistance of Bodies moving in Fluids. By the Rev. SAMUEL VINCE, A. M. F. R. S. Plumian Professor of Astronomy and experimental Philosophy, in the University of Cambridge.*

IN a former paper upon the motion of fluids, I stated the difficulties to which the theory is subject, and shewed its insufficiency to determine the time of emptying vessels, even in the most simple cases; I also proved, by actual experiment, that in many instances there was no agreement between their results, and those deduced from theory. The great

* Philos. Trans. 1798.

difference between the experimental and theoretical conclusions, in most of the cases which respect the times in which vessels empty themselves through pipes, necessarily leads us to suspect the truth of the theory of the action of fluids under all other circumstances. In the doctrine of the resistance of fluids, we see strong reasons to induce us to believe, that the theory cannot generally lead us to any true conclusions. When a body moves in a fluid, its particles strike the body; and in our theoretical considerations after this action, the particles are supposed to produce no further effect, but are conceived to be, as it were, annihilated. But in fact this cannot be the case; and what we are to allow for their effect afterwards, is beyond the reach of mere theoretical investigation. Whatever theory therefore we can admit, must be that which is founded upon such experiments, as include in them every principle which is subject to any degree of uncertainty: we must therefore have recourse to experiments, in order to establish any conclusions upon which we may afterwards reason. In the paper above mentioned, I described a machine to find the resistances of bodies moving in fluids; since which time I have made a variety of experiments with it upon bodies moving both in air and water, and have every reason to be satisfied of its great accuracy. In this paper I propose to examine the resistance which arises from the action of non-elastic fluids upon bodies.

This subject divides itself into two parts; we may consider the action of water at rest upon a body moving in it; or we may consider the action of the water in motion when the body is at rest. We will first give the result of our experiments in the former case, and compare them with the conclusions deduced from theory. Now the radius of the axis of the machine made use of, was 0,2117 inches; the area of the four planes, was 3,73 inches; the distances of their centres of resistance from the axis, was 7,57 inches; and they moved with a velocity of 0,66 feet in a second. The first column of the following table exhibits the angles at which the planes struck the fluid; the second column shews the resistance by experiment, in the direction of their motion in troy ounces: the third column gives the resistance by theory, assuming the perpendicular resistance to be the same as by experiment; the fourth column shews the power of the sine of the angle to which the resistance is proportional.

Angle.	Experiment.	Theory.	Power.
10°	0,0112	0,0012	1,73
20	0,0364	0,0093	1,73
30	0,0769	0,0290	1,54
40	0,1174	0,0616	1,54
50	0,1552	0,1043	1,51
60	0,1902	0,1476	1,38
70	0,2125	0,1926	1,42
80	0,2237	0,2217	2,41
90	0,2321	0,2321	

The

The fourth column was thus computed: let s be the sine of the angle to radius unity, r the resistance at that angle, and suppose r to vary as s^m ; the $r^m : s^m :: 0,2321 : r$, hence $s^m = \frac{r}{0,2321}$, and consequently $m = \frac{\log. r - \log. 0,2321}{\log. s}$; and by substituting for r and s their several corresponding values, we get the respective values of m , which are the numbers in the fourth column. Now the theory supposes the resistance to vary as the cube of the sine; whereas the resistance decreases from an angle of 90° in a less ratio than that, but not as any constant power, nor as any function of the sine and cosine that I have yet discovered. Hence the actual resistance is always greater than that which is deduced from theory, assuming the perpendicular resistance to be the same; the reason of which, in part at least is, that in our theory we neglect the whole of that part of the force, which after resolution acts parallel to the plane; whereas (from the experiments which will be afterwards mentioned) it appears that part of that force acts upon the plane; also the resistance of the fluid which escapes from the plane into the surrounding fluid, may probably tend to increase the actual resistance above that which the theory gives, in which that consideration does not enter; but as this latter circumstance affects the resistance at all angles, and we do not know the quantity of effect which it produces, we cannot say how it may affect the ratio of the resistances at different angles.

In theory the resistance perpendicular to the planes is supposed to be equal to the weight of a column of fluid, whose base = 3,73 inches, and altitude = the space through which a body must fall to acquire the velocity of 0,66 feet. Now that space is 0,08124 inches, consequently the weight of column = 0,1598 troy ounces; but the actual resistance was found to be = 0,2321 ounces. Hence the actual resistance of the planes; the resistance in our theory :: 0,2321 : 0,1598, which is nearly as 3 : 2.

I am aware that experiments have been made upon the resistance of bodies moving in water, which have agreed with our theory. An extensive set was instituted by D'Alembert, Condorcet, and Bossut, the result of which very nearly coincided with theory, so far as regards the absolute quantity of the perpendicular resistance. Their experiments were made upon floating bodies drawn upon the fluid, by a force acting upon them in a direction parallel to the surface of the fluid. There can be no doubt but that these experiments were very accurately made. The experiments here related were also repeated so often, and with so much care, and the results always agreed so nearly, that there can be no doubt but that they give the actual resistance to a very considerable degree of accuracy. In our experiments the planes were immersed at some depth in the fluid; in the other case the bodies floated on the surface; and I can see no way of accounting for the difference of the resistances, but by supposing, that at the surface of the fluid, the fluid from the end of the body may escape more easily than when the body is immersed below the surface; but this I confess appears by no means a satisfactory solution of the difficulty. The resistances of bodies descending in fluids manifestly come under the case of our experiments.

Two semi-globes were next taken, and made to revolve with their flat sides forwards. The diameter of each was 1,1 inches; the distance of the centre of resistance from the axis, was 6,22 inches; and they moved with a velocity of 0,542 feet in a second; and the resistance was found to be 0,08339 ounces by experiment. By theory the resistance is 0,05496 ounces, hence the resistance by experiment: the resistance by theory :: 0,08339 : 0,05496, agreeing very well with the above mentioned proportion. But when the spherical sides moved forwards with the same velocity, the resistance was 0,034 ounces. Hence the resistance on the spherical side of a semi-globe : resistance on its base :: 0,034 : 0,08339; but this is not the proportion of the resistance of a perfect globe, to the resistance of a cylinder of the same diameter, moving with the same velocity, because the resistance depends upon the figure of the back part of the body.

I therefore took two cylinders of the same diameter as the two semi-globes, and of the same weight; and giving them the same velocity, I found the resistance to be 0,07998 ounces; therefore the resistance on the flat side of a semi-globe : the resistance of a cylinder of the same diameter, and moving with the same velocity, :: 0,08339 : 0,07998. This difference can arise only from the action of the fluid on the back side of the semi-globe, moving with its flat side forwards, being less than that on the back of the cylinder, in consequence of which the semi-globe suffered the greater resistance. The resistance of the cylinders thus determined directly by experiment, agrees very well with the foregoing experiments. The resistance *ceteris paribus*, varies as the square of the velocity very nearly, and may be taken so for all practical purposes, as I find by repeated experiments made both upon air and water, in the manner described in my former paper. Hence for the different planes the resistance varies as the area \times the square of the velocity. Now the resistance of the planes whose area was 3,73 inches, moving with a velocity of 0,66 feet in a second, was found to be = 0,2321 ounces. Also the area of the two cylinders was 1,9 inches, and their velocity was 0,542 feet in a second; to find therefore the resistance of the cylinders from that of the planes, we have $0,66^2 \times 3,73 : 0,542^2 \times 1,9 :: 0,2321$ ounces : 0,07973 ounces; for the resistance on the cylinders differing but a very little from 0,07998 ounces, the resistance found from direct experiment.

Now to get the resistance on a perfect globe, we must consider that when the back part is spherical, the resistance is greater than when it is flat in the ratio of 0,08339 : 0,07998; hence the resistance on a globe : the resistance on a semi-globe in the same ratio; but the resistance on the semi-globe was 0,034 ounces; hence 0,07998 : 0,08339 :: 0,034 ounces : 0,0354 ounces, the resistance of a globe, consequently the resistance of a globe : the resistance of a cylinder of the same diameter moving with the same velocity in water, :: 0,0354 : 0,07998 :: 1 : 2,23.

We proceed next to compare the actual resistance of a globe, with the resistance assumed in our theory. In the first place, the absolute quantity of resistance has been found to be greater than that which we use in theory, in the ratio of 0,2321 : 0,1598; but by theory the resistance of the globe : the resistance of the cylinder :: 1 : 2, or as 1,115 : 2,23;

hence by theory we make the resistance of the globe too great in the ratio of 1,115 : 1; and it is too small, from the former consideration, in the ratio of 0,1598 : 0,2321: therefore the actual resistance of the globe : the resistance in theory :: 0,2321 : 0,1598 \times 1,115 : 0,2321 : 0,1782, which is nearly in the ratio of 4 : 3. Thus far we have considered the resistance of bodies moving in a fluid; we come next to consider the action of a fluid in motion upon a body at rest.

A vessel, five feet high, was filled with a fluid, which could be discharged with a stop cock, in a direction parallel to the horizon. The cock being opened, the curve which the stream described was marked out upon a plane set perpendicular to the horizon; and by examining this curve, it was found to be a very accurate parabola, the abscissa of which was 13,85 inc. and the ordinate was 50 inc. hence the latus rectum was 180,5 inc. one-fourth of which is 45,1 inc. which is the space through which a body must fall to acquire the velocity of projection; hence that velocity was 189,6 inc. in a second. And here by the way we may take notice of a remarkable circumstance. The depth of the cock, below the surface of the fluid, was 45,1 inc. hence the velocity of projection was that which a body acquires in falling through a space equal to the whole depth of the fluid, whereas, through a simple orifice the velocity would have been that which is acquired in falling through half the depth; the pipe of the stop cock, therefore, increased the velocity of the fluid in the ratio of $1 : \sqrt{2}$, and gave it the greatest velocity possible; the length of the pipe was 3 inc. and the area of the section 0,045 inc. also the base of the vessel was a square, the side of which was twelve inches.

The area of the section of the pipe may be found very accurately in the following manner: The vessel being kept constantly full, receives the quantity of fluid, run out in any time t'' , and then weigh it, by which we shall be able to get the quantity in cubic inches. Now, if v = the velocity of the fluid when it issues from the pipe, a = the area of the section of the pipe, l = the length of the cylinder of water run out, whose base = a and m = the quantity of fluid discharged in t'' , then $v : l :: 1'' : t''$; hence $l = vt$; but $al = m$; therefore $avt = m$; hence $a = \frac{m}{vt}$. In the present instance $t = 20$ $m = 170,63$ cubic inches $v = 189,6$; hence $a = 0,045$.

Let A B C D (Fig. 1. Plate XXII.) be a solid piece of wood, upon which are fixed two upright pieces rs , tu ; between these a flat lever, eac , is fastened in a perpendicular position on the axis xy , and nicely balanced; and let a be a point directly against the middle of the axis in a line perpendicular to the plane of the lever. This apparatus is placed against the stop cock, at the distance of about one inch, and when the water is let go, let us suppose the centre of the stream to strike the lever perpendicularly at e ; take $ace = ae$, and on the opposite side to that on which the stream acts fasten a fine silk string at c , and bring it over a pulley p , and adjust it in a direction perpendicular to the plane of the lever, and at the end which hangs down fix a scale Q , the weight of which is to be previously determined. All the

the apparatus being thus adjusted, open the stop cock, and let the fluid strike the lever, and put such weight into the scale as will first keep the lever in its perpendicular situation, and that weight with the weight of the scale must be just equivalent to the action of the fluid. Thus we get the perpendicular effect of the water. Now incline the plane of the lever at any angle to the direction of the stream, and adjust the string perpendicular to the plane as before; then put such a weight into the scale as will keep the lever perpendicular to the horizon whilst the fluid acts upon it, and you get that part of the effect of the fluid which acts perpendicular to the plane. In this manner, when the fluid acts oblique to the plane, we get the perpendicular part of the force. The second column of the following table shews this effect by experiment, for every tenth degree of inclination shewn in the first column, and the third column shews the effect by theory from the perpendicular force, supposing it to vary as the sine of inclination.

Angle.	Experiments.			Theory.		
	oz.	dwt.	grs.	oz.	dwt.	grs.
90°	1	17	12	1	17	12
80	1	17	0	1	16	22
70	1	15	12	1	15	6
60	1	12	12	1	12	11
50	1	18	10	1	18	17
40	1	4	10	1	4	2
30	0	18	18	0	18	18
20	0	12	12	0	12	19
10	0	6	4	0	6	12

It appears from hence that the resistance varies as the sine of the angle at which the fluid strikes the plane; the difference between the theory and experiment being only such as may be supposed to arise from the want of accuracy, to which the experiments must necessarily be subject.

Let us now first consider what the whole perpendicular resistance by experiment is when compared with that by theory. Now by theory the resistance is equal to the weight of a column of the fluid, whose base = 0,045 inc. and altitude = 45,1 inc. and the weight of that column is = 1 oz. 1 dwt. 10 grs. Hence resistance by theory : the resistance by experiment :: 1 oz. 1 dwt. 10 grs. : 1 oz. 17 dwts. 12 grs. :: 514 : 900.

In the next place let us examine what is this resistance compared with the resistance of a plane moving in a fluid. We here prove, that the resistance of the fluid in motion acting on the plane at rest : the resistance by theory :: 900 : 514 ; and we have before proved that the resistance by theory : the resistance of a plane body moving in a fluid :: 1598 : 2321 ; hence the resistance of a fluid in motion upon a plane at rest, the resistance of the same plane moving with the same velocity in a fluid at rest :: 900 × 1598 : 514 × 2321 :: 1438200 : 1192954 :: 6 : 5 nearly. Now we know that the actual effect on the plane must be the same in both cases;

cases; and the difference, I conceive, can arise only from the action of the fluid behind the body in the latter case, there being no effect of this kind in the former case. For in respect to the pressure before the body, that will probably be the same in both cases; for there is a pressure of the column of the spouting fluid acting against the particles which strike the body at rest, similar to the action of the fluid before the body upon the particles which strike the body moving in the fluid. Hence the resistance of the planes moving in the fluid with the velocity here given, is diminished about one-fifth part of the whole by the pressure behind the body; but with different velocities this diminution must increase as the velocity increases.

The effect of that part of the force which acted *perpendicular* to the plane being thus established, we proceed next to examine what part of the whole force which acts *parallel* to the plane is effective. To determine which, the axis wv (Fig. 2.) was fixed perpendicular to the plane of the lever $abcd$, and the ends of the axis were conical, and laid in conical holes; and the thread from which the scale was hung was fixed to the edge at e , and acted perpendicular to it, and the weight drew the lever in the direction es , contrary to that in which the fluid tends to move the lever, and it acted at the same perpendicular distance from the axis below as the fluid acted above it. Let xmz be a line parallel to the horizon when the lever is perpendicular to it, and which passes through the centre of the stream, and let xmz be also the direction of that part of the force which acts parallel to the plane. This apparatus being adjusted, the experiments were made for every tenth degree of inclination; and here a circumstance took place for which I can give no satisfactory reason. Having gone through the experiments once, and noted the results, I repeated them; and to my great surprise I found all the second results to be very different from the first. The experiments were therefore repeated again, and the results were still different. Being certain that the experiments were very accurately made each time, I was totally at a loss to conjecture to what circumstance this difference of results was owing. By repeating, however, the experiments, and observing at what point of the line xmz , the centre of the stream, acted, I discovered that the effect varied by varying that point, that it was greatest when the stream struck the lever as near as it could to x ; less when it struck it at the middle m , and least when it struck it as near as it could to z , notwithstanding that the stream acted at the same perpendicular distance from the axis in each case, and the parallel part of the force always acted in the line xmz . At the angles $80^\circ 70^\circ 60^\circ$ the fluid striking as near as it could to the edge z , gave the lever a motion not in the direction xmz , but in the opposite direction zmx , as appeared by taking away the scale. I have therefore marked such results with the sign —, the motion produced being then in a direction opposite to that which ought to have been produced by that part of the force of the stream which acts parallel to the plane of the lever. The forces which are here put down are those which take effect in a direction parallel to the plane of the lever for every tenth degree of inclination; the perpendicular force being 1 oz. 17 dwts. 12 grs.

At

						dwts. grs.	
At 80° incl.	Edge <i>z</i>	-	-	-	-	-	-
	Middle <i>m</i>	-	-	-	-	3	3
	Edge <i>x</i>	-	-	-	-	10	17
At 70° incl.	Edge <i>z</i>	-	-	-	-	-	-
	Middle <i>m</i>	-	-	-	-	6	2
	Edge <i>x</i>	-	-	-	-	11	10
At 60° incl.	Edge <i>z</i>	-	-	-	-	-	-
	Middle <i>m</i>	-	-	-	-	7	9
	Edge <i>x</i>	-	-	-	-	11	22
At 50° incl.	Edge <i>z</i>	-	-	-	-	0	17
	Middle <i>m</i>	-	-	-	-	8	20
	Edge <i>x</i>	-	-	-	-	13	21
At 40° incl.	Edge <i>z</i>	-	-	-	-	1	16
	Middle <i>m</i>	-	-	-	-	8	6
	Edge <i>x</i>	-	-	-	-	13	15
At 30° incl.	Edge <i>z</i>	-	-	-	-	3	20
	Middle <i>m</i>	-	-	-	-	7	2
	Edge <i>x</i>	-	-	-	-	12	15
At 20° incl.	Edge <i>z</i>	-	-	-	-	4	16
	Middle <i>m</i>	-	-	-	-	6	0
	Edge <i>x</i>	-	-	-	-	11	12
At 10° incl.	Middle <i>m</i>	-	-	-	-	5	12

It is a remarkable circumstance, that the effect of the fluid at *z* increased regularly as the angle decreased; for though I did not measure the negative effects, I could plainly perceive that that was the case; whereas the effects at *m* and *x* increased to about the middle of the quadrant, and then decreased. At 10° the obliquity was such, that the section of the stream extended very nearly from one side of the lever to the other.

As it appears by experiment that the velocity of the fluid flowing out of the vessel was equal to the velocity which a body acquires in falling down the altitude of the fluid above the orifice, the square of the velocity must be in proportion to that altitude. To find, therefore, in this case whether the resistance varied as the square of the velocity, I let the water flow perpendicularly against the plane (Fig. 1.) at different depths, and I always found the resistances to be in proportion to the depth, and therefore in proportion to the square of the velocity, agreeing with what takes place when the body moves in the fluid.

VIII.

An Account of a Kettle for boiling inflammable Fluids. In a Letter from THO. P. SMITH to ROBERT PATTERSON.*

Philadelphia, June 14, 1798.

SIR,

WHEN we consider the many unhappy accidents that occur from vessels, containing inflammable fluids, boiling over, and setting fire to buildings, in which manufactures of them are carried on, it must strike us a matter of importance to form a vessel which should be so constructed as to prevent any of those accidents, and yet of so simple a form, as to render it fit for general use. Impressed with these ideas, I take the liberty of offering, for your approbation, the following plan.

Let A B C D (Plate XXI.) represent a large kettle, D E a spout running out at the distance of three or four feet, commencing at D, four or five inches from the brim of the kettle, and the termination of it, E just as high as the brim C. Let the bottom of this spout be covered with wet sponges or rags. Now suppose the kettle to be filled up to D with any fluid, then as soon as it commenced boiling it would rise in the kettle, and in rising but a small perpendicular height would pass considerably up the spout D E: here the liquor would soon cool, and of consequence fall back into the kettle, and the whole subside to its original height. This would occur as often as the fluid rose above D, as the evaporation from the wet sponges or rags would keep D E constantly cool.

It would perhaps be best to pass the spout through the side of the building into the open air, as thereby the evaporation would be increased, and, consequently, the spout kept at a lower temperature; in this case it might be covered.

In case of the fluid to be boiled possessing a very strong elective attraction to caloric, or the matter of heat, the spout might be extended to the width of the diameter of the kettle, or a projecting shelf might be formed all round, lined below with wet sponges or rags.

I remain, dear SIR,

Your's, &c.

Mr. Robert Patterson.

T. P. SMITH.

P.S. In conformity to the wish of the society, I procured a vessel in the form here proposed. I first tried the experiment with water; it boiled very rapidly; but every time the water rose into the spout, it immediately subsided, although the spout had been for some time directly exposed to the heat of one of Lewis's furnaces; I afterwards attempted it with oil, but before the oil boiled, the soldering of the vessel, which was made of tin, melted.

* American Transactions, 1799.

IX.

Letter from Mr. DAVY, Superintendant of the Pneumatic Institution, to Mr. NICHOLSON, on the Nitrous Oxide, or Gaseous Oxide of Azote, on certain Facts relating to Heat and Light, and on the Discovery of the Decomposition of the Carbonate and Sulphate of Ammoniac.

SIR,

SINCE my discovery of the respirability, and extraordinary effects of the gaseous oxide of azote in April, 1799, a great portion of my time has been devoted to experiments on its properties, composition, and mode of action on living beings. These experiments are nearly compleated; but as at least two months will elapse before they can be published, and as some of the facts to which they relate have been made known to the world in Dr. Beddoes's Notice, to prevent dangerous and inconclusive experiments, I beg leave to communicate to the public, by means of your Journal, a short account of the mode in which it has usually been prepared for experiments on respiration. Nitrate of ammoniac perfectly neutralised, and rendered as dry as possible, must be exposed to a heat not below 310, or above 400° of Fahrenheit *. At this temperature it is decomposed into water and gaseous oxide of azote, or as I would rather call it, nitrous oxide.

The gas must be passed through water, and suffered to remain in contact with it at least an hour and an half before it is respired. A sufficient test of its purity is the combustion of sulphur in it, with a vivid rose coloured flame. The same water should be used for receiving it through, and retaining it in different experiments. A pound of dry nitrate of ammoniac properly decomposed, produces rather more than four cubic feet of air. I have found that the nitrous oxide may be likewise procured in a state of great purity, by exposing nitrous gas to dry sulphite of pot-ash. 1 of nitrous gas decomposed in this manner, produces nearly 0,5 of nitrous oxide.

The solution of metals in dilute nitrous acid never produces air sufficiently pure for respiration, and the decomposition of nitrous gas by the sulphures, wetted iron, &c. is a process too slow ever to be employed with advantage.

My investigation of the nature, properties, &c. of the nitrous oxide, and the aeriform fluids relating to it, will consist,

1. Of experiments on its production, from the decomposition of nitrous acid and nitrous gas in different modes, its analysis, and the analysis of the substances connected with it.
2. On its action on different incombustible substances, on the combustion of charcoal,

* This decomposition was discovered by the illustrious Berthollet. I have found that at a temperature above 500°, nitrous gas and nitrogen are evolved as well as nitrous oxide. Whenever there is a luminous appearance in the retort, more or less of these two substances will be produced.

fulphur, iron, phosphorus, and hydrogen in it, on its decomposition by the compound combustible bodies, &c.

3. On its absorption in respiration, with a general investigation of this process, and the changes effected in different gases by it.

4. A history of its effects, containing the experience of different individuals who have respired it, furnished by themselves. Every day we gain new evidences in favor of its powers. A number of persons have breathed it since the publication of Dr. Beddoes's Notice; all have been affected, and by far the greater number pleasantly. As yet we have tried it in no disease, except palsy; but as it supplies to the system two principles so essential to perceptive existence, as oxygen and nitrogen, and increases the powers of life generally without producing any ascertainable exhaustion; there is reason to hope that it will be a powerful agent in many diseases of debility.

An experiment on the collision of flint and steel in vacuo, and in carbonic acid, published in my Essay on Heat and Light*, differs considerably in its results from a similar one made long ago by the ingenious Mr. Hawksbee†. In repeating my experiment a number of times under new circumstances, I have discovered the cause of this difference; when the gunlock is snapped in carbonic acid, or in water, if a sharp and thin flint be made use of, and the springs be strong, a faint red light is generally perceptible: but if the flint be thick, and sufficiently sharp only to strike off particles from the steel, without suffering considerable abrasion of its own parts; though vivid sparks are produced in the atmosphere, not the slightest luminous appearance is perceptible in carbonic acid. If instead of flint, fluor spar, phosphate of lime, or sugar, be rubbed pretty briskly against steel in carbonic acid, as much light is produced as when they are rubbed against it in common air. These facts induce me to believe, that whenever light is produced by the collision of flint and steel in carbonic acid, it arises from the collision of small particles of flint against each other, and never from the ignition of steel. I have often examined in a microscope, the small particles of steel struck off by flint in carbonic acid; the edges are generally deprived of metallic lustre, and exhibit an appearance of fusion; it is probable, however, that this appearance is owing to a partial oxidation of the particle, from the decomposition of the water held in solution by carbonic acid. In Mr. Hawksbee's experiments, the apparatus was contrived in such a way, as to produce perpetual abrasion of particles of flint. In my experiment the flint was not sufficiently thin to produce light in carbonic acid, consequently it ought to have produced none in vacuo; but if (as the facts I am about to detail render probable) the light produced by flint, fluor spar, phosphate of lime, &c. on collision be electrical, there are strong reasons for believing, that in a Torricellian vacuum, or in a vacuum formed by the absorption of carbonic acid, by a solution of pot-ash sufficiently concentrated to form a solid compound with it, no light would be visible.

* West Country Contributions.

† Phil. Trans. No. XXIV. 2185. (or Hawksbee's Physico-mechan. Exper. 2d edit. octavo, London, 1719, page 26.)

Mr. T. Wedgwood found that fluor spar, phosphate of lime, flint, &c. were luminous, not only when heated, but when struck together, and that under water, or in any kind of air. Scheele discovered that fluor spar, after calcination, lost its phosphorescence. I exposed to a long continued red heat fluor spar, phosphate of lime, sulphate of strontian, sulphate of barytes, glass, sulphate of lime, and carbonate of lime, all of which were before phosphorescent, and produced light by collision under water.

After being suffered to cool in the light, they were placed on a heated iron successively. The fluor spar, the phosphate of lime, and the sulphate of strontian and barytes, were not phosphoric at any temperature. The calcareous spar, which had lost a portion of its carbonic acid, and the gypsum, were nearly as luminous as before; the phosphorescence of the glass and flint was barely perceptible.

Two pieces of the calcined fluor were now rubbed together, they produced as much light as before. The phosphate of lime, the calcareous spar, and the gypsum, had lost their coherence; so that the pieces could not be rubbed against each other with sufficient force. The sulphate of strontian and barytes, the glass, and the flint, produced as much light as before on collision.

I found that all these bodies were non-conductors of electric fluid. On rubbing a large crystal of quartz with woollen, it became highly electric. Fluor spar was likewise made electric when heated and strongly rubbed. To prove, however, more satisfactorily whether the light produced by the collision of two non-conductors was electric, two cylinders of glass were struck against each other, so as to produce light, and one of them was placed in contact with a Leyden phial. After a number of collisions, on applying a conductor to the phial, I procured a small spark.

Two pieces of pyrites, sufficiently hard to cut glass, and extremely brittle, produced an immense quantity of light, when struck together, in the atmosphere; but not the slightest luminous appearance under water. This body is a good conductor of electric fluid. Do not these facts go far to prove that light, when produced by the collision of bodies in water, or non-respirable air, is electric; and generated by the rapid transmission of electrical fluid, excited by collision between two non-conducting surfaces, to a conducting body? And as iron can be heated, to a degree at which it is capable of decomposing oxygen gas, in a non-respirable air without being luminous; and as pyrites is not luminous under water, is it not probable that light is accidental to, and not necessarily produced by, high temperature?

The admission of such inferences would be favourable to my theory of the combinations of light; but facts have occurred to me with regard to the decomposition of bodies which I have supposed to contain light, without any luminous appearance. Till I have satisfactorily explained these facts by new experiments, I beg to be considered as a sceptic with regard to my own particular theory of the combinations of light and theories of light in general. On account of this scepticism, and for other reasons, I shall in future use the common nomenclature; excepting that as my discoveries concerning the gaseous oxide would render it highly

improper to call a principle, which in one of its combinations, is capable of being absorbed by the venous blood, and of increasing the powers of life, azote, I shall name it, with Dr. Pearson, Mr. Chaptal, &c. nitrogene, and the gaseous oxide of azote, the properties and composition of which have been misunderstood by the chemists who gave it that name, nitrous oxide.

Many months ago I made a number of experiments on the composition, analysis, and decomposition of the ammoniacal salts. These experiments have afforded me curious and interesting results; but a wish to complete the investigation relating to the nitrous oxide have prevented me from pursuing them to their full extent. Among these results, as affording useful, practical applications, I shall mention the decomposition of the carbonate and sulphate of ammonia. Carbonate of ammonia undergoes a change in its composition with every change of its temperature; on being heated it gives out carbonic acid, and when cooled absorbs it again; when passed through a tube heated red, it is decomposed into water, charcoal, nitrogene, and hydro-carbonate. Sulphate of ammonia, the partial decomposition of which was discovered by Mr. Hatchet, when sent through a tube heated red, is decomposed into sulphur, water, and nitrogene.

I remain, SIR,

Your's, with much respect,

H. DAVY.

X.

Additional Remarks on the Hygrometer and Photometer. By Mr. JOHN LESLIE.

TO MR. NICHOLSON.

SIR,

LOOKING over the notice of the hygrometer and photometer in your Journal, I perceive a few typographical errors, particularly in the punctuation, but which an attentive reader may easily correct. There is a mistake, however, in the drawing, which it would be proper to rectify: the shorter branch of the instrument should occupy the centre of the socket, in order that the two balls may stand in the middle or axis of the cylindrical case.

I forgot to remark that, in frosty weather, when the lower ball is enveloped with a congealed crust, the same degrees of dryness in the air will be marked by somewhat larger spaces on the scale of the hygrometer. For in that case evaporation has a double office to perform, converting the film of ice successively into the liquid and into the aeriform state; the former process expends 75° centigrade of heat, and the latter 524° , and hence the size of the corresponding divisions are to be conceived augmented in the proportion of 75×524 , to 524 , or that of 8 to 7: that is, to obtain the real dryness in time of frost, one-eighth part must be

deducted

deducted from the number of degrees indicated on the scale. It may not be superfluous to observe that, on exposing the humid coat to freeze, the liquor should be depressed to the lowest point, by touching the upper ball with the hand, else, during the act of congelation, the instrument is liable to be deranged.

By inserting these additional remarks in your next number, you will much oblige

SIR, your most obedient servant,

January 6, 1800.

JOHN LESLIE.

P.S. An easy and expeditious method has since occurred for obtaining, with remarkable accuracy, the graduation of the scale after the compound tube is bent. To the bottom of a small faucer fasten, with soft cement, the instruments to be graduated, disposed at intervals, the standard one in the centre, in a vertical position, with temporary scales affixed, and prepared to act as hygrometers. Set this faucer on a smooth earthen plate: then take a tall glass receiver, and pour into it some concentrated sulphuric acid, and keep turning it round and gradually inclining it till the whole of the inside is completely moistened. In this state, invert the receiver over the instruments, and let it stand on the plate, putting at the same time a few drops of the acid about the rim. The air thus included will quickly acquire a dryness of 50 or 60 degrees, which will continue without any visible alteration for a quarter, or perhaps half, an hour. The relative proportions of the different scales may therefore be noted with the utmost precision. With two nice correspondent thermometers, one of them having its bulb covered with wet bibulous paper, the standard instrument could first be constructed. By attending to those directions, I should presume that even a common artist might qualify himself to prepare the instruments. I will only add, that for the hygrometer it is preferable to blow both balls of colourless glass, and cover the lower one with a bit of gold-beater's skin.

XI.

An Account of some Experiments on the Fecundation of Vegetables. By THOMAS ANDREW KNIGHT, Esq.

(Concluded from page 460.)

IN the course of the preceding experiments I could never observe, that the character either of the male or female in this plant at all preponderated in the offspring; but as this point appeared interesting, I made a few trials to ascertain it. And as the foregoing observations had occurred in my experiments, made principally to obtain new and improved varieties of the pea for garden culture, I chose for a similar purpose the more hardy varieties, usually sown in the fields. By introducing the farina of the largest and most luxuriant kinds into the blossoms of the most diminutive, and by reversing this process, I found

that the powers of the male and female, in their effects on the offspring, are exactly equal. The vigour of the growth, the size of the seeds produced, and the season of maturity were the same, though the one was a very early, and the other a late variety. I had in this experiment, a striking instance of the stimulative effects of crossing the breeds; for the smallest variety, whose height rarely exceeded two feet, was increased to six feet; whilst the height of the large and luxuriant kind was very little diminished. By this process it is evident, that any number of new varieties may be obtained; and it is highly probable, that many of these will be found better calculated to correct the defects of different soils and situations, than any we have at present; for I imagine that all we now possess have in a great manner been the produce of accident; and it will rarely happen in this, or any other case, that accident has done all that art will be found able to accomplish.

The success of my endeavours to produce improved varieties of the pea, induced me to try some experiments on wheat; but these did not succeed to my expectations. I readily obtained as many varieties as I wished, by merely sowing the different kinds together; for the structure of the blossom of this plant (unlike that of the pea) freely admits the ingress of adventitious farina, and is thence very liable to shoot in varieties. Some of those I obtained were excellent; others very bad; and none of them permanent. By separating the best varieties, a most abundant crop was produced; but its quality was not quite equal to the quantity, and all the discarded varieties again made their appearance. It appeared to me an extraordinary circumstance, that in the years 1795 and 1796, when almost the whole crop of corn in the island was blighted, the varieties thus obtained, and these only, escaped in this neighbourhood, though sown in several different soils and situations.

My success on the apple (as far as long experience and attention have enabled me to judge, from the cultivated appearance of trees which have not yet borne fruit) has been fully equal to my hopes. But as the improvement of this fruit was the first object of attention, no probable means of improvement, either from soil or aspect, were neglected. The plants, however, which I obtained from my efforts to unite the good qualities of two kinds of apple, seem to possess the greatest health and luxuriance of growth, as well as the most promising appearance in other respects. In some of these the character of the male appears to prevail; in others that of the female; and in others both appear blended, or neither is distinguishable. These variations, which were often observable in the seeds taken from a single apple, evidently arise from the want of permanence in the character of this fruit, when raised from seed.

The results of similar experiments on another fruit, the grape, were nearly the same as those on the apple, except that by mingling the farina of a black and white grape, just as the blossoms of the latter were expanding, I sometimes obtained plants from the same berry so dissimilar, that I had good reason to believe them the produce of superfœtation. By taking off the cups, and destroying the immature small parts (as in the pea) I perfectly succeeded in combining the characters of different varieties of this fruit, as far as the changes of form and autumnal tints in the leaves of the offspring will allow me to judge.

Many

Many experiments of the same kind were tried on other plants; but it is sufficient to say, that all tended to evince, that improved varieties of every fruit and esculent plants may be obtained by this process, and that nature intended that a sexual intercourse should take place between neighbouring plants of the same species. The probability of this will, I think, be apparent, when we take a view of the variety of methods which nature has taken to disperse the farina, even of those plants in which it has placed the male and female parts within the same empalement. It is often scattered by an elastic exertion of the filaments, which support it on the first opening of the blossom, and its excessive lightness renders it capable of being carried to a great distance by the wind. Its position within the blossom is generally well adapted to place it on the bodies of insects; and the villous coat of the numerous family of bees is not less well calculated to carry it. I have frequently observed with great pleasure, the dispersion of the farina of some of the grasses, when the sun had just risen in a dewy morning. It seemed to be impelled from the plant with considerable force; and being blue was easily visible, and very strongly resembled in appearance the explosion of a grain of gunpowder. An examination of the structure of the blossoms of many plants, will immediately point out, that nature has something more in view than that its own proper males should fecundate each blossom; for the means it employs are always those best calculated to answer the intended purpose. But the farina is often so placed, that it can never reach the summit of the pointal, unless by adventitious means; and many trials have convinced me, that it has no action on any other part of it. In promoting this sexual intercourse between neighbouring plants of the same species, nature appears to me to have an important purpose in view: for independent of its stimulative power, this intercourse certainly tends to confine within more narrow limits, those variations which accidental richness, or poverty of soil usually produces. It may be objected, by those who admit the existence of vegetable mules, that under this extensive intercourse these must have been more numerous; but my total want of success in many endeavours to produce a single mule plant, makes me much disposed to believe, that hybrid plants have been mistaken for mules; and to doubt (with all the deference I feel for the opinions of Linnæus and his illustrious followers) whether nature ever did, or ever will permit the production of such a monster. The existence of numerous mules in the animal world between kindred species is allowed; but nature has here guarded against their production, by impelling every animal to seek its proper mate; and amongst the feathered tribe, when from perversion of appetite sexual intercourse takes place between those of distinct genera*, it has in some instances, at least, rendered the death of the female the inevitable consequence. But in the vegetable world there is not any thing to direct the male to its proper female: its farina is carried by winds and insects to plants of every different genus and species; and it therefore appears to me (as vegetable mules certainly are not common) that nature has not permitted them to exist at all.

* This is said to be the case with the drake and the hen.

I cannot difmifs this fubject without expreffing my regret that thofe who have made the fcience of botany their ftudy, fhould have confidered the improvement of thofe vegetables which, in their cultivated ftate, afford the largeft portion of fubfiftence to mankind and other animals, as little connected with the object of their purfuit. Hence it has happened that whilft much attention has been paid to the improvement of every fpecies of ufeful animal, the moft valuable efculent plants have been almoft wholly neglected. But when the extent of the benefit which would arife to the agriculture of the country from the poffeffion of varieties of plants which, with the fame extent of foil and labour, would afford even a fmall increafe of produce, is confidered, this fubject appears of no inconfiderable importance. The improvement of animals is attended with much expence, and the improved kinds neceffarily extend themfelves flowly; but a fingle bufhel of improved wheat or peas may, in ten years, be made to afford feed enough to fupply the whole ifland; and a fingle apple, or other fruit tree, may within the fame time be extended to every garden in it. Thefe confiderations have been the caufe of my addreffing the foregoing obfervations to you at this time; for it was much my wifh to have afcertained before I wrote to you, whether in any inftance a fingle plant can be the offspring of two male parents. The decifion of that queftion muft of neceffity have occupied two years, and muft therefore be left to the teft of future experiment.

PHILOSOPHICAL NEWS, &c.

On the Soldering of Glafs.

CIT. Pajot des Charmes, Correfpondent with the Philomathic Society, has laid before this Society, and the Inftitute, fome fmall pieces of glafs compofed of feveral fragments, which he has contrived to join and folder together with fuch firmnefs, that the glafs will rather break on the fide of the joint, than exactly upon it. This operation is equally effectual on fuch pieces as have their fractures either ftrait or winding, fquare or bevel, ftarred, &c. The part where the joint is, is fcarcely vifible, and Cit. Pajot has in fome inftances made it almoft entirely difappear. When it is vifible it merely prefents a fimple thread, which does not break the luminous rays as a crack does.

By means of this procefs, if brought to perfection, a glafs of confiderable value may be obtained, by uniting the fmall pieces with little expence. As in order to compleat the union it is neceffary to heat the glafs, and to laminate it, thefe operations have the advantage of altering the colour of a glafs of a difagreeable tint, and of caufing a great part of the bubbles which diffigure it to difappear, by making them take an oblong form, which renders

renders them less visible. Lastly, it is possible by this lamination, to increase the size of a glass when it is of sufficient thickness.

Cit. Pajot has not made public the process he has followed in his operations *.

Bulletin des Science, Brumaire, An. 8.

Simple Method of obviating the Resistance of the Air against the Pendulums of Clocks.

The celebrated David Rittenhouse, late president of the American Philosophical Society, has, in the fourth volume of their transactions, described a simple method of removing the effects of the resistance of the air upon the isochronism of pendulums. He remarks, that clocks of this construction have been made to measure time with astonishing accuracy; and if there be still some causes of inequality in their motions, the united efforts of mechanism, philosophy, and mathematics, will probably in time remove them. That the last and least of these causes which perhaps may be worthy of notice, when all others of more importance are removed, is that arising from unequal density of the air, which, by varying the actual weight of the pendulum, must accelerate or retard its motion. The extreme difference he estimates at half a second a day. He observes, that a remedy dependant on the barometer will not be strictly accurate, as the weight of the entire column does not precisely correspond with the density of its base. The simple method he proposes is as follows:

The pendulum consists of an inflexible rod, carrying the ball beneath and continued above the centre of suspension to an equal (or an unequal) distance upwards. At this extremity is fixed another ball of the same dimensions (or greater or less, according as the continuation is shorter or longer) but made as light as possible. The oscillations of this upper ball will be accelerated by its buoyancy by the same quantity as those of the lower would be retarded, and thus, by a proper adjustment, the two effects might be made to balance and correct each other.

Mr. Rittenhouse, with his usual candour, observes, that he has no doubt but that, notwithstanding the plausibility of this theory, there will be some difficulties in the practice. But he is persuaded they will not prove insuperable.

* Cit. Swediaur has informed the society that a citizen, of the name of Hollenwegar, had made about 12 or 14 years before, in the presence of Lavoisier, Meunier, and himself, some experiments, by means of which he soldered together, in a firm and almost imperceptible manner, some fragments of cast glass.

Cit. Chaptal also observes, that more than 15 years ago, a decanter of crystal glass was presented to him, the stopper of which was so perfectly united with the neck, that it might be cut into plates, without the line or circle of union being sensibly perceived. The fluid it contained was the *liquor filicum*, and the decanter had remained reversed for a considerable time:

From this fact he conceived the possibility of soldering together two plates of glass, and he offered my ideas on this subject, and shewed the decanter to the audience in a public lecture, and I think by gradually abstracting the dissolving alkali, it might perhaps be possible to unite plates of glass by the precipitated flux. NOTE OF THE EDITOR.

I have lately seen some pieces of glass, of different colours, connected or soldered together in plates by an artist in London. The junctures were excessively neat, some of them in right lines, and some waved; from which last fact I conclude that the cement was capable of supporting ignition and bending of the glass.---W.N.

The

The only experiment he made on the subject, was merely to shew that a pendulum can be made in this manner, which shall vibrate quicker in a dense medium, than in one more rare, contrary to what takes place with common pendulums.

He made a compound pendulum on the above principles, of about one foot in its whole length. This pendulum, on many trials, made in the air 57 vibrations in a minute. On immersing the whole in water, it made 59 vibrations in the same time; shewing evidently, that its returns were quicker in so dense a medium as water, than in the air. When the lower bob or pendulum only was plunged in water, it made no more than 44 vibrations in a minute.

Distillation by the Application of the cooling Mixture.

Mr. C. Wistar, who in the third volume of the American Transactions described the production of a visible vapor from ice, at the melting point, suspended in air reduced to the temperature of 0 Fahrenheit; which he considered as the consequence of a general law of nature, has made some more experiments in illustration of the same law*.

The inference, or law which he drew from those facts was, that the non-elastic vapor does not depend upon any positive quantity or degree of sensible heat in the evaporating body, but upon a relative degree, exceeding that of the atmosphere to which it is exposed; and that it is produced by the passage of heat from the moist body into the contiguous air. If this theory be true, he states as a consequence, that a slow distillation may be performed with the common apparatus by applying cold to the receiver or refrigeratory, without increasing the heat of the retort, or substance to be distilled, as there will be a continual passage of heat from the body, to be evaporated or distilled into the air of the receiver.

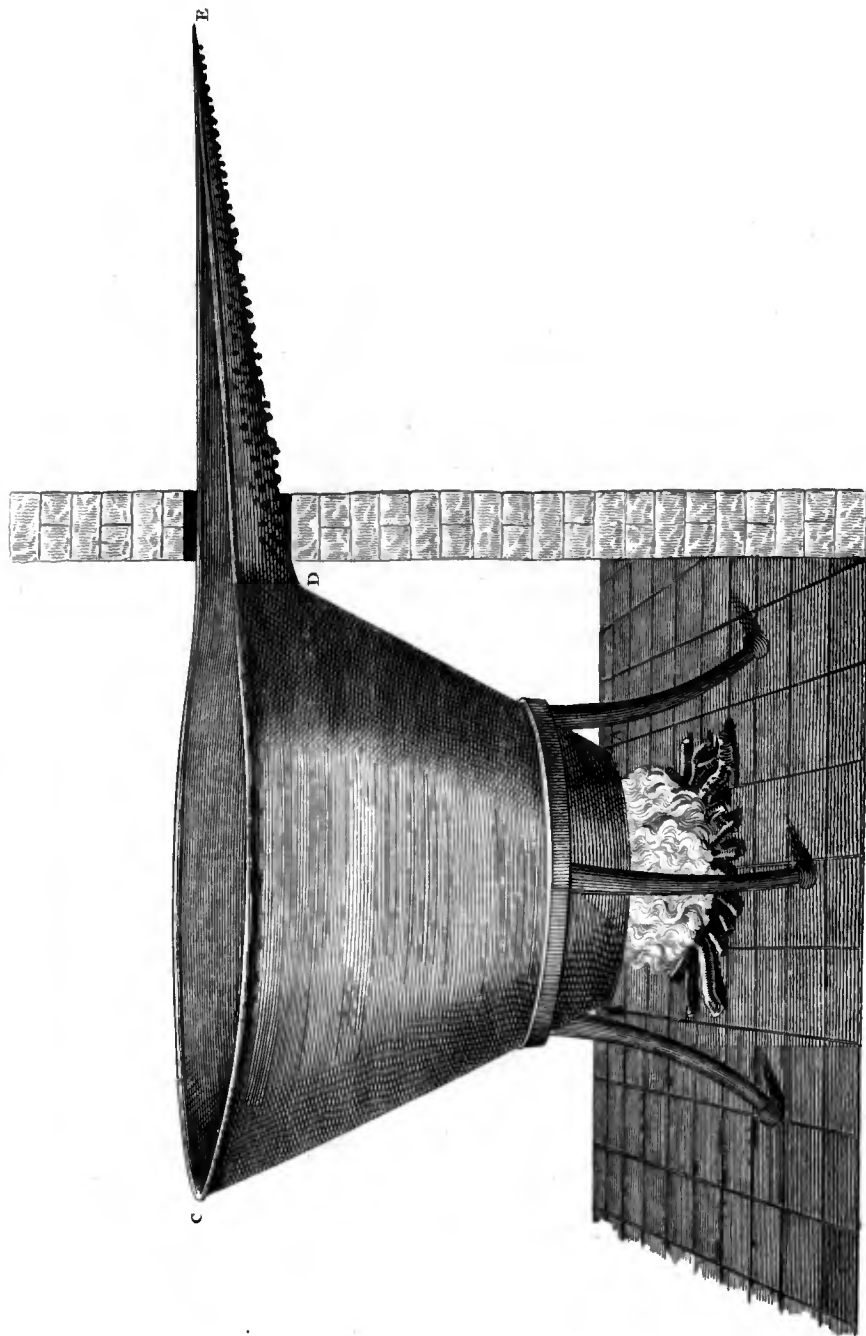
He submitted this conclusion to the test of experiment, because it had been suggested, that the vapor which appeared to arise from ice, might have arisen from the mixture of different portions of air of different temperatures; whereas by distilling and evaporating in a luted retort and receiver, there can be no mixture of warm and cold air; and by using a substance which is not contained in the atmosphere, all suspicion must be removed of the vapor itself having arisen from the included air.

With views of this kind he poured an ounce and half of sulphuric ether into a retort, and luted it to a receiver with a long neck, which was placed in a mixture of salt and snow, while the retort itself was surrounded by the common air at the temperature of 50° Fahrenheit. The frigorific mixture was seldom lower than 10°, so that the difference between the temperatures of the two vessels did not exceed 40°. When the apparatus had been thirty hours in this situation, the frigorific mixture was removed, and one third of the ether was found distilled into the receiver. Another perfectly similar apparatus and charge, was exposed to the air at 50° for thirty hours, but without any application of the freezing mixture. No distillation took place.

The former experiment was varied, by substituting camphor in the place of the ether. The retort stood in air at 50°, and the receiver in the freezing mixture. At the end of thirty hours, some of the camphor was found to be sublimed in the same arborescent form as it usually has when produced by heat.

* American Philos. Transf. IV. 72.

Apparatus for boiling inflammable Fluids.



Maddox sc.



Resistance of Fluids.

Fig. 1.

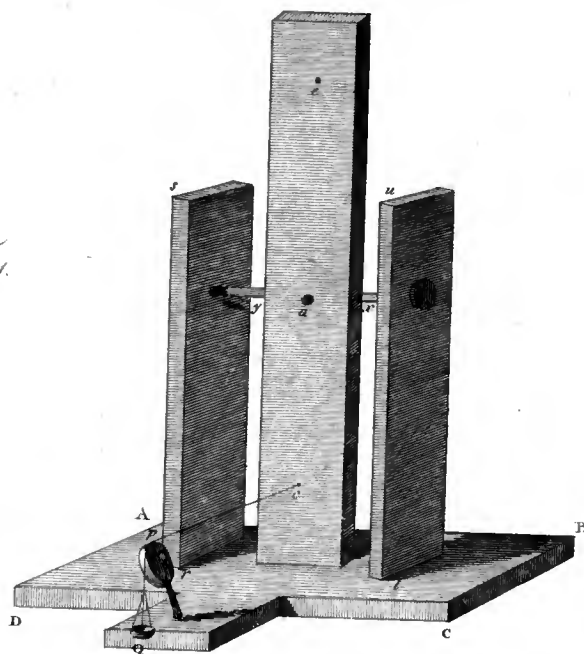
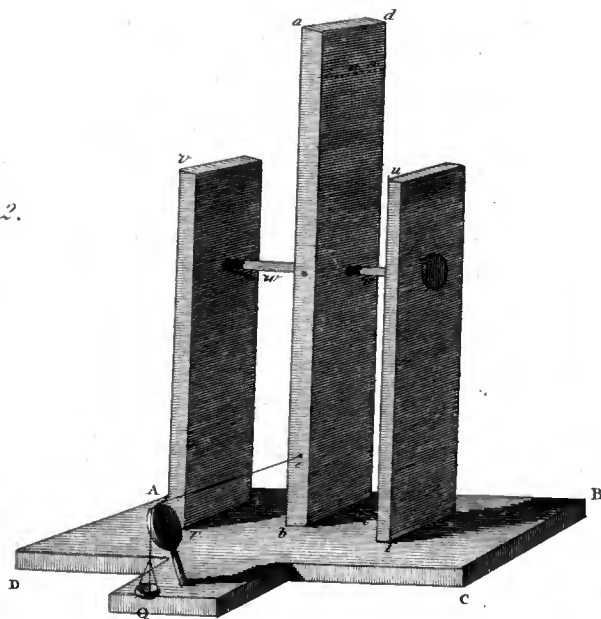


Fig. 2.





A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

MARCH 1800.

ARTICLE I.

Directions for making the best Composition for the Metals of Reflecting Telescopes, and the Method of Casting, Grinding, Polishing, and giving the great Speculum the true parabolic Figure. By the Rev. JOHN EDWARDS, B. A.

(Concluded from page 497.)

Of polishing the Metal, and giving it the true parabolic Figure.

THE rough grinder, of an *elliptical* form, is now to be covered with common pitch. I generally make my own pitch by boiling tar in a ladle or crucible over a very slow fire, till it becomes of the consistence I require; for a greater nicety is required in the degree of the hardness of the pitch. The harder the pitch is the better figure it will give to the metal, as it does not alter its figure in working as soft pitch does; besides the metal will acquire a lustre upon a polisher moderately hard, so as to shew objects reflected upon it as vivid and as near their natural colour as possible; but if the pitch is too soft, some of its finest particles will always adhere to the face of the metal, and form a very fine and thin cuticle or covering upon its surface. This circumstance is rendered very evident by viewing any white object in the metal (a sheet of white paper for example) when that fine cuticle or thin surface of the pitch upon the speculum will cause it to shew the object of a dingy brown colour,

and not of its genuine whiteness. Pitch may be easily made harder by adding a proper quantity of rosin. I often use equal quantities of pitch and rosin, so as to make the mixture just so hard, when cold, as to receive an impression from a moderate pressure of my nail. A polisher made with pitch and rosin has this advantage, viz. though it is hard, yet it is not so brittle as when pitch only is used, and made hard by boiling it, and consequently not so liable to break or chip off at the edges, and thereby scratch the metal. Pour the melted pitch and rosin when pretty cool from the crucible upon the elliptic tool,* so as to cover it every where when spread upon it with an iron spatula, about the thickness of a half-crown piece. If the covering is too thin, it will continually alter its figure by the heat it acquires in working the metal upon it, and thereby give a bad figure also to the speculum. When it is somewhat cool, lay a piece of writing paper upon the surface of the pitch, press the face of the mirror upon it, and you will find the polisher will be nearly figured to the form of the speculum. If it has not taken an exact figure every where, which would appear by the fine marks of the grain of the paper upon the pitch, gently warm the surface of the pitch, and repeat the operation as before, until you have formed it of the exact † figure of the metal. With a penknife take away now all the superfluous pitch from the edge of the polisher, and with a conical piece of wood form the hole in the middle accurately round: in other words, let the pitchy surface be every where of the exact size and shape as the lead tool, which is under it. It may be necessary to mention, that the hole in the middle of the polisher should go quite through the tool, (for a particular reason) and should be made of the same size, or somewhat less, than the hole in the middle of the speculum. This is a necessary caution, and indeed I have always found that small mirrors, without any hole in the middle, will polish much better, and the figure will be more correct, if the polisher has a hole in the middle of it. The powder I prefer above all others, to give a most exquisite lustre, is *colcothar of vitriol*, and not *putty*. *Putty* gives metals a white lustre, or as workmen call it, a silver hue; but good *colcothar of vitriol* will polish with a very fine and high black lustre, so as to give the metal finished with it the complexion of polished steel. To know if the *colcothar of vitriol* is good, put some of it into your mouth, and if you find it dissolves away it is good; but if you find it hard, and crunch between your teeth, then it is bad, and not well burned. Good *colcothar of vitriol* is of a deep red, or a deep purple colour, and is soft and oily when rubbed between the fingers; bad *colcothar of vitriol* is of a light red colour, and feels harsh and gritty. The *colcothar of vitriol* should be levigated between two surfaces of polished steel, and wrought with a little water; when it is worked dry,

* The elliptic tool must be made pretty warm, or the pitch will not adhere to it.

† When the polisher is brought to its true figure, gently warm it at the fire, and with the edge of a knife divide it into several squares, by pressing the edge of the knife gently upon the pitch; these squares, by receiving the small portion of the metal that works off it in polishing, will cause the figure of the speculum to be more correct, than if no such squares had been made. The polisher may also be formed without the writing paper, by dipping the mirror into cold water, and afterwards pressing it upon the surface of the pitch, (when it is somewhat cool) and by repeating this operation till it has taken the exact figure of the metal.

you may add a little more water, to carry it lower down to what degree you please. When the colcothar of vitriol has been wrought dry three or four times, it will acquire a black colour, and will be low enough, or sufficiently fine, to give an exquisite lustre. This levigated colcothar of vitriol I put in a small phial, and pour some water upon it, and afterwards I use it for polishing the metals, in the same manner that washed putty is always directed to be made use of for that purpose. I always put on a large quantity of washed colcothar of vitriol at once, so as to saturate the pitch, and form a fine coating of the colcothar, and very rarely make use of a second application. If a second or third application of colcothar should be found necessary to bring the metal to a high lustre, or to take out any scratches upon its face, use it very sparingly, or you will destroy the polish you have already attained. When the metal is nearly polished, it will always generate some black mud upon the surface of the mirror, and also upon the tool. Wipe it now away from the face of the metal with some very soft wash leather; though if too much of this mud be taken away, it will not polish so well. Indeed, a little experience in these matters will better suffice, than a volume written upon the subject.

In regard to the *parabolic figure* to be given to the metal, no particular caution is required in the polishing; the *elliptical tool* will always cause the speculum to work into an accurate parabolical figure, supposing the transverse and conjugate diameters bear the true proportion to each other, and the metal is not too thick to prevent it always from adhering firmly and uniformly to the polisher. Should the pitch prove too soft, it will give way and alter the figure a little. This circumstance will render the figure of the mirror sometimes a small degree short of the parabola, and sometimes a very little beyond it; but by a little perseverance the correct figure is very easily acquired. I could very easily give the reader the reason, why an *elliptical tool* of a proper proportion will always give a parabolical figure; and if the transverse diameter is increased, it will then always give an hyperbolical figure; but as I am writing upon the practical part of making reflecting telescopes, and not the theory, I will not offend his patience. To convince any one of the certainty of my assertions, let him polish a metal of $2\frac{1}{2}$ inches diameter, and $9\frac{1}{2}$ inches focus, upon an *elliptical tool*, whose diameters are $2\frac{1}{2}$ and 3 inches; and I can assert, he will always find the metal, when polished, (if it is not too thick) beyond the parabola, or it will always prove hyperbolical. If he polishes it upon a *circular tool* in the common way, with cross strokes in every direction possible, using first a few round strokes every time he changes his position, he will find it will always prove spherical, and consequently *short* of the parabola. A very little experience in these matters, will convince any one of the ease and certainty of giving the great speculum a parabolic figure, by polishing it in a common manner only with cross strokes in every possible direction, upon an *elliptical tool* of the proper dimensions, in which for common foci and apertures, viz. $2\frac{1}{2}$ to $9\frac{1}{2}$ focus, or 3,8 inches in diameter to 18 inches focus, the diameters should be 10 to 9. The shortest diameter of the ellipse being accurately the same as the diameter of the metal, and the longest diameter of the ellipse to the shortest diameter, as 10 to 9.

Ludlow, July 19, 1781.

JOHN EDWARDS.
APPENDIX.

A P P E N D I X.

An Account of several Compositions of Metals and Semi-Metals, on which Trials were made to find out the most proper Mixture for the Specula of Reflecting Telescopes. By the Rev. JOHN EDWARDS, B. A.

1. COPPER and tin * equal parts ; very bad, soft, and of a blue color.
2. Copper with arsenic $\frac{1}{8}$; but little different from the first.
3. Tin 2, copper 1 ; much worse than the preceding ones.
4. Copper 32, tin 16, arsenic 4 ; † black and brittle.
5. Copper 6, tin $1\frac{3}{4}$, arsenic 1 ; very indifferent.
6. Copper 32, tin 14, arsenic 2 ; a very good metal.
7. Copper 32, tin $13\frac{1}{2}$, arsenic 1 ; not quite so good as the sixth.
8. Copper 32, tin $13\frac{1}{2}$, arsenic $1\frac{1}{2}$; a good metal.
9. Copper 32, tin 15, arsenic 2 ; much better than any of the above.
10. Copper 6, tin 2, arsenic 1 ; compact, but very yellow when polished.
11. Copper 3, tin $1\frac{1}{4}$; compact, and whiter than the tenth.
12. Copper 32, tin $14\frac{1}{2}$; a pretty good metal, but polishes too yellow.
13. Copper 32, tin 15, arsenic 2, flint glass ‡ in powder 3 ; very bright, but rotten.
14. Brass 6, tin 1 ; compact, but too yellow.
15. Two parts of 11th composition, and 1 part of 14 composition ; compact, but much too yellow when polished.
16. Brass 5, tin 1 ; somewhat whiter than the 14th.
17. Brass 4, tin 1 ; a good metal, but rather yellow.
18. Brass 4, tin 1, with arsenic $\frac{1}{8}$; whiter than 17.
19. Brass 3, tin $1\frac{1}{2}$; will not polish well.
20. Brass 2, tin 1 ; of a sparry nature.
21. Tin 3, brass 1 ; too soft, being only a kind of hard pewter.
22. Brass and arsenic equal parts ; a dirty white colour.
23. Brass, copper, and arsenic equal parts ; a dingy white.

* By tin, I always mean grain tin.

† The nitre was added to fix the arsenic.

‡ Flint glass was added as a flux. See Shaw's Chemistry, p. 255. Note, the 10th is the composition of Sir Isaac Newton. See Appendix to Gregory's Optics, p. 221. The 11th, 14th, and 15th are the compositions of Mr. Molyneux. See Smith's Optics, Vol. II. p. 304. And the 12th is the composition of Mr. Mudge. See Philosophical Transactions, Vol. LXVII. p. 298.

§ These compositions are mentioned by Neri and Kunckell in Neri's art of glass making. Surely they never tried those compositions themselves, but took them upon the report of other authors ; as the 19 will not take a good lustre, and the 21 is very soft, like hard pewter, therefore highly improper for specula, which should be as hard as possible.

24. Brass.

24. Brass and platina, equal parts; very difficult to fuse and mix well together, is then malleable, and of a dingy white colour like 22, composition.
 25. Copper 32, tin 14, crude antimony 4; black and rotten.
 26. Copper 32, tin 14, crude antimony 1; blueish and rough grained.
 27. Copper 32, tin 15, arsenic 4, bismuth 2; much too rotten.
 28. Copper 32, tin 15, arsenic 3, bismuth 1; much too yellow when polished, and appears also porous.
 29. Copper 2, zinc 1; a pale malleable metal.
 30. Copper and zinc equal parts; still malleable and rough grained.
 31. Copper 32, tin 15, arsenic 4, zinc 4; a good metal, but does not take a high lustre.
 32. The 31st composition fluxed with corrosive sublimate; a compact and hard metal, but rather yellow when polished.
 33. Copper* 32, tin 16; a most beautiful, brilliant composition, but much too brittle and rotten.
 34. Copper 32, tin 17; blueish and rough grained.
 35. Copper 32, tin 18; blueish and rough grained.
 36. Brass 2, zinc 1; nearly of a gold colour.
 37. Brass and zinc equal parts; a pale gold color, and rough grained.
 38. Spelter 4, tin 1; very rotten.
 39. Copper and crude antimony equal parts; of a sparry nature.
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II.

Experiments and Observations on Shell and Bone. By CHARLES HATCHETT, Esq. F. R. S.
(Concluded from page 506.)

THE bones of fish, such as those of the salmon, mackarel, brill, and skate, afforded phosphate of lime; and the only difference was, that the bones of these fish appeared in general to contain more of the cartilaginous substance relative to the phosphate of lime, than is commonly found in the bones of quadrupeds, &c.

The different bones also of the same fish were various in this respect; and the bones about the head of the skate only differed from cartilage by containing a moderate proportion of phosphate of lime.

It is at present believed, that phosphate, with some sulphate of lime, constitutes the whole of the ossifying substance; and perhaps the formation of bone from cartilage depends only on the phosphate of lime; but whether this is the case or not, it is fit that I

* Unless the copper is very pure, this composition will be of a dark blue colour, as 15 ounces of grain tin will generally saturate two pounds weight of copper.

should

should notice a third substance, which constantly occurred in the course of my experiments.

When human bones or teeth, as well as those of quadrupeds and fish, whether recent or calcined, were exposed to the action of acids, an effervescence, although at times but feeble, was produced. This circumstance at first I did not particularly notice; but the following experiments excited my attention:

After the phosphate of lime had been precipitated from the solutions of various teeth and bones by pure ammoniac, I observed that a second precipitate, much smaller in quantity, was obtained by the addition of carbonate of ammoniac. This second precipitate dissolved in acids with much effervescence, during which carbonic acid was disengaged, and selenite was formed by adding sulphuric acid. Moreover the solution of this precipitate did not contain any phosphoric acid; nor did the liquor, from which the precipitate had been separated, afford any trace of it.

This precipitate was, therefore, carbonate of lime; but I still was not certain that it existed as such in the teeth and bones.

Although regular and comparative analyses of the bones of different animals have not hitherto been made, yet by the experiments of Messrs. Gahn, Scheele, Macquer, Fourcroy, Berniard, and the Marquis de Bullion, it has been proved that phosphate of lime is the principal ossifying substance of bones in general, and that this is accompanied by a small proportion of saline substances, and by sulphate of lime.

I was, therefore, desirous to ascertain whether the carbonate of lime, which I had obtained by the above mentioned experiments, had been produced from the sulphate of lime, decomposed by the alkaline precipitant, or whether the greater part had not existed in the bones in the state of carbonate.

Each of the solutions in nitric acid afforded a precipitate with nitrate of barytes; but the quantity of sulphuric acid thus separated appeared by far too small to be capable of saturating the whole of the carbonate of lime obtained from an equal quantity of the solution. To prove, therefore, the presence of the carbonic acid, and the consequent formation of carbonate of lime, portions of the various teeth and bones were immersed at separate times in muriatic acid; and the gas produced was received in lime water, by which it was speedily absorbed, and a proportionate quantity of carbonate of lime was obtained.

Although it appears that the principal effects during ossification are produced by the phosphate of lime, yet we here see that not only some sulphate, but also some carbonate of lime enters the composition of bones; and it is not a little curious to observe, that as the carbonate of lime exceeds in quantity the phosphate of lime in crustaceous marine animals and in the egg-shells of birds, so in bones it is *vice versa*. It is possible, when many accurate comparative analyses of bones have been made, that some may be found composed only of phosphate of lime; and that thus shells containing only carbonate of lime, and bones containing only phosphate of lime, will form the two extremities of the chain.

I shall

I shall now make a few remarks on the enamel of teeth.

When a tooth, coated with enamel, is immersed in diluted nitric or muriatic acid, a feeble effervescence takes place, and the enamel is completely dissolved; so also is the bony part; but the cartilage of that part is left, retaining the shape of the tooth. Or if a tooth, in which the enamel is intermixed with the bony substance, is plunged in the acid, the enamel and the bony part are dissolved in the same manner as before; that is to say, the enamel is completely taken up by the acid, while the tooth, like other bones, remains in a pulpy or cartilaginous state, having been deprived of the ossifying substance. Consequently, those parts which were coated or penetrated by lines of enamel are diminished in proportion to the thickness of the enamel which has been thus dissolved; but little or no diminution is observed in the tooth*.

Mr. Hunter has noticed this; and speaking of enamel says, "when soaked in a gentle acid, there appears no gristly or fleshy part with which the earthy part had been incorporated †."

Now when the difference, which has been lately stated between porcellaneous shell and mother of pearl, is considered, it is not possible to avoid the comparing of these to enamel and tooth.

When porcellaneous shell, whole, or in powder, is exposed to the action of acids, it is completely dissolved without leaving any residuum.

Enamel is also completely dissolved in the like manner.

Porcellaneous shell and enamel when burned, emit little or no smoke, nor scarcely any smell of burned horn or cartilage.

Their figure, after having been exposed to fire, is not materially changed, except by cracking in some parts; their external gloss partly remains, and their colour at most becomes grey, very different from what happens to mother of pearl, or tooth. In their fracture they have a fibrous texture; and in short, the only essential difference between them appears to be, that porcellaneous shell consists of carbonate of lime, and enamel of phosphate of lime, each being cemented by a small portion of gluten.

In like manner, if the effects produced by fire and acid menstrua, on shells composed of mother of pearl, and on the substance of teeth and bone, are compared, a great similarity will be found; for when exposed to a red heat,

1st. They smoke much, and emit a smell of burned cartilage or horn.

2ndly. They become of a dark grey, or black colour.

3dly. The animal coal thus formed is of different incineration.

4thly, They retain much of their original figure; but the membranaceous shells are subject to exfoliate ‡.

* I have also observed, that when raspings of enamel are put into diluted nitric, or muriatic acid, they are dissolved without any apparent residuum; but when raspings of tooth or bone are thus treated, portions of membrane or cartilage remain corresponding to the size of the raspings.

† Natural History of the Human Teeth, p. 35.

‡ This is a natural consequence arising from their structure.

5thly.

5thly. These substances (pearl, mother of pearl, tooth and bone) when immersed in certain acids, part with their hardening or ossifying substances, and then remain in the state of membrane or cartilage.

6thly. When previously burned, and afterwards dissolved in acids, a quantity of animal coal is separated, according to the proportion of the gelatinous, membranaceous, or cartilaginous substance, and according to the duration of the red heat.

And lastly, the acid solutions of these substances by proper precipitants, afford carbonate of lime in the one case, and phosphate of lime principally in the other, in a proportion relative to the membrane or cartilage with which, or on which, the one or the other had been mixed or deposited.

As porcellaneous shell principally differs from mother of pearl, only by a relative proportion between the carbonate of lime and the gluten, or membrane; in like manner the enamel appears only to be different from tooth or bone, by being destitute of cartilage, and by being principally formed of phosphate of lime cemented by gluten.

The difference in the latter case seems to explain, why the bones and teeth of animals fed on madder, become red, when at the same time the like colour is not communicated to the enamel; for it appears probable that the cartilages, which form the original structure of the teeth and bones, become the channel by which the tinging principle is communicated and diffused.

These comparative experiments prove, that there is a great approximation in the nature of porcellaneous shell and the enamel of teeth, and also in that of mother of pearl and bone; and if a shell should be found composed of mother of pearl, coated by the porcellaneous substance, it will resemble a tooth coated by the enamel, with the difference of carbonate being substituted for phosphate of lime.

Some experiments on cartilaginous substances (which I intended to have inserted in this paper, but which I am prevented from doing, as they are not as yet sufficiently advanced) have in a great measure convinced me, that membranes and cartilages (whether destined to become bones by a natural process, as in young animals, or whether they become such by morbid ossification, as often happens in those which are aged) do not contain the ossifying substance, or phosphate of lime, as a constituent principle. I mean by this, that I believe the portion of phosphate of lime found in cartilaginous and horny substances, to be simply mixed as an extraneous matter; and that when it is absent, membrane, cartilage, and horn are most perfect and complete.

The frequent presence of phosphate of lime in cartilaginous substances, is not a proof of its being one of their constituent principles, but only that it has become deposited and mixed with them, in proportion to the tendency they may have to form modifications of bone; or according to their vicinity with such membranes or cartilages, as are liable to such a change. If horns are examined, few I believe will be found to contain phosphate of lime in such a proportion, as to be considered an essential ingredient. I would not be understood to speak here of such as stag, or buck horn, for that has every chemical character

of

of bone, with some excess of cartilage; but I allude to those in which the substance of the horn is distinctly separate from the bone; and which, like a sheath, covers a bony protuberance, which issues from the os frontis of certain animals*.

Horns of this nature, such as those of the ox, the ram, and the chamois, also tortoise-shell, afford after distillation and incineration, so very small a residuum, of which only a small part is phosphate of lime, that this latter can scarcely be regarded as a necessary ingredient.

By some experiments made on 500 grains of the horn of the ox, I obtained, after a long continued heat, only 1,50 grains of residuum; and of this less than half proved to be phosphate of lime.

78 grains of the horn of the chamois afforded only 0,50 of residuum; and 500 grains of tortoise-shell yielded not more than 0,25 of a grain, of which less than half was phosphate of lime.

Now it must be evident, that so very small a quantity cannot influence the nature of the substances which afforded it; and the same may be said of synovia, 480 grains of which did not yield more than one grain of phosphate of lime.

This substance is undoubtedly various in its proportions, in all these and other animal substances, arising probably from the age and habit of the animal which has produced them; but I believe that I may at least venture to place some confidence in the foregoing experiments, as several others made since the above was written, have tended to confirm them†.

In the course of making the experiments which have been related, I examined the fossil bones of Gibraltar, as well as some glossopetræ, or shark's teeth. The latter afforded phosphate and carbonate of lime; but the carbonate of lime was visibly owing principally to the matter of the calcareous strata which had inclosed these teeth, and which had insinuated itself into these cavities, left by the decomposition of the original cartilaginous substance.

The bones of the Gibraltar rock also consist principally of phosphate of lime; and the cavities have been partly filled by the carbonate of lime which cements them together.

Fossil bones resemble bones, which by combustion have been deprived of their cartilaginous part; for they retain the figure of the original bone, without being bone in reality, as one of the most essential parts has been taken away. Now such fossil, or burned bones,

* Nature seems here to have made an analysis, or separation of horn from bone.

† These experiments were repeated on bladders, which I chose in preference to any other membrane, as not being liable to ossification, and therefore likely to contain very little, or no phosphate of lime. 250 grains of dry hog's bladder after incineration left a residuum, the weight of which did not exceed $\frac{1}{30}$ of a grain. This was dissolved in diluted nitric acid; and upon adding pure ammoniac, some faint traces of phosphate of lime were observed. Now as 250 grains of bladder did not afford more than $\frac{1}{30}$ of a grain of residuum, of which only a part consisted of phosphate of lime; there is much reason to regard this experiment as an additional proof, that the phosphate of lime is not an essential ingredient of membrane.

can no more be regarded as bone, than charcoal can be considered as the vegetable, of which it retains the figure and fibrous structure.

Bones which keep their figure after combustion, resemble charcoal made from vegetables, replete with fibre; and cartilaginous bones which lose their shape by the same cause, may be compared to succulent plants, which are reduced in bulk and shape in a similar manner.

From these last experiments, I much question if bodies, consisting of phosphate of lime like bones, have concurred materially to form strata of limestone or chalk; for it appears to be improbable, that phosphate is converted into carbonate of lime, after these bodies have become extraneous fossils.

The destruction or decomposition of the cartilaginous parts of teeth and bones in a fossil state, must have been the work of a very long period of time, unless accelerated by the action of some mineral principle; for after having in the usual manner steeped in muriatic acid the os humeri of a man brought from Hythe in Kent, and said to have been taken from a Saxon tomb, I found the remaining cartilage nearly as compleat as that of a recent bone. The difficult destructibility of substances of a somewhat similar nature, appears also from the piercing implements formed of horn, which are not unfrequently found in excavations of high antiquity.

III.

*Some Account of the Elastic Gum Vine of Prince of Wales's Island, and of Experiments made on the milky Juice which it produces: with Hints respecting the useful Purposes to which it may be applied. By JAMES HOWISON, Esq.**

OUR first knowledge of the plant being a native of our island arose from the following accident. In our excursions into the forests, it was found necessary to carry cutlasses, for the purpose of clearing our way through the underwood. In one of those an elastic gum vine had been divided, the milk of which drying upon the blade, we were much surprized in finding it possess all the properties of the *American Caout-chouc*. The vine which produces this milk is generally about the thickness of the arm, and almost round, with a strong ash-coloured bark, much cracked, and divided longitudinally; has joints at a small distance from each other, which often send out roots, but seldom branches; runs upon the ground to a great length; at last rises upon the highest trees into the open air. It is found in the greatest plenty at the foot of the mountains, upon a red clay mixed with sand, in situations completely shaded, and where the mercury in the thermometer will seldom exceed summer heat.

* Asiatic Researches, V. 157.

In my numerous attempts to trace this vine to its top, I never succeeded; for, after following it in its different windings, sometimes to a distance of two hundred paces, I lost it, from its ascending among the branches of trees that were inaccessible either from their size or height. On the west coast of *Sumatra* I understand they have been more successful; *Dr. Roxburgh* having procured from thence a specimen of the vine in flowers, from which he has classed it; but whose description I have not yet seen.

With us the *Malays* have found tasting of the milk the best mode of discriminating between the elastic gum vine, and those which resemble it in giving out a milky juice, of which we have a great variety; the liquid from the former being much less pungent or corrosive than that obtained from the latter.

The usual method of drawing off the milk is by wounding the bark deeply in different places, from which it runs but slowly, it being full employment for one person to collect a quart in the course of two days. A much more expeditious mode, but ruinous to the vine, is cutting it in lengths of two feet, and placing under both ends vessels to receive the milk. The best is always procured from the oldest vines. From them it is often obtained in a consistence equal to thick cream, and which will yield two thirds of its own weight in gum.

The chemical properties of this vegetable milk, so far as I have had an opportunity of examining, surprisingly resemble those of animal milk. From its decomposition in consequence of spontaneous fermentation, or by the addition of acids, a separation takes place between its *caseous* and *serous* parts, both of which are very similar to those produced by the same processes from animal milk. An oily or butyrous matter is also one of its component parts, which appears upon the surface of the gum so soon as the latter has attained its solid form. The presence of this considerably impeded the progress of my experiments, as will be seen hereafter.

I was at some trouble in endeavouring to form an extract of this milk so as to approach to the consistence of new butter, by which I hoped to retard its fermentative stage, without depriving it of its useful qualities; but as I had no apparatus for distilling, the surface of the milk, that was exposed to the air, instantly formed into a solid coat, by which the evaporation was in a great degree prevented. I, however, learned, by collecting the thickened milk from the inside of the coats, and depositing it in a jelly pot, that, if excluded from the air, it might be preserved in this state for a considerable length of time.

I have kept it in bottles, without any preparation, tolerably good, upwards of one year; for, notwithstanding the fermentation soon takes place, the decomposition in consequence is only partial, and what remains fluid, still retains its original properties, although considerably diminished.

Not having seen *M. Fourcroy's* memoir on *Caout-chouc*, I could not make trials of the methods proposed by him for preserving the milk unaltered.

In making boots, gloves, and bottles, of the elastic gum, I found the following method the best: I first made moulds of wax, as nearly of the size and shape of what they repre-

fented as possible: these I hung separately upon pins, about a foot from the ground, by pieces of cord wrought into the wax; I then placed under each a soup plate, into which I poured as much of the milk as I thought would be sufficient for one coat. Having dipped my fingers in this, I completely covered the moulds one after another, and what dropped into the plates was used as part of the next coat: the first I generally found sufficiently dry in the space of ten minutes, when exposed to the sun, to admit of a second being applied: however, after every second coat, the oily matter before mentioned was in such quantity upon the surface, that, until washed off with soap and water, I found it impossible to apply any more milk with effect; for, if laid on, it kept running and dividing like water upon wax.

Thirty coats I in common found sufficient to give a covering of the thickness of the bottles which come from *America*. This circumstance may, however, at any time be ascertained, by introducing the finger between the mould and gum, the one very readily separating from the other.

I found the fingers preferable to a brush, or any instrument whatever, for laying on the milk; for the moment a brush was wet with that fluid, the hair became united as one mass. A mode which at first view would appear to have the advantage of all others for ease and expedition in covering clay and wax moulds with the gum, *viz.* immersing them in the milk, did not at all answer upon trial; that fluid running almost entirely off, although none of the oily matter was present; a certain degree of force seeming necessary to incorporate by friction the milk with the new formed gum.

When, upon examination, I found that the boots and gloves were of the thickness wanted, I turned them over at the top, and drew them off, as if from the leg or hand, by which I saved the trouble of forming new moulds. Those of the bottles being smallest at the neck, I was under the necessity of dissolving in hot water.

The inside of the boots and gloves which had been in contact with the wax being by far the smoothest, I made the outside. The gloves were now finished, unless cutting their tops even, which was best done with scissors. The boots, however, in their present state, more resembled stockings, having as yet no soles. To supply them with these, I poured upon a piece of gunny a proper quantity of milk, to give it a thick coat of gum. From this, when dry, I cut pieces sufficiently large to cover the sole of the foot, which, having wet with the milk, I applied; first replacing the boot upon the mould to keep it properly extended. By this mode the soles were so firmly joined, that no force could afterwards separate them. In the same manner I added heels and straps, when the boots had a very neat appearance. To satisfy myself as to their impermeability to water, I stood in a pond up to their tops for the space of fifteen minutes, when, upon pulling them off, I did not find my stockings in the least damp. Indeed, from the nature of the gum, had it been for a period of as many months, the same result was to have been expected.

After being thus far successful, I was greatly disappointed in my expectations with regard to their retaining their original shape; for, on wearing them but a few times, they
lost

lost much of their first neatness, the contractions of the gum being only equal to about seven eighths of its extension.

A second disadvantage arose from a circumstance difficult to guard against, which was, that if, by any accident, the gum should be in the smallest degree weaker in one place than another, the effect of extension fell almost entirely on that part, and the consequence was, that it soon gave way.

From what I had observed of the advantage gained in substance and uniformity of strength, by making use of gunny as a basis for the soles, I was led to suppose, that if an elastic cloth, in some degree correspondent to the elasticity of the gum, were used for boots, stockings, gloves, and other articles, where that property was necessary, that the defects above mentioned might in a great measure be remedied. I accordingly made my first experiment with *Coffimbazar* stockings and gloves.

Having drawn them upon the wax moulds, I plunged them into vessels containing the milk, which the cloth greedily absorbed. When taken out, they were so completely distended with the gum in solution, that, upon becoming dry by exposure to the air, not only every thread, but every fibre of the cotton had its own distinct envelope, and in consequence was equally capable of resisting the action of foreign bodies as if of solid gum.

The first coat by this method was of such thickness, that for stockings or gloves nothing farther was necessary. What were intended for boots required a few more applications of gum with the fingers, and were finished as those made with the gum only.

This mode of giving cloth as a basis I found to be a very great improvement: for, besides the addition of strength received by the gum, the operation was much shortened.

Woven substances, that are to be covered with the gum, as also the moulds on which they are to be placed, ought to be considerably larger than the bodies they are afterwards intended to fit; for, being much contracted from the absorption of the milk, little alteration takes place in this diminution in size, even when dry, as about one third only of the fluid evaporates before the gum acquires its solid form.

Great attention must be paid to prevent one part of the gum coming in contact with another while wet with the milk or its whey; for the instant that takes place, they become inseparably united. But should we ever succeed in having large plantations of our own vine, or in transferring the *American* tree (which is perhaps more productive) to our possessions, so that milk could be procured in sufficient quantity for the covering various cloths, which should be done on the spot, and afterwards exported to *Europe*, then the advantages attending this singular property of the milk would for ever balance its disadvantages: cloths, and coverings of different descriptions, might then be made from this gum cloth, with an expedition so much greater than by the needle, that would at first appear very surprising: the edges of the separate pieces only requiring to be wet with the milk, or its whey, and brought into contact, when the article would be finished, and fit for use. Should both milk and whey be wanting, a solution of the gum in ether can always be obtained, by which the same end would be accomplished

Of all the cloths upon which I made experiments, nankeen, from the strength and quality of its fabric, appeared the best calculated for coating with the gum. The method I followed in performing this, was, to lay the cloth smooth upon a table, pour the milk upon it, and with a ruler to spread it equally. But should this ever be attempted on a larger scale, I would recommend the following plan: to have a cistern for holding the milk a little broader than the cloth, to be covered with a cross bar in the centre, which must reach under the surface of the milk, and two rollers at one end. Having filled the cistern, one end of the piece of cloth is to be passed under the bar, and through between the rollers; the former keeping the cloth immersed in the milk, the latter in pressing out what is superfluous, so that none may be lost. The cloth can be hung up at full length to dry; and the operation repeated until of whatever thickness wanted. For the reasons above-mentioned, care must be taken that one fold does not come in contact with another while wet.

Having observed that most of the patent catheters and bougies made with a solution of the elastic gum, whether in ether or in the essential oils, had either a disagreeable stickiness, or were too hard to admit of any advantage being derived from the elasticity of the gum, I was induced to make some experiments with the milk towards removing these objections.

From that fluid, by evaporation, I made several large sized bougies of pure gum, which, from their over-flexibility, were totally useless. I then took some slips of fine cloth covered with the gum, which I rolled up until of a proper size, and which I rendered solid by soaking them in the milk, and then drying them. These possessed more firmness than the former, but in no degree sufficient for the purpose intended. Pieces of strong catgut, coated with the gum, I found to answer better than either.

Besides an effectual cloathing for manufacturers employed with the mineral acids, which had been long a desideratum, this substance, under different modifications, might be applied to a number of other useful purposes in life; such as making hats, great coats, boots, &c. for sailors, soldiers, fishermen, and every other description of persons who, from their pursuits, are exposed to wet stockings; for invalids, who suffer from damps; bathing caps, tents, coverings for carriages of all kinds, for roofs of houses, trunks, buoys, &c.

This extraordinary vegetable production, in place of being injured by water, at its usual temperature * is preserved by it. For a knowledge of this circumstance I am indebted to the *Chinefe*. Having some years ago commissioned articles made of the elastic gum from *China*, I received them in a small jar filled up with water, in which state I have since kept them without observing any signs of decay.

Should it ever be deemed an object to attempt plantations of the elastic gum vine in *Bengal*, I would recommend the foot of the *Chittagong*, *Rajmahal* and *Bauglipore* hills, as

* From an account of experiments made with the elastic gum by M. Grossart, inserted in the *Annals de Chimie* for 1792, it appears, that water, when boiling, has a power of partially dissolving the gum so as to render one part capable of being finally joined to another by pressure only.

situations where there is every probability of succeeding, being very similar in soil and climate to the places of its growth on *Prince of Wales's Island*. It would, however, be advisable to make the first trial at this settlement, to learn in what way the propagation of the plant might be most successfully conducted. A further experience may also be necessary, to ascertain the season when the milk can be procured of the best quality, and in the greatest quantity, with the least detriment to the vine.

IV.

Miscellaneous Observations relative to the Western Parts of Pennsylvania, particularly those in the Neighbourhood of Lake Erie. By ANDREW ELLICOTT.*

DEAR SIR,

I TAKE the liberty of transmitting to you the following miscellaneous observations, collected from my notes, relative to Lake Erie, and the Western Country, the perusal of which I flatter myself will not be unsatisfactory or uninteresting.

The situation of this lake is already well known, and therefore a particular topographical description will here be unnecessary; but a variety of phenomena which attend it, merit a more minute consideration, and cannot fail to engage the attention of the philosopher; phenomena which in all probability are common to all large lakes of fresh water.

In the summer season fogs are seldom observed on the margin of the lake. The three summer months that I resided at Presqu' Isle, no fogs were seen during the whole time. The horizon was generally clear, and the stars shone with remarkable lustre. The most common winds here generally resemble the sea and land breezes, in the West Indies. From the end of spring till the beginning of autumn, they blow, except at the time of storms, from the lake upon the land during great part of the day, and from the land upon the lake during the night: the change generally takes place between the hours of seven and ten in the morning, and about the setting of the sun in the evening. These breezes, alternately blowing in opposite directions, render those situations contiguous to the lake extremely pleasant during the heat of the summer months, and have most probably a very salutary influence upon the atmosphere.

A strong easterly wind will occasion a considerable depression, and a strong westerly wind a considerable swell of the waters in Presqu' Isle Bay. In the former case, a portion of the water is driven towards the upper end, and in the latter, towards the lower end of the lake. To these causes we are to attribute those ebbings, and flowings, which have so frequently been mistaken for regular tides: for a little reflection will convince one, that the moon can have no sensible effect upon the waters of the lakes. When the wind ceases the waters re-

* Addressed to Mr. Robert Patterson, Vice-President of the American Philosophical Society, and inserted in their Transactions, Vol. IV. lately published.

turn to restore the equilibrium, and an undulation will be visible for several days after those storms, and appears to be but slightly affected by the alternate breezes already mentioned.

In the western country, and especially in the neighbourhood of the lakes, dews are very heavy. On the Ohio and Allegany rivers, and their numerous branches, fogs are very common, and of remarkable density; they do not however appear to contain any portion of those noxious miasmata, which are so frequently combined with the fogs on the eastern side of the mountains; nay the inhabitants of Pittsburgh consider them as possessed of salubrious qualities. From a variety of observations I am convinced that the atmosphere in the western country, and particularly in the vicinity of the lakes, contains a greater quantity of moisture than in the middle Atlantic states. The wooden works which contained my instruments were always uncommonly swelled, and frequently very much injured in that country, though constantly defended from the rain, and occasionally exposed to the sun. The ivory and wood of my sectors with brass joints, always expanded above the metal; this expansion was not sudden, but effected by slow degrees. Whether this excess of moisture arises from the extensive forests which constantly preserve the earth in a state of humidity or from more permanent causes, future observations must determine.

Iron is here more susceptible of rust, and brass sooner tarnished than in the Atlantic states; but this susceptibility of rust I observed to be greater in the forests than in those parts of the country that had been cleared for cultivation, and from these circumstances the probable cause is ascertained.

The southern shores of Lake Erie are generally high; in many places they are perpendicular, and various strata of stone are considerably elevated above the surface of the water. The streams which discharge themselves into the lake over these strata form a great variety of cascades of a romantic appearance, which increase the beauty of the country, and must at some future period enhance the value of the lands.

At the lower end of the lake, and for some distance up it, these strata consist of limestone intermixed with flint and marine petrifications, but the other strata are generally slate and excellent freestone. About Presqu' Isle there is but little lime-stone to be seen, it lies in detached pieces, and is likewise interspersed with flint and marine petrifications.

In a large extent of country on the western side of the Allegany Mountain, the strata of stone are horizontally disposed, except in some places where that position has been changed by the undermining of creeks and rivers. In these places where the strata have been deprived of their support, they have fallen from their original positions, and therefore deviate from the general rule. This law of nature is established on the south side of Lake Erie, but how far west of the mountains the same obtains, has never yet been ascertained. The horizontal position of the strata on that lake has a pleasing effect; the softer lamina are worn away by the beating of the waves, the harder remain projected, and at a distance resemble wainscoting or mouldings.

From the horizontal disposition of these strata the following conclusions may be deduced; first, that the country has never been disturbed by those terrible convulsions which a great
part

part of this globe must have experienced at some remote period of antiquity; and secondly, that those naturalists are deceived, who suppose that the strata were originally parallel to the axis of the earth.

Before I conclude my observations on this subject, I shall take the liberty of adding an account of the falls of Niagara, which are in some measure connected with the horizontal disposition of the strata in the Western and North Western Country.

This stupendous cataract of water infinitely excels all other natural curiosities of the country, and exhibits a spectacle scarce equalled in grandeur by any object in the physical world. Lake Erie is situated upon one of those horizontal strata in a region elevated about three hundred feet above the country which contains Lake Ontario. The descent which separates the two countries, is in some places almost perpendicular, and the immense declivity formed by these strata occasions both the cataract of Niagara and the great falls of Cheneseco. This remarkable precipice generally runs in a south-western direction from a place near the Bay of Toronto on the northern side of Ontario, round the western angle of the lake; from thence it continues its course generally in an eastern direction, crossing the strait of Niagara and the Cheneseco river, till it is lost in the country towards the Seneca Lake.

The waters of this cataract formerly fell from the northern side of the slope, near the landing place; but the action of such a tremendous column of water falling from such an eminence, through a long succession of ages, has worn away the solid stone for the distance of seven miles, and formed an immense chasm which cannot be approached without horror. In ascending the road from the landing to Fort Slausser the eye is continually engaged in the contemplation of the awful, and romantic scenes which present themselves, till the transcendent magnificence of the falls is displayed to view, the imagination is then forcibly arrested, and the spectator is lost in silent admiration! down this awful chasm, the waters are precipitated with amazing velocity after they make the great pitch, and such a vast torrent of falling water communicates a tremulous motion to the earth, which is sensibly felt for some poles round, and produces a sound which is frequently heard at the distance of twenty miles. Many wild beasts that attempt to cross the rapids above this great cataract, are destroyed; and if geese or ducks inadvertently alight in these rapids, they are incapable of rising upon the wing again, and are hurried on to inevitable destruction.

The great height of the banks renders the descent into the chasm extremely difficult; but a person after having descended may easily proceed to the base of the falls, and a number of persons may walk in perfect safety a considerable distance between the precipice and the descending torrent, where conversation is not much interrupted by the noise, which is not so great here as at some distance. A vapour or spray of considerable density, resembling a cloud, continually ascends, in which a rainbow is always seen when the sun shines, and the position of the spectator is favourable. In the winter this spray attaches itself to the trees, where it is congealed in such quantities as to divest them of their smaller branches, and

produces a most beautiful chrystalline appearance; a circumstance which attends the falls of Chenefeco, as well as those of Niagara.

A singular appearance is observed at these falls, which has never perhaps been noticed by any writer. Immediately below the great pitch a commixture of foam and water is puffed up in spherical figures, about the size of a common haycock. They burst at the top, and discharge a column of spray to a prodigious height; they then subside, and are succeeded by others, which exhibit the same appearances. These spherical forms are most conspicuous about midway between the west side of the streight, and the island which divides the falls, and where the largest column of water descends. This appearance is produced by the ascension of the air, which is carried down by the column of falling water in great quantities to the bed of the river.

The river at the falls is about seven hundred and forty three yards wide, and the perpendicular pitch is one hundred and fifty feet in height. In the last half mile immediately above the falls the descent of the water is fifty-eight feet; but the difficulty which would attend the business, prevented me from attempting to level the rapids in the chasm below; though from conjecture, I concluded that the waters must descend at least sixty-five feet; and from these results it appears that the water falls about two hundred and seventy-three feet, in the distance of about seven miles and an half.

I am, Sir, with respect,

Your friend,

ANDREW ELLICOTT.

To Robert Patterson.

V.

*An Account of the Pearl Fishery in the Gulph of Manar, in March and April 1797. By HENRY J. LE BECK, Esq.**

FROM the accounts of the former pearl fisheries at *Ceylon*, it will be found, that none have ever been so productive as this year's. It was generally supposed that the renter would be infallibly ruined, as the sum he paid for the present fishery was thought exorbitant when compared with what had been formerly given; but this conjecture in the event appeared ill founded, as it proved extremely profitable and lucrative.

The farmer this time was a *Tamul* merchant, who for the privilege of fishing with more than the usual number of donies or boats, paid between two and three hundred thousand *Porto-novo* pagodas, a sum nearly double the usual rent.

* Asiatic Researches, IV. 493.

These boats he farmed out again to individuals in the best manner he could, but for want of a sufficient number of divers some of them could not be employed.

The fishing, which commonly began about the middle of *February*, if wind and weather allowed, was this year, for various reasons, delayed till the end of the month; yet so favourable was the weather, that the renter was able to take advantage of the permission granted by the agreement, to fish a little longer than the usual period of thirty days.

The fishery cannot well be continued after the setting in of the southern monsoon, which usually happens about the 15th of *April*, as, after that time, the boats would not be able to reach the pearl banks, and the water being then so troubled by heavy seas, diving would be impracticable; in addition to which, the sea-weed, a species of *fucus*, driven in by the southerly wind, and which spreads to a considerable distance from the shore, would be an impediment.

Many of the divers, being Roman Catholics, leave the fishery on *Sundays* to attend divine service in their church at *Aripoo*; but if either a *Mahomedan* or *Hindoo* festival happens during the fishing days, or if it is interrupted by stormy weather, or any other accident, this lost time is made up by obliging the Catholics to work on *Sundays*.

The fear of sharks, as we shall see hereafter, is also another cause of interruption. These, amongst some others, are the reasons that, out of two months, (from February till April) seldom more than thirty days can be employed in the fishery.

As this time would be insufficient to fish all the banks, (each of which has its appropriate name, both in Dutch and Tamul,) it is carried on for three or four successive years, and a new contract annually made till the whole banks have been fished, after which they are left to recover.

The length of time required for this purpose, or from one general fishing to another, has not yet been exactly determined; it was, therefore, a practice to depute some persons to visit the banks annually, and to give their opinion, whether a fishery might be undertaken with any degree of success*?

From various accounts, which I have collected from good authority, and the experience of those who assisted at such examinations, I conjecture, that every seven years such a general fishery could be attempted with advantage, as this interval seems sufficient for the pearl shells to attain their growth: I am also confirmed in this opinion, by a report made by a Dutch governor at *Jafnas* of all the fisheries that have been undertaken at *Ceylon* since 1722; a translation of which is to be found in Wolfe's Travels into *Ceylon*. But the ruinous condition in which the divers leave the pearl banks at each fishery, by attending only to the profit of individuals, and not to that of the public, is one great cause, that it requires twice the above mentioned space of time, and sometimes longer, for rendering the fishing productive. They do not pay the least attention, to spare the young and immature shells that contain no pearl; heaps of them are seen thrown out of the boats as useless,

* A gentleman, who assisted at one of the last visits, being an engineer, drew a chart of the banks, by which their situation and size are now better known than formerly.

on the beach between *Manâr** and *Aripoo*; if these had been suffered to remain in their native beds, they would, no doubt, have produced many fine pearls. It might, therefore, be advisable, to oblige the boat people to throw them into the sea again, before the boats leave the bank. If this circumstance, in sparing the small pearl shells, to perpetuate the breed was always observed, succeeding fisheries might be expected sooner, and with still greater success: but the neglect of this simple precaution will, I fear, be attended with similar fatal consequences here, as have already happened to the pearl banks on the coast of *Persia*, *South America*, and *Sweden*, where the fisheries are by no means so profitable at present as they were formerly.

Another cause of the destruction of numbers of both old and young pearl shells, is the anchoring of so many boats on the banks, almost all of them used differently formed, clumsy, heavy, wooden anchors, large stones, &c. &c. If this evil cannot be entirely prevented, it might, at least, be greatly lessened, by obliging them all to use anchors of a particular sort, and less destructive.

This season the *Seewel* bank only was fished, which lies above twenty miles to the westward of *Aripoo*, opposite to the fresh water rivers of *Moosalee* *Modragam* and *Pomparipoo*. It has been observed, that the pearls on the north-west part of this bank, which consists of rock, are of a clearer water than those found on the south-east, nearest the shore, growing on corals and sand.

Condatchey is situated in a bay, forming nearly a half moon, and is a waste, sandy district, with some miserable huts built on it. The water is bad and brackish, and the soil produces only a few, widely scattered, stunted trees and bushes. Those persons who remain here during the fishery are obliged to get their water for drinking from *Aripoo*, a village with a small old fort, lying about four miles to the southward. Tigers, porcupines, wild hogs, pangolines, or the *Ceylon armadillos*, are, amongst other quadrupeds, here common. Of amphibia, there are tortoises, especially the *testudo geometrica*, and various kinds of snakes. A conchologist meets here with a large field for his enquiries. The presents which I made to the people employed in the fishery, to encourage them to collect all sorts of shells which the divers bring on shore, produced but little effect; as they were too much taken up in searching after the mother of pearl shells to pay attention to any other object. However, my endeavours were not entirely useless; I will specify here a few of the number I collected during my stay: different kinds of *peetines* †, *palium porphyreum*, *solen radiatus* ‡, *Venus castrensis*, Linn. § *astrea hyotis* ||, *ostr. Forskollii*, *ostr. Malleus* **, *mytilus hirundo* Linn. ††, *spondilus crocius*, *pholas pusillus*, Linn. ‡‡, *mitra episcopalis*, Linn.

* *Manara*, properly *Manar*, is a *Tamul* word, and signifies a sandy river, from the shallowness of the sea at that place.

† Scallops.

‡ Radiated razor shell.

§ Alpha cockle.

|| Double cocks-comb.

** Hammer oyster; these were pretty large, but many broken and some covered by a calcareous crust. It is very probable that, among those, there may be some precious white ones.

†† Swallow muscle.

‡‡ The wood piercer.

Iepas striata Pennanti, (vide Zool. Brit.), *patella tricarinata*, Linn. *bulla perfecta maculata**, *harpa nobilis*, *porcellana salita*, Rumph. †, *strombus scorpio*, and other of inferior kinds. Amongst the zoophytes, many valuable species of *spongia*, *corallina*, *fatularia*, &c. a great variety of sea stars, and other marine productions, that cannot be preserved in spirits, but should be described on the spot. These, as well as the description of the different animals inhabiting the shells, are the more worthy of our attention, and deserve farther investigation, as we are yet very deficient in this branch of natural history.

During the fishing season, the desert, barren place, *Condatchey*, offers to our view a scene equally novel and astonishing. A heterogeneous mixture of thousands of people of different colours, countries, casts, and occupations, the number of tents and huts, erected on the sea shore, with their shops or bazars before each of them; and the many boats returning on shore in the afternoon, generally richly laden; all together form a spectacle entirely new to an *European* eye. Each owner runs to his respective boat as soon as it reaches the shore, in hopes of finding it fraught with immense treasure, which is often much greater in imagination than in the shell; and though he is disappointed one day, he relies with greater certainty on the next, looking forward to the fortune promised him by his stars, as he thinks it impossible for the astrological predictions of his *Brâhmen* to err.

To prevent riot and disorder, an officer with a party of *Malays* is stationed here. They occupy a large square, where they have a field piece and a flag staff for signals.

Here and there you meet with brokers, jewellers, and merchants of all descriptions; also, sutlers offering provisions and other articles to gratify the sensual appetite and luxury. But by far the greater number are occupied with the pearls. Some are basely employed in afforting them; for which purpose they make use of small brass plates perforated with holes of different sizes; others are weighing and offering them to the purchaser; while others are drilling or boring them; which they perform for a trifle.

The instrument, these people carry about with them for this purpose, is of a very simple construction, but requires much skill and exercise to use it; it is made in the following manner: the principal part consists of a piece of soft wood, of an obtuse, inverted, conical shape, about six inches high and four in diameter in its plain surface; this is supported by three wooden feet, each of which is more than a foot in length. Upon the upper flat part of this machine are holes, or pits, for the larger pearls, and the smaller ones are beat in with a wooden hammer. On the right side of this stool, half a cocoa nut shell is fastened, which is filled with water. The drilling instruments are iron spindles, of various sizes, adapted to the different dimensions of the pearls, which are turned round in a wooden head by a bow. The pearls being placed on the flat surface of the inverted cone, as already mentioned, the operator sitting on a mat, presses on the wooden head of his instrument with the left hand, while, with his right, he moves the bow which turns round the moveable part of the drill; at the same time, he moistens the pearl, occasionally dipping the little

* Diving snail, (Grew, Mus.)

† Salt-coury, Kl.

finger of the same hand into the water of the cocoa nut shell, with a dexterity that can only be attained by constant practice.

Amongst the crowd are found vagabonds of every description, such as *Pandarams*, *Andee*, or *Hindu* monks, fakirs, beggars, and the like, who are impertinently troublesome. Two of these wretches particularly attracted the attention of the mob, though their superstitious penance must have disgusted a man of the least reflection: one had a gridiron, of one and a half foot long, and the same in breadth, fastened round his neck, with which he always walked about, nor did he take it off either when eating or sleeping; the other had fastened round that member, which decency forbids me to mention, a brass ring, and fixed to it was a chain, of a fathom in length, trailing on the ground; the links of this chain were as thick as a man's finger, and the whole was exhibited in a most scandalous manner.

The pestilential smell occasioned by the numbers of putrifying pearl fishes, renders the atmosphere of *Condatchey* so insufferably offensive when the south-west wind blows, that it sensibly affects the olfactory nerves of any one unaccustomed to such cadaverous smells. This putrefaction generates immense numbers of worms, flies, muskitoes, and other vermin; all together forming a scene strongly displeasing to the senses.

Those who are not provided with a sufficient stock of money suffer great hardships, as not only all kinds of provisions are very dear, but even every drop of good water must be paid for. Those who drink the brackish water of this place are often attacked by sickness. It may easily be conceived what an effect the extreme heat of the day, the cold of the night, the heavy dews, and the putrid smell, must have on weak constitutions. It is, therefore, no wonder that of those who fall sick many die, and many more return home with fevers, fluxes, or other equally fatal disorders.

The many disappointments, usually experienced by the lower classes of men in particular, make them often repent of their coming here. They are often ruined, as they risk all they are worth to purchase pearl shells; however, there are many instances of their making a fortune beyond all expectation. A particular circumstance of this kind fell within my own observation: a day labourer bought three oysters * for a copper fanam (about the value of two-pence) and was so fortunate as to find one of the largest pearls which the fishery produced this season.

The donies appointed for the fishery are not all procured at *Ceylon*; many came from the coasts of *Coromandel* and *Malabar*, each of which has its distinguishing number. About ten o'clock at night a gun is fired as a signal, when they sail from *Condatchey* with an easterly or land wind, under the direction of a pilot, If the wind continues fair, they reach the bank before day, and begin diving at sun rise, which they continue till the west or sea breeze sets in, with which they return. The moment they appear in sight, the colours are

* The *East India* pearl shell, is well known to be the *matrix perlarum* (mother of pearl) of Rumphius, or the *Mytilus margaritiferus* of Linnæus; consequently the general term pearl oyster must be erroneous; however, as it has long been in common use, I hope to be excused for continuing it.

hoisted at the flag staff, and in the afternoon they come to an anchor, so that the owners of the boats are thereby enabled to get their cargoes out before night, which may amount to 30,000 oysters, if the divers have been active and successful.

Each boat carries twenty-one men and five heavy diving stones for the use of ten divers, who are called in *Tamul*, *kooly kárer*, the rest of the crew consists of a tandel, or head boatman, and ten rowers, who assist in lifting up the divers and their shells.

(To be continued.)

VI.

Description of the Air Furnace of CIT. LECOUR.

THE air furnace, of which a section is given in Plate XXIII. Fig. 2, was used in the experiments described page 134 of our Journal, and is taken from the same report of Guyton. It is constructed of brick; its fire-place being 25 centimeters square ($9\frac{3}{4}$ inches) within, and 45 ($17\frac{1}{2}$ inches) in height. It is closed on one side by an iron cover, inclined about 25 degrees. The chimney, which is also of brick, is 25 centimeters square below, and one-fifth less at top, namely at an elevation of 13 decimeters (about 4 feet 3 inches) where it enters into a large chimney, about 15 metres (or near 5 feet) high, the excess of which is closed by a kind of door.

In this furnace the heat was raised to about 150 pyrometree degrees of Wedgwood.

VII.

On the Lamp for Tallow, and the Combustion of that Material. By Mr. WILLIAM CLOSE.

TO MR. NICHOLSON.

SIR,

SOON after the description of the lamp for burning tallow was sent to you, I made a little addition to the apparatus, which makes it more convenient.

For an appendix to the preceding detail, I send you an account of the new arrangement,

And am, Sir, your humble servant,

WILLIAM CLOSE.

Dalton, Jan. 25th, 1800.

The

The cup C, in Plate XVI. is made without the tube *g* in the inside, and the thumb screw is fixed near its bottom.

Another cup, about three quarters of an inch deep, is made to slide easily into the cup C: the brim of it is surrounded by a ring which projects outwardly, and to the bottom is foldered the tube *f*.

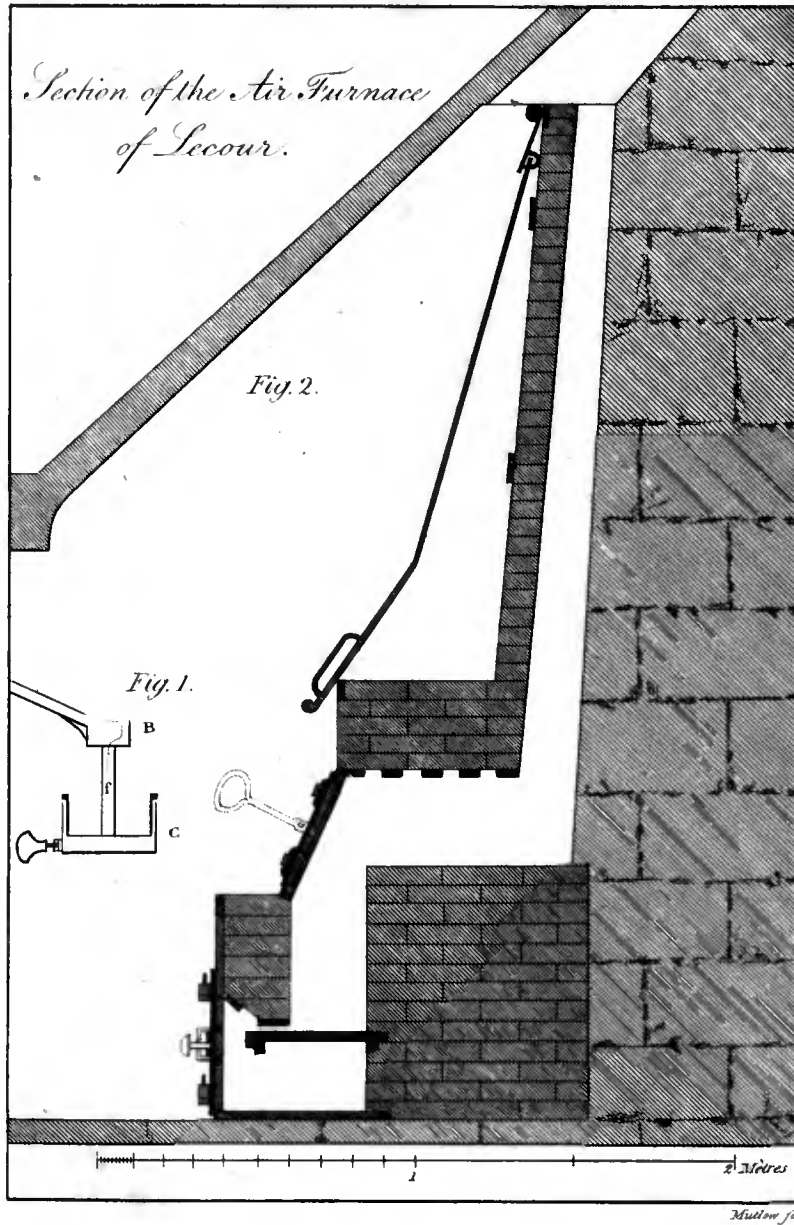
The cup B is foldered to the tube *f*, as before, and when detached from the rest of the apparatus, is accompanied by the additional cup, which serves it for a bottom or stand, and for the receptacle of the superfluous tallow.

Lastly, when B is put into its proper place, the ring which surrounds the interior cup rests upon the brim of the cup C, and prevents the bottom from touching the tallow which drops into C when B is removed.

Section of the cups B, C, Fig. 1, Plate XXIII. and the interior cup, intended only to illustrate the description. I have not yet seen your last Number: I have bought rendered hogs' lard at 6d. a pound lately at this place. It is now about 7d. Sheep's suet at 7d. Tallow sold last year at about 7s. 6d. a stone. Train oil is about 5d. a pint.

Farmers and country people consume a great quantity of impure tallow in *rust candles*. Tallow is also burnt in a common cylindrical drinking glass, in some places the wick is supported upon a piece of split stick, the lower end of which is thrust into some soft substance put into the bottom of the glass; a quantity of water is then poured in, and the remainder filled with melted tallow.

This is but an inconvenient method of burning tallow: the light is much dimmed by the sides of the glass covered with oil; it requires snuffing, which is not easy to effect; and the light is very unsteady from currents of air passing over the top of the glass.





I N D E X

TO THE

THIRD VOLUME.

A.

ABSTINENCE of the Chameleon, Serpents, Spiders, &c. 1
 Accum, Mr. his Samples of Beet-Sugar, 474
 Achard on the White Beet and its Sugar, 237, 288
 Acid of Borax, on its decomposition, 257, 292
 Acid, gallic, Experiments on its quantity in Vegetables, 392
 Acid, nitric, 325
 Acid, real, Kirwan on, 210
 Adams on Electricity, 476
 Air, proper regulation of for Combustion, 162
 Alderson, John, M.D. on the Geology of the Vicinity of Hull and Beverley, 285
 Antiphlogistian Theory, invented by Robert Hooke, 497
 Archimedes, invented the Areometer, 91
 Areometer, its Origin, 89, 91
 Asoph ul Dowlah, 183
 Azote, gaseous Oxide of, strange Effects produced by respiring it, 446. Manner of making it, 515
 Azote, Oxide of, 325

B.

Babington, Wm. M. D. 428
 Balance, Hydrostatic, new, 100. Ramfiden's, 101
 Bamboo, flex in, 57
 Bancroft, Dr. 482
 Banks, Sir J. 218
 Barometer, of very large Dimensions, as first Mover to a Clock, 126, 173
 Barytes, given to Animals, 40. Henry on, 169. Its Habitudes with Silix and Alumine, 123. With Animal Matters, 124
 Bath Waters, Examination of by Dr. Gibbes, 359, 403, 452
 Beam Compals, Description of a New, 99
 Beddoes, Dr. 39, 41, 43, 56, 95, 108, 137.
 On the strange Effects produced by Respiration of the Gaseous Oxide of Azote, 446
 Beech, Oil, Project for making, 229

Beet, Sugar from, 168
 Bernouilli, 13
 Berthoud, 187
 Biggin, Geo. Esq. on the Quantities of Gallic Acid and Tanning Principle contained in Plants, 392
 Bird, 99
 Bleaching, Improvement in, 252, 253
 Boiler for Steam Engines, on the proper Form of, 86
 Boilers, Rule for Constructing, 164. Description of one for a Brewhouse, 166
 Boissénade, C. 207
 Bones, on their Analysis, by Charles Hatchett, Esq. 500
 Borax, Acid of, its Decomposition by Crell, 257, 292
 Bridge Wooden, at Schaffhausen, 427
 Brougham, Mr. Examination of his Theory of Light, 222
 Bucci, M. 235
 Boyle, R. 209

C.

Calais Cliff seen double by Refraction, 144
 Cane, its Epidermis contains Silix, 56
 Caout-Chouc, 435, 534
 Carbon, its different States of Combination in Diamond, Plumbago, Charcoal, and other Bodies, 303
 Carradori, on the Air supposed to exist in Snow, 119
 Carrots, Sugar from, 168
 Calking Metallic Speculums, 494
 Cast Steel, formed by Means of the Diamond, 353
 Cavanilles, M. on the Earthquake in Peru, 377
 Cementation Furnace for Steel, 88
 Cevennes, Mountains in the, rendered Fertile, 295
 Chameleon, 1
 Chaptal, J. A. on the Oxides of Iron in dyeing Cotton, 92, 129. On the Fertilization of the Mountains of Cevennes, 295. On a Disease of the Chestnut, 331, 523
 Charcoal, its comparative Effect as Fuel, 166
 Charnes, Pajot des, on the Soldering of Glass, 522

Chestnut, 331
 Chezy, M. on the Plumb Line and Spirit Level, 396
 Chladni, Professor, on the Sonorous Vibrations of Glass Plates, 43, 185, 236
 Chromate of Iron, 314
 Clofe, Mr. Wm. his Lamp for burning Tallow, 363, 547
 Clayfield, Mr. W. on the Sulphate of Strontian found near Bristol, 36, 95, 137, 169
 Clothing of Animals, 163
 Clouet, Cit. Report on his Method of making Steel, 131
 Coak, its comparative Effect as Fuel, 166
 Coal, Pitcoal and Charcoal, their comparative Effect as Fuel, 166
 Coates, Mr. 56
 Coins, Method of Copying, by Rochon, 72
 Coke, see Coak
 Collier, Mr. his Description of the Furnace for making Steel, 88
 Collinson, Mr. 23, 73
 Combustion, spontaneous of Human Beings, 305
 Comestor, Peter, 207
 Contributions to Physical and Medical Knowledge, by Dr. Beddoes, 44
 Copal, Solutions of, 347
 Coquebert on the Fineness of Tin or Pewter, 356. On Poison for Arrows, 390
 Correa de Serra, J. on a submarine Forest on the Coast of England, 216
 Corle, John, Esq. on the Elephant, 181, 193, 247
 Corundum Stone, Mr. Greville on, 5
 Cotton, Use of the Oxides of Iron in Dyeing, 92, 129
 Coulomb, on the Strength of Men and Horses, 416
 Cox's Museum, perpetual Motion of, 126, 173
 Crayons, Fabrication of, 406
 Crell, Lorenz. de, on the Decomposition of the Acid of Borax, 257, 292
 Crooks, John, Patentee for Fish Soap, 112

D.

Dalrymple, Sir John, Account of his Soap of Fish, 108

Dance, Geo. Esq. his Survey of the Plan, &c. of Mr. Dodd, for improving London Bridge, 137
 D'Arcy on Vison, 33
 Davy, Mr. H. on the Respirability of Gaseous Oxide of Azote, 55, 93. On Silica in Vegetables, 56, 138. On the Formation of Respirable Nitrous Oxide, 515
 Daubenton, on Sheep and other Animals, 370
 Dancus Carota, 168
 Decandole, A. P. his Account of Defaussure, 178
 Deriabin, Mr. 37, 39
 Defaussure, Life of, 178
 Devahk, or Cubit of the Nilometer, 331
 Dew, 3
 Diamond, Guyton on its Combustion and Composition, 298. Its Application to Steel-making, 353
 Didot's Stereotype Publications, 44
 Dis, Mere of, Account and Analysis of its Water, 77
 Distillation by Cold, 524
 Dizé on Ether, 43
 Dodd, Mr. his Plan for admitting Ships above London Bridge, 135
 Doors, Register, 167. Regulating, 168
 Duhalde, 61
 Dyeing, colours, brightened by Oxymuriate, 159. Haussman on, 176

E.

Earth, whether its Rotation be regular, 30, 53
 Earths, their Mutual Actions, 419, 483
 Earthquake in Peru, 377
 Edwards, the Rev. John, on the Construction of Metallic Speculums, 490, 525
 Eisenrahm, 456
 Eisenmann, 457
 Elastic Gum, 435. Gum-Vine, 534. Uses of its Milk, 535
 Electrical Machine, Pocket, 4
 Electricity, Adams on, 476
 Elephant, curious Particulars respecting its History, 185, 247
 Ellicott, Andrew, his Observations on the Western Parts of Pennsylvania, 539
 Emerson, 51
 Eolus, Harp of, Account of its Effects, &c. 310
 Escapement, Crank by Mr. S. Goodrich, 342. By Soumille, 416
 Ether, Rectification of, 43. Distilled by Cold, 524. Sulphuric, 326
 Evelyn, Sir G. S. 32, 34, 97, 147, 200, 243
 Expansion Bars, compound, Project for a first Mover with, 127, 172

F.

Fabroni on the Action of Metals called Galvanic, 309
 Farm, the King's at Windfor, 422, 429
 Fecundation of Vegetables, Knight on, 458, 519

Fell, Mr. J. his Pocket Electrical Machine, 4
 Fertilization of the Mountains of the Cevennes, 295
 Figures, Resonant of Chladni, 185
 Fire-Places, Construction of, 162, & seq.
 Fishes, their Power to deprive Water of Oxygen, 119
 Floating Bodies, their Motions, 269
 Fluids, lateral Motions of, 13, 59. Sprouting, 20. Resistance of, Experiments on, by the Rev. S. Vince, 506
 Fly, the Insect, Vibrations of its Wing counted, 33
 Fordyce, Dr. Geo. his Pendulum, 30, 98
 Forsyth, Rev. Alex. John, on the Oxygenated Muriatic Acid, 158
 Fourcroy, Cit. his Error respecting Mayow, 108
 Forest, Submarine on the Coast of England, 216
 Franklin, Dr. his Invention for Printing, 63
 Fuel, Generation of Heat by its Combustion, 161. Great Savings of, 162
 Furnace, of Professor Pott, 344. Of Cit. Lecour, 547
 Furst, the Printer, 62

G.

Gallic Acid, its Quantity in various Vegetables, 392
 Galvani, 235
 Galvanism, Fabroni on, 309
 Gases, found through a Pipe, 43
 Gazometer, of Seguin, 405
 Ged, William, his Stereotype Printing, 44, 62
 Gibbs, Dr. 41, 95, 137. On the Bath Waters, 359, 403, 452
 Glass Plates, Chladni on their Vibrations, 185
 Glais, Soldering of, 522
 Goodrich, Simon, his Crank Escapement for Clocks, 342, 416
 Gough, Mr. on the Nutrition of Succulent Vegetables, 1
 Graham, Geo. his Length of the Seconds' Pendulum, 33. His Standard, 200
 Gren, Mr. some Account of, 94
 Greville on Corundum, 5
 Gridiron, Pendulum, 205
 Grubenman, Ulric, 427
 Gunpowder, Proof by Regnier, 198
 Guyton, Cit. Experiments on the Combustion and Composition of the Diamond, 298. On the Formation of Steel by Means of this Substance, 353. On the Affinities exerted by the Earths on each other, 419, 483

H.

Harris, Mr. his Beam, 106
 Harris, Mr. Stephen, 182
 Harp of Eolus, 310
 Hassenzratz, 89
 Hatchett, Charles, Esq. Analysis of the

Water of the Mere of Dis, 77. On Shell and Bone, 500, 529
 Hat-making, Report on the Process of, 23
 Hat-making, 73
 Hatton, Mr. his Pendulum, 30, 98
 Haussman on Dyeing, 176
 Haüy, 9
 Heat, Experiments by Count Rumford on its supposed Weight, 381. Generation of, 161. Radiant, 162. To confine, 163
 Henry, Mr. W. 41. On Sugar, on Barytes and Strontian, 168. Analysis of an Ore of Iron, 454
 Hernstadt on the Production of Sugar from indigenous Plants, 333, 410
 Herschel, Dr. 30, 52
 Higgins, Mr. William, his Improvement of the Chemical Process of Bleaching, 253
 Hill, Aaron, on Beech Oil, and other Inventions, 229
 Hooke, Rob. his early Development of the Antiphlogistic Theory, 497. His very curious Lamp, 468
 Hope, Dr. 170
 Horses, their Strength estimated, 416
 Howard, Mr. Ed. his fulminating Mercury, 236
 Hull and Beverley, Vicinity of, its Geology, 285
 Huyheus, 29
 Hydrotatic Lamp of Mr. Peter Keir, 467
 Hygrometer, New, by Mr. John Leslie, 461, 518
 Hypacia, invented the Areometer, 89

I.

Indigo, curious Experiments on, 478
 Indigo, Proust on, 325
 Inflammation of Oils by Nitric Acid, 327
 Ink, Improvement of, 158
 Institution for diffusing Mechanical Knowledge, Constitution of, 45, 283
 Institute of France, 329
 Integers of Measure, 157
 Iron, Use of its Oxides in dyeing Cotton, 92. Chromate of, 314. Combined with Glass, 132, with Carbon, 133, with its own Oxide, 133. Converted into Steel, by Means of the Diamond, 353
 Iron, native of Peru, its singular Clearness of Surface, 374
 Jacquin, Prof. on Sugar from Carrots, &c. 168
 Jameison, Mr. Robert, his Experiments on Saponaceous Compounds, 108

K.

Kaolin Analyzed, 265
 Keith, Alex. Esq. his Thermometer, 268
 Kelly on Spherics, 140
 Kent, Nath. Esq. on his Majesty's Farm at Windfor, 422, 429
 Kettle for boiling Inflammable Fluids, 514
 Kirwan, Mr. 166, 170. On the Proportion of Acid in Salts, 210
 Klaproth's Analysis of the Spinel, 349
 Knight,

Knight, A. Esq. on the Fecundation of Vegetables, 458, 519

L.

Lair, Citizen, on the spontaneous Combustion of Human Individuals, 305
Lake Erie, Miscellaneous Observations relative to, 539
Lamark, Cit. on the Influence of the Moon on the Atmosphere of the Earth, 488
Lamp, Hydrostatic, of Mr. P. Keir, 467. With a Float, by Rob. Hooke, 468
Lamp for burning Tallow, 365
Le Beck, Henry J. His Account of the Pearl Fishery in the Gulph of Manar, 542
Leibnitz, 27
Leroi, Alphonfus, on the internal Use of Phosphorus, 84
Leslie, Mr. John, his Photometer and Hygrometer, 461, 518
Level, Spirit, grinding of, 396
Light, on its Reflexibility, 222
Lightning, singular Accident by, 432
Lime, on the different Sorts used in Agriculture, 440. Sulphuret, of its Use in Bleaching, 253
Lincolnshire, submarine Forest on its Coast, 216
Linsed Oil, to clear it from Mucilage, 345
Lint, mechanical Fabrication of, 22
Logarithms, Callet's Stereotype Tables of, 44
Lomet, on Crayons, 406
London-Bridge, Dodd's Project for improving it by an Arch of 300 Feet span, 135

M.

Mabillon, Father, 207
Machine for engraving Copper-Plates, by Rochon, 65
Machines for Hat-making on, 28
Macie, Mr. 57
Manures, Middleton on, 340
Mars, Opposition of, 331
Measure, invariable, 29
Measure of the Meridian of France, Report on, 316, 365
Men, their Strength estimated, 416
Mercury, fulminating, 236
Mercury in Sea Salt, 375
Metals, their mutual Action, 309
Metrical System of France, 316, 365
Middleton, John, Esq. on Manures, 340
Mineralogical Society, 138
Mineralogy, by Dr. Babington, 428
Moon, its Influence on the Atmosphere of the Earth, 488
Monge on the Apparent Attraction and Repulsion of Floating Bodies, 269
Montucla, 380
More, Sam. on the Power of Horses, 136
Motions, perpetual, 126, 172
Mountains in the Cevennes, Fertilization of, 295

Munich, Kitchen and Brewery at, 161, 166
Muriate of Soda, its Decomposition, 470

N.

Newton, Sir Isaac, 222
Neuheusel Brewhouse, its Boiler described, 166
Nitric Acid, 325
Nitrite of Potash, 326
Nitrous Oxide, 515
Nomenclature, Chemical, Dr. Pearson on, 476
Notcutt, Mr. 41, 95, 137
Nutrition of succulent Vegetables, 1
N. W. on Barometrical and Thermometrical Perpetual Motions, 126, 172. His New Gridiron Pendulum, 205

O.

Odeon at Paris, Remarks on its Conflagration, 306
Oils, changed by Oxymuriate of Potash, 160
Oils, Inflammation of by Nitric Acid, 327
Organ Pipes, 16
Oxide of Azote Gaseous, its Respirability, 55, 93
Oxygen, erroneously supposed to Exist in Snow, 119
Oxymuriate of carbonated Potash, used to brighten Colours, 158

P.

Palaemon, Rhemnius Fannius, de Ponderibus et Mensuris, 20
Paleography, 283
Pearl Fishery, Account of, 542
Pearson, Dr. on Chemical Nomenclature, 476
Pearson, Rev. Wm. on the Philosophical Uses of a common Watch, 49, 189
Peas, curious Experiments on their Improvement, 458, 519
Pendulum, invariable Measure from, 29. Method of obviating the Resistance of the Air against, 523. New Gridiron, 205
Pepys, Mr. Junr. 138
Pelchier, Dr. 168, 171
Petrie, Wm. Esq. on a singular Accident by Lightning, 432
Petuntze analyzed, 265
Pewter, on its Fineness, 356
Phosphorus, its powerful Effects as a Medicine, 84
Photometer, New, by Mr. John Leslie, 461, 518
Pitcoal, its comparative Effect as Fuel, 166
Planetary System of the Particles of Bodies, 234
Plates of Glass, Chladni on their Vibration, 185
Plumb Line and Spirit Level, 396
Poison, anciently used for Arrows, 390
Pottery, Vauquelin on, 264

Pott, Professor, Description of his Furnace, 344
Powder Proof of Regnier, 198
Prevost, Professor, on Light, 222
Printing, 61. Stereotype, 44, 62, 71
Pront. A. on Typography, 379
Proust, on Nitric Acid---Oxide of Azote---Indigo---Nitrite of Potash---Residue of Ether---Inflammation of Oils by Nitric Acid---Ammoniac---Putrid Water at Sea, 325. On Native Iron of Peru, 374. Mercury in Sea Salt, 375
Pyrites, their daily Formation, 84. Artificial, 426
Pyrometer, picce of Wedgwood analyzed, 265

R.

Ramsden, some Particulars of a Balance made by him, 101
Reflexibility, 222
Refraction, Horizontal, its Uncertainty, 140. Unusual Effects of explained by Vince, 142
Register Doors, 167
Regnier, his Powder Proof, Dynamometer, Safeguard for Musquetry, and Electrical Machine, 198
Resonant Figures of Chladni, 185
Respiration of the gaseous Oxide of Azote, strange Effects produced by, 446
Richardson, Mr. 41, 95, 137
Rittenhouse, his Improvement of the Pendulum, 523
Rochon on the Typographic Art, 61
Roederer, Cit. 283
Roman Foot, 201
Romé del'Isle, 9
Rotation of the Earth, whether regular, 30, 53
Roxburgh, Wm. M.D. on the Elastic Gum Vine, 435
Royal Society, Transactions of, 186
Rubies, their specific Gravities, 11
Rumford, Count, on the Management of Fire, 161. On a Lamp for Tallow, 365. On the Weight ascribed to Heat, 381
Rankelrube, 237, 288

S.

Sage, B. G. on the Conflagration of the Odeon, 306
Salverte, Eusebe, on the Invention of the Areometer, 89
Sandys, Capt. 184
Sapplies, their specific Gravity, 11
Schaffhausen Bridge, 427
Sea Salt, Mercury in, 375
Sedum Acre, Nutrition of, 2
Seguin, his Gazometer, 405
Sempervivum Tectorum, its Nutrition, 1, 3
Sleep and domestic Animals, Experiments on, by Daubenton, 370
Sheldrake, Mr. T. on the Purification of Linsed Oil, &c. 345
Shells, their Analysis, by Charles Hatchett, Esq. 500, 529

Ships

Ships seen Double, Treble, and Inverted, 142
 Short-hand, its Velocity and Value estimated, 380
 Silex in Vegetables, 56
 Smith, Tho. P. his Kettle for boiling inflammable Fluids, 511
 Snow contains no Air, 119
 Soap improved by Oxymuriate of Pot-ash, 160
 Soap, short History of, 109; general Processes for making, 110; Contents of the waste Lees 111; of Wool, 117; of Glue, 118; of Peat, 118
 Society, Mineralogical, 138
 Sound, Phenomena of, Venturi on, 16.
 Produced by Gales, 43
 Specific Gravities of Stones, 11
 Speculums of Telescopes, their Construction, by the Rev. J. Edwards, 490
 Spherics, Kelly, on, 140
 Spider, on the Web of, 369
 Spinel, Klaproth's Analysis of, 319
 Spirit Level, Chezy, on the Fabrication of, 396
 Standard of Weight and Measure, 97, 147, 200, 203, 213
 Steam, Apparatus for saving, 282
 Steam Engines, Boiler for, its best Figure, 86
 Steel, Clouet's Process for making it by the Carbon of Lime-stone, 131
 Steel-making, Furnace for, 88
 Stereotype printing, 44
 Stocking Loom, Invention of, 230
 Stop Watches, Imposition in making, 54
 Strength of Men and Horses valued, 416
 Strontian, its Habitudes with Silex and Alumine, 122
 Strontites, Henry on, 171
 Strontian of Gloucestershire, 36, 95
 Submarine Forest, 216
 Succulent Vegetables, Nutrition of, 1
 Sugar from Carrots, Beets, &c. 168. In Starch Water, 169. From the White Beet, 237, 288. Its Products ascertained by Hermstadt from the Sugar Maple, the Indian corn, Siberian Bear's Breech, Juice of Grapes, Juice of Birch, 333.

Beet Root, 410. Turnep, 413. Cole-wort, 413. Skirret, 414. Parsneps, 414.
 Account of Samples from Beet, 474
 Sulphuret of Lime, substituted for Pot-ash in Bleaching, 253
 Swiftmess of Writing, 380
 Sytem, Metrical, of France, 316, 365

T.

Tables of specific Gravities of Acids, by Kirwan, 213. Of the Composition of Salts, 214
 Tallow, very great Importation of, 112.
 Burned in a Lamp, 365
 Tassaert on the Chromate of Iron, 314
 Tassie, Mr. 72
 Tanning Principle, its Quantity in various Plants ascertained, 392
 Telescope, on the Discovery of, 207
 Tennant, Smithson, Esq. on the different Sorts of Lime used in Agriculture, 410
 Theo, 89
 Theory, Antiphlogistian, its first Invention by Rob. Hooke, 497
 Thermometer, Register, of Keith, 268
 Time Pieces, 187
 Tin, dying by the Oxides of, 173
 Tin or pewter, on its Fineness, 356
 Tobin, Mr. 36
 Topazes, their specific Gravity, 11
 Transactions, Philosophical, 186
 Troughton, 99
 Troughton, Mr. Edw. 233
 Turgot, M. 64
 Typographic Art, 61

V.

Vanhelmont, 209
 Van Swinden, his Report on the Measure of the Meridian of France, &c. 316, 365
 Vauquelin on Strontian and Barytes considered as Alkalis, 122. On the Decomposition of Muriate of Soda, 470. On Pottery, 264

Vegetables, Succulent; Nutrition of, 1.
 Silex, in the Epidermis of, 56. Fecundation of, 458, 519
 Venturi, J. B. his Researches on the lateral motion of Fluids, 13, 59
 Vince, the Rev. S. on certain strange Effects of Refraction, 140. On the Resistance of Bodies moving in Fluids, 506

U.

Urceola Elastica, 435

W.

Walker, Adam, his Lectures, 332
 Watch, common, its Philosophical Uses, 49, 189
 — Stop, Deception in, 54
 Waters of Bath, Dr. Gibbes on, 359, 403, 452
 Water, the Fuel required to heat it, 165
 — Weight of a cubic Inch of, determined, 156
 Watson, Dr. on Vision, 33
 Wedgwood's Pyrometer Piece, its Analysis, 265
 Welter on the Treatment of Bodies with Nitrous Acid, 277
 White Beet, culture of, 237, 288
 Whitehurst, his Chronometer to subdivide Seconds, 50. His Pendulum, 30, 98, 156
 Windsor, his Majesty's Farm at, 422, 429
 Wiseman, Mr. Benj. on the Mere of Difs, &c. 77
 Wistar, Mr. C. on Distillation by Cold, 524
 Withering, 2
 Wren, Sir Chr, 29

Y.

Young, Matthew, E.D. on the Harp of Eolus, 310





